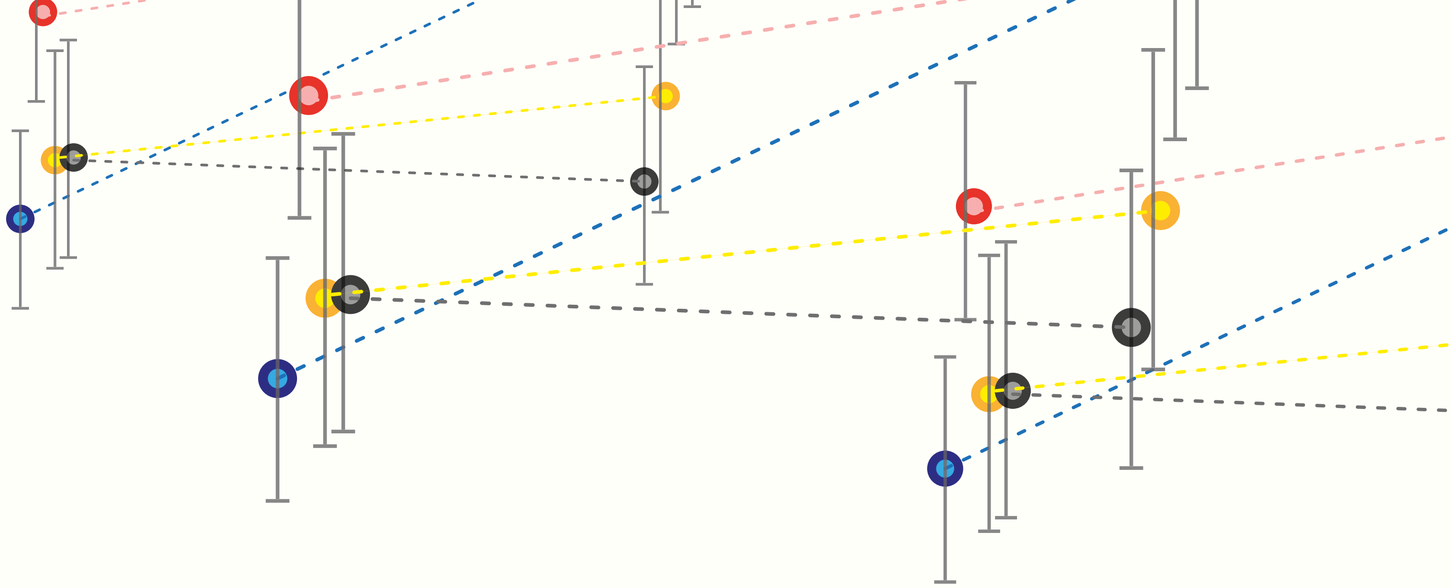
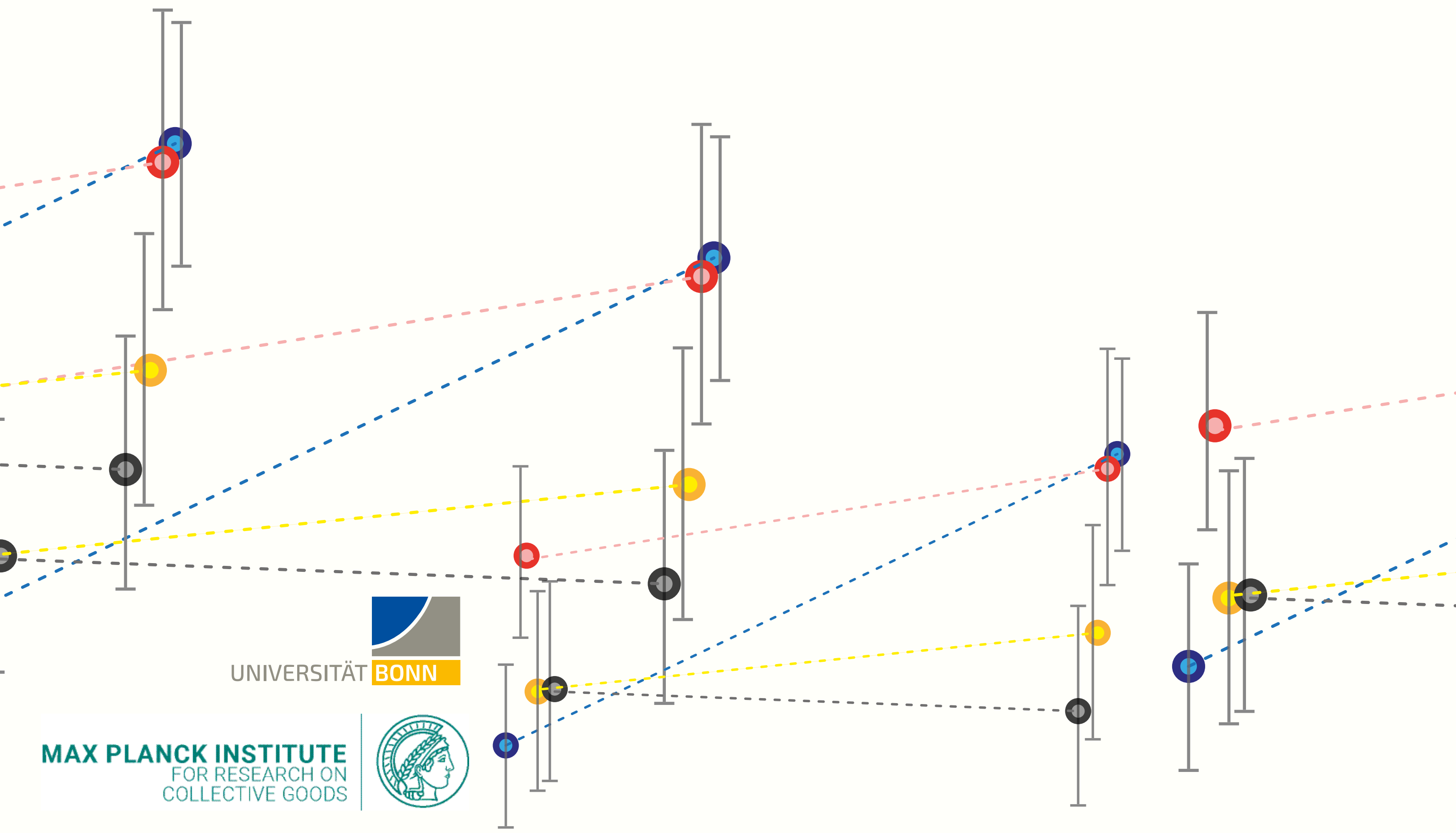


Stephan Tontrup

Behavioral-Self-Management

A Model Case for Fundamental Empirical Legal Research



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FOR RESEARCH ON
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Behavioral-Self-Management
A Model Case for Fundamental Empirical Legal Research

Dissertation zur Erlangung des Grades eines
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Deutscher Teil

I. Einführung

Vertragsrechts soll Pareto-Verbesserungen ermöglichen, also dafür sorgen, dass ein Gut übertragen werden kann, wenn ein Käufer es höher bewertet als der Berechtigte und beide dem Transfer zustimmen. Wie gut das gelingt, hängt vor allem von der Struktur des Vertragsrechts ab, zum Beispiel von den Transaktionskosten, die es induziert. Formvorschriften etwa steigern Transaktionskosten, dispositives Gesetzesrecht soll sie senken. Indem es Verträge formfrei zustande kommen lässt und sie mit dispositiven Regeln ergänzt oder Fehler heilt, um ihre Gültigkeit zu sichern, verhindert das Vertragsrecht, dass Pareto-Verbesserungen durch die Transaktionskosten aufgezehrt werden.

Für die Leichtigkeit des Rechtsverkehrs gibt es aber nicht nur prozesshafte, sondern auch viele behaviorale Voraussetzungen und Hindernisse. Eines davon untersuche ich hier: Verlustaversion und Besitzeffekte. Wenn Menschen die Verfügungsgewalt über Güter und Rechte innehaben, schreiben sie ihnen einen vielfach höheren Wert zu, verglichen mit dem, was sie bereit sind zu geben, um die gleichen Güter und Rechte zu erlangen. Die Differenz resultiert daraus, dass Menschen Nutzen nicht linear als ein mehr oder weniger, sondern relativ zu Referenzpunkten erfahren. Ein Weniger als der Referenzpunkt ist dann ein Verlust, ein Mehr ein Gewinn. Verluste wiegen schwerer und können deshalb nicht durch symmetrische Gewinne ausgeglichen werden. Der Besitzstatus ist ein solcher Referenzpunkt; die Verfügungsgewalt über ein Gut abzugeben wird daraufhin als Verlust erfahren, der Verkaufserlös als Gewinn. Das könnte bedeuten, dass objektive Pareto-Verbesserungen häufig gar nicht erkannt werden. Unter einer objektiven Pareto-Verbesserung will ich verstehen, dass ein Käufer im Vergleich zum Verkäufer das Gut sowohl dann höher bewertet, wenn er nicht besitzt, als auch dann, wenn er die Verfügungsgewalt hat. Trotzdem kommt die Transaktion nicht zustande, wenn der Verkäufer aufgrund seiner Verlustaversion einen höheren Preis von dem Käufer haben will, als dieser zahlen will. Das könnte die Effizienz des Rechtsverkehrs gravierend beeinträchtigen und zu massiven Wohlfahrtsverlusten führen.

Das Recht kann Besitzeffekte durch die Fassung seiner Regeln aber erheblich beeinflussen. So zeigt die Evidenz zum Beispiel, je stärker eine Rechtsposition ist, desto stärker ist auch der Besitzeffekt an ihr. Das deutsche Recht sieht etwa eine Erfüllung in natura vor. Der Besitzeffekt kann dann dazu führen, dass Vertragspartner auch dann auf Erfüllung bestehen, wenn sie in ihrer Lage gar nicht effizient ist (Depoorter & Tontrup 2013). Sollte deshalb wie im Common Law nur ein Recht auf Schadensersatz bestehen? Sollte auf dispositives Recht verzichtet werden, weil die, die es begünstigt, das Recht als Status quo ansehen und es deshalb aufgrund ihrer Verlustaversion nicht aufgeben wollen, selbst wenn diese Regeln den Bedürfnissen der Parteien gar nicht entsprechen (Korobkin 1998). Zur Leichtigkeit des Rechtsverkehrs gehört auch die Durchsetzung eines vertraglichen Anspruchs, dass also der aus dem Vertrag Berechtigte die Herausgabe des Geschuldeten verlangen kann. Jurys scheinen aber die Verlustaversion des Besitzers zu übernehmen und geben viel eher dem Besitzer als dem Kläger statt. Sollte das Beweismaß deshalb geändert werden (Zamir und Ritov 2013)? Der Vertrag, könnte auch durch andere Formen des Austausches ersetzt werden, die keine Zustimmung des Berechtigten verlangen: So könnten Dritte in einem Liability-Regime ein Recht zur Aneignung von Gütern bei Entschädigung erhalten, um Besitzeffekte zu schwächen (Buccafusco & Sprigman 2011). Eigentumsrechte könnten auch hoheitlich umverteilt werden, damit sie direkt zu denjenigen gelangen, die sie höher bewerten. Sollte eine Eminent Domain also auch für private (infrastrukturrelevante) Projekte zugelassen werden wie zum Beispiel im Kelo-Fall entschieden (Nadler & Diamond 2008)?

Es geht aber nicht nur um den Austausch von Gütern. Besitzeffekte könnten auch zu höheren Schadensersatzverpflichtungen führen: Bei deliktischen Handlungen wollen viele als "Full-Compensation" die subjektive Wertschätzung des Verletzten für sein Rechtsgut ersetzen (Arlen 1985). Evidenz zeigt, dass wenn die Jury die Perspektive des Geschädigten übernimmt, wozu sie die richterlichen Entscheidungsanleitungen auffordern, auch sie den Besitzeffekt des Geschädigten zeigt. Sollte sie sich deshalb, anstatt nach eigenem Ermessen entscheidet, an fixen Ersatzzahlungen orientieren? Auch bei einer Eminent-Domain könnte der Besitzeffekt je nach Maßstab der Entschädigung zu einem Ausgleich deutlich über Marktpreisen führen. Sollten auch hier Marktpreise mit fixem subjektivem Premium ersetzt werden (Lee 2013)?

Die Rechtswissenschaft hat auf diese Fragen mit einer Flut an Publikationen reagiert und hat – wie oben angedeutet - ein ganzes Bündel rechtspolitischer und dogmatischer Reaktionen vorgeschlagen. Korobkin (2013, 1998) hat über 1.600 Artikel allein in den USA gezählt, die sich mit den rechtlichen Konsequenzen von Besitzeffekten auseinandersetzen; weltweit sei die doppelte Anzahl erschienen. Ein großer Teil der rechtlichen Literatur nimmt an, dass Besitzeffekte die effiziente Allokation vieler wichtiger Güter gefährden, am häufigsten diskutiert werden geistiges Eigentum und Grundbesitz.

Die juristischen Vorschläge reagieren dabei auf eine sehr robuste Evidenz: Hunderte von psychologischen und ökonomischen Experimenten belegen die Verhaltenseffekte von Verlustaversion und Besitzeffekten. Die Evidenz bezieht sich auf einfache Konsumgüter genauso wie auf riskante Anlagegüter. Vor allem verleiht aber auch die gewaltige Effektgröße dem Bias Gewicht: Studien variieren zwar in ihren Angaben bzgl. verschiedener Güter, es lässt sich aber verallgemeinern, dass die abgefragte Wertschätzung der Berechtigten die Zahlungsbereitschaft der Käufer regelmäßig zumindest um das Doppelte übersteigt. Geht es um Besitzeffekte an Rechten, sind die Unterschiede sogar noch dramatischer.

Ich will hier zeigen, dass diese Evidenz die rechtliche Fragestellung, ob und inwieweit Besitzeffekte den Austausch von Gütern und Rechten beeinträchtigen, nicht so klar beantwortet, wie es scheint. Denn viele dieser Experimente testen Theorievoraussetzungen; dabei reduzieren sie ihr experimentelles Design auf das für die Beantwortung ihrer Forschungsfrage Notwendige. Um auf Grundlage der Prospekt-Theorie zu testen, ob der Besitzstatus als Referenzpunkt Verlustaversion auslöst und Entscheidungen beeinflusst, genügt etwa das folgende minimale Tauschdesign: Eine Hälfte der Probanden erhält Gut A und ihnen wird Gut B zum Tausch angeboten. Die andere Hälfte erhält B und sie kann es gegen A tauschen. Ein Besitzeffekt liegt vor, wenn die Häufigkeit dafür, dass die Güter getauscht werden, abnimmt, wenn die Probanden sie im Besitz haben.[1] Dabei ist es offensichtlich im besten Interesse der Probanden, ihre wahren Präferenzen zu offenbaren, also nur zu überlegen, welches der beiden Güter sie mehr schätzen und dementsprechend zu entscheiden.

Bei einem realen Güteraustausch stehen aber natürlich andere Motivationen im Vordergrund, als nur die eigene Wertschätzung richtig einzuschätzen. Marktakteure wollen Geld verdienen und sie haben strategische Anreize ihre Preissetzung an der potentiellen Zahlungsbereitschaft der Käufer auszurichten und nicht an ihrer persönlichen Wertschätzung. Auf Grundlage der Prospekt-Theorie spielt es aber keine Rolle, ob die Akteure traden wollen und strategische Anreize haben oder nicht. Aus ihrer Sicht hängt der Bias nur davon ab, dass, wenn der Besitzstatus Referenzpunkt ist, der Berechtigte das Abgeben eines Guts aus seiner Verfügungsgewalt als Verlust erfährt. Solange man also auf Grundlage der Prospect-Theorie annimmt, dass markttypische Anreize Verlustaversion nicht beeinflussen, ist es folgerichtig, sie auch in Experimenten nicht zu berücksichtigen. Strategische Anreize trügen dann nichts zur Forschungsfrage bei, könnten aber die Interpretation der Ergebnisse erschweren. Denn wenn die Probanden Anreize haben, weniger zu bieten und mehr zu verlangen, dann ist nicht klar, ob die Preisdifferenz zwischen Verkaufspreis und Zahlungsbereitschaft durch Besitzeffekte oder Anreize verursacht ist.[2] Die vorhandene Evidenz für Besitzeffekte beruht deshalb praktisch ausschließlich auf

[1] Ähnlich weit verbreitet ist das Preisdifferenzdesign: Im ersten Treatment erhalten die Probanden Besitz und Eigentum an einer Tasse. Dann entscheiden sie zu welchem Preis sie sie Tasse verkaufen wollen. In einem zweiten Treatment erhalten sie eine finanzielle Ausstattung und entscheiden dann, für welchen Preis sie das Gut kaufen würden. Ist der Verkaufspreis höher als die Zahlungsbereitschaft, liegt ein Besitzeffekt vor.

[2] Zur Lösung dieses methodischen Problems habe ich neues Design entwickelt.

der Abfrage wahrer Wertschätzungen und nicht auf strategischen Entscheidungen, wie sie in realen Märkten getroffen werden.

Aus der Fokustheorie leitet sich dagegen die These ab, dass strategisches Entscheiden Besitzeffekte reduzieren kann: Verlustaversion entsteht daraus, dass der Referenzpunkt kognitive Aufmerksamkeit steuert; liegt der kognitive Fokus auf dem Besitzstatus, dann bekommt das Gut mehr Aufmerksamkeit und sein Verlust wiegt in der Entscheidung schwerer. Im Ergebnis wird das Gut dann überbewertet.[3] Der Referenzpunkt ist aber nicht immer gleich salient, sondern konkurriert um kognitive Aufmerksamkeit. Offensichtlich ist das, wenn in einer Entscheidungssituation mehrere Referenzpunkte präsent sind. Es ist aber auch der Fall, wenn eine Aufgabenstellung mit ihren Erfordernissen den Fokus der Aufmerksamkeit in eine andere Richtung steuert. Viele psychologische Experimente zeigen, dass Probanden sogar Offensichtliches nicht wahrnehmen, wenn die Information für die ihnen gestellte Aufgabe nicht wichtig ist.[4] Ein Referenzpunkt steht also in einem Kontext, in dem er mehr oder weniger salient ist, je nachdem, wie viel Aufmerksamkeit andere Faktoren auf sich ziehen. Wenn Probanden nun versuchen, für ihr Gut einen möglichst guten Preis zu erhalten, dann verliert der Besitzstatus an Salienz, weil die Aufgabe ihren Fokus auf Präferenzen und Zahlungsbereitschaften möglicher Käufer lenkt. Das Gut erhält weniger Aufmerksamkeit und der Bias nimmt ab. Weil die Aufgabe, die Entscheider zu lösen haben, also ganz wesentlich bestimmt, worauf sich ihre Aufmerksamkeit bezieht, erwarte ich, dass eine markttypische Aufgabe mit entsprechenden Anreizen Besitzeffekte verdrängen wird: Die Bewertungsaufgabe, wie sie etwa das von vielen Experimenten verwendete Tauschdesign den Probanden stellt, fokussiert Probanden auf Besitz und Gut und forciert damit den Bias, während eine Tradingaufgabe, wenn die Probanden also durch strategisches Verhalten mehr verdienen können, den Fokus auf andere Faktoren lenkt, und damit den Besitzeffekt reduziert. Für die rechtliche Frage nach der Intensität einer möglichen Beeinträchtigung des Rechtsverkehrs ist das offensichtlich von großer Bedeutung.

[3] Johnson, Häubl und Keinan (2007) vermuten sogar, dass der Bias aus einem sequentiellen kognitiven Prozess hervorgeht: Sie nehmen an, dass sich durch den Besitzstatus die Aufmerksamkeit vor allem auf die Attribute der Güter bezieht. Individuen prozessieren deshalb zuerst die positiven Eigenschaften des Gutes und erst dann die Vorteile, die sie durch einen Tausch erzielen können. Die Eigenschaften des Guts werden so besser memoriert und deshalb in einer Verkaufsentscheidung stärker gewichtet, was im Ergebnis zu einer verzerrten Überbewertung des Guts führt.

[4] Ein Beispiel ist der Artikel von Dreibach, G. & Haier, H. (2009). *How Task Representations Guide Attention: Further Evidence for the Shielding Function of Task*. Journal of Experimental Psychology, 35(2) 477– 486.

A. Besitzeffekte und Trading

Meine erste Studie testet also, ob Besitzeffekte und Verlustaversion eliminiert werden, wenn Individuen eine Trading- und keine Bewertungsaufgabe zu lösen haben. Dafür habe ich ein neues experimentelles Design entwickelt, das es mir erlaubt, den Probanden strategische Anreize zu präsentieren und trotzdem ihren Bias sauber zu isolieren (siehe unten im Detail). Im Ergebnis verschwindet der Bias weitgehend.

Zwar haben schon frühere Arbeiten über den Einfluss von Märkten auf Besitzeffekte und Verlustaversion berichtet; sie nehmen aber an, dass Teilnehmer zunächst Erfahrungen mit Marktentscheidungen und den Gütern, mit denen sie handeln, sammeln müssen, damit sich ihr Bias reduziert. In ihren Studien verwenden sie Bewertungsaufgaben, fokussieren ihre Probanden also auf deren Besitzstatus; diesen durch die Aufgabenstellung induzierten Fokus zu überwinden, gelingt offenbar nur sehr erfahrenen Händlern. List etwa (2003) gibt in einer Feldstudie erfahrenen und unerfahrenen Händlern eine Autogrammkarte eines Sportlers und bietet ihnen an, die Karte gegen eine zweite zu tauschen. Nur professionelle, auf Autogrammkarten spezialisierte Händler mit mehr als zehnjähriger Markterfahrung behielten oder tauschten die Karten dabei unabhängig davon, ob sie die Autogrammkarten im Besitz hatten oder nicht. List leitet aus diesem Ergebnis ab, Marktteilnehmer müssten erst die Eigenschaften der Güter, die sie anbieten und ihre eigenen Präferenzen für diese Güter verstehen lernen. Engelmann und Holland (2010) nehmen an, die Marktteilnehmer müssten sich erst an Prozedere und Unsicherheiten des Marktes gewöhnen, um ihren Besitzeffekt zu überwinden. Auch sie lassen ihre Versuchsteilnehmer zwei Güter tauschen. Sie gehen davon aus, dass Individuen ihre Unsicherheit und damit ihren Bias häufig nicht überwinden, und stattdessen Marktaktivitäten meiden.

Meine Studie präsentiert den Probanden dagegen ein-perioden Design, das es ihnen nicht ermöglicht, Erfahrung zu sammeln. Der Besitzeffekt wird also nicht durch Erfahrung, sondern durch eine markttypische Aufgabe und einen automatischen kognitiven Prozess neutralisiert. Da sehr viele Rechtsgeschäfte zumindest zum Teil auch strategischen Charakter haben, sind meine Ergebnisse auf eine Vielzahl von Transaktionen anwendbar. Zudem betrifft der Debiasing-Mechanismus nicht nur erfahrene professionelle Trader, wie List annimmt, sondern hängt nur von der gestellten markttypischen Aufgabe ab. Der Einfluss von Besitzeffekten auf die Effizienz des Rechtsverkehrs könnte damit deutlich kleiner sein, als bisher in der rechtlichen Literatur vermutet. Die Bedeutung dieses Ergebnisses für die rechtspolitischen und dogmatischen Interventionen, die die Auswirkungen der Besitzeffekte auf den Rechtsverkehr korrigieren sollen, analysieren wir unten im Detail.

B. Debiasing durch Vertretung und Gremienentscheidungen

Neben diesem ersten, analysiere ich in meiner Arbeit weitere Debiasing-Mechanismen, die sich auf Rechtsregeln und rechtliche Handlungs-möglichkeiten beziehen. So zeige ich in meiner zweiten Studie, dass Besitzeffekte auch dann eliminiert werden, wenn Probanden ihre Verantwortlichkeit für den Verkauf eines Guts mit einem Vertreter oder mit einer Gruppe teilen können.

Individuen empfinden Reue über den Eintritt nachteiliger Weltzustände, insoweit sie sich für deren Eintritt eine Verantwortlichkeit zuschreiben, also glauben, mit einer alternativen Handlung hätten sie einen negativen Ausgang vermeiden können. Sie antizipieren diese Reue aber auch, und damit kann sie ihre Entscheidungen beeinflussen. Bei einer Transaktion einer Aktie etwa, antizipiert der Entscheider, dass er es bereuen wird, die Aktie nicht behalten zu haben, wenn sie im Wert steigen sollte, umgekehrt antizipiert er aber auch, dass er es bereuen wird, sie nicht verkauft zu haben, wenn die Aktie im Preis fällt. Solange sich die erwarteten emotionalen Kosten für die Handlungsalternativen die Waage halten, beeinträchtigen sie seine Entscheidung nicht. Der Bias entsteht erst durch den Referenzpunkt, der den Aufmerksamkeitsfokus auf den Besitzstatus legt. Dadurch erhält die Möglichkeit, dass die Aktie nach Verkauf steigen könnte, größeres Gewicht und es werden höhere emotionale Kosten für diesen Fall erwartet. Die Aktie wird also behalten.

Wenn Individuen einen Vertreter anweisen oder die Entscheidung mit anderen gemeinsam treffen, etwa in einem Unternehmen, dann reduziert sich die Reue, die sie empfinden, weil sie sich eine geringere Verantwortlichkeit für den Entscheidungsausgang zuschreiben. In meiner Studie zeigt sich, dass Probanden, die ein Lotterielos durch einen Vertreter oder qua Votum einer Gruppe verkaufen, kaum emotionale Kosten über den Verkauf antizipieren. Dabei können sie den Vertreter sogar präzise anweisen, oder die Entscheidung der Gruppe nur akzeptieren, wenn sie zu dem von ihnen gewünschten Ergebnis kommt. Es genügt also, nicht unmittelbar selbst oder nicht allein zu entscheiden.[5]

In der Praxis ist dieser Debiasing-Mechanismus der geteilten Verantwortlichkeit von wesentlicher Bedeutung, denn Parteien werden häufig Verträge nicht selbst abschließen, sondern werden durch eigene Mitarbeiter, Intermediäre oder Rechtsbeistände vertreten. In diesem Fall sollten Besitzeffekte deutlich reduziert werden. Das gilt vor allem auch für Organe von Unternehmen. Wichtig ist das Ergebnis aber auch für Juryentscheidungen, zum Beispiel, wenn über die Entschädigung für deliktisches Handeln entschieden wird.

C. Vertretung als Self-Debiasing-Strategie

Die Literatur nimmt bisher zumeist an, Menschen seien kaum in der Lage, Verlustaversion und Besitzeffekte selbst zu erkennen und noch weniger, sie zu kontrollieren (van Boven et al. 2000 über sog. Empathy Gaps). Eine Annahme, die viele der vorgeschlagenen rechtlichen Interventionen, die wir unten analysieren werden, motiviert hat. Meine Arbeit zeigt aber auch, dass Individuen von sich aus Debiasing-Mechanismen nutzen, also Debiasing-Strategien verwenden.

In einer weiteren Studie teste ich, ob Probanden als Self-Debiasing-Strategie gezielt ihre Verantwortlichkeit teilen, indem sie einen Vertreter Ausführungsverantwortung übertragen. Die Studie gibt den Probanden die Möglichkeit, zwei Lose zu tauschen, die beide die gleiche 50%ige Gewinnchance für denselben Gewinn anbieten. Wer tauscht erhält einen Bonus; damit gilt, wer sein Los behält, offenbart damit den eigenen Bias. Die Probanden können einen kostenpflichtigen Vertreter anweisen, den Tausch für sie vorzunehmen. Da Erfolg oder Misserfolg des Transfers zufällig sind, können die Probanden keine Expertise von ihrem Vertreter erwarten; sie können aber Verantwortlichkeit an ihn abgeben und dadurch ihre emotionalen Transaktionskosten reduzieren. Beinahe die Hälfte der Probanden nutzen die bereitgestellten Vertreter, um ihren Bias zu reduzieren. Durch die Delegation verschwindet der Besitzeffekt fast vollständig. Um ihren Bias zu überwinden, könnten sich also Inhaber von Patentrechten, Land- oder Hauseigentümer gezielt vertreten lassen, wenn das nicht ohnehin schon aus Gründen der Expertise oder aus rechtlichen Gründen der Fall ist.

Dass die Probanden mit ihrem Bias strategisch umgehen können, zeigt sich noch deutlicher in einer weiteren Studie. In vielen Situationen sind mehrere Referenzpunkte präsent, die unsere Aufmerksamkeit binden können. So kann neben dem Besitzstatus auch das Verhalten anderer Marktteilnehmer ein Referenzpunkt sein. Viele Handelsplattformen stellen diese Informationen ja in Echtzeit bereit. Wieder lasse ich die Probanden Lose mit gleichem Wert und Gewinnchance tauschen. Bevor sie jedoch entscheiden, können sie jetzt erfragen, wie die anderen Teilnehmer entschieden haben. Die Probanden wissen, dass ihnen diese Information keine Expertise verschafft, aber sie ermöglicht ihnen, ihren Referenzpunkt zu wechseln: Der kognitive Fokus verschiebt sich von ihren Besitzstatus auf das Verhalten der anderen Probanden. Wenn die Mehrheit den objektiv vorteilhaften Trade getätigt hat, dann führt dieser Referenzpunkt dazu, dass sie, bei negativem Ausgang, weniger emotionale Kosten über einen Tausch erwarten, als darüber, ihr Los zu behalten. Wieder greift eine Mehrheit der Probanden auf die Information zu und die Häufigkeit der Tauschentscheidungen steigt signifikant. Die Studie zeigt also, dass Individuen gezielt beeinflussen können, welche Referenzpunkte für ihr eigenes Entscheidenden relevant werden. Damit können sie die Richtung ihres Bias zumindest zum Teil selbst steuern. Teilnehmer an Märkten könnten also Marktinformationen selektiv nutzen, um zu verhindern, dass ihr Bias ihnen schadet.

Bias-Self-Management ist bisher wenig untersucht, weil weithin angenommen wird, Individuen könnten viele Biases kaum antizipieren. Forschung interessiert sich (auch) deshalb gerade in Business Schools vor allem für optimale Debiasing-Strategien und testet dann deren Effizienz im Vergleich zu strikt rationalem Entscheiden. Ziel ist das Entscheidungsverhalten etwa in Unternehmen zu verbessern, indem die Debiasing-Strategien gezielt erlernt werden. Self-Debiasing lässt sich aber nicht vorschreiben. Aus rechtlicher Sicht ist es deshalb interessanter, ob Menschen von sich aus Debiasing-Strategien nutzen, um Besitzeffekte zu neutralisieren, und wie effektiv diese Strategien auch ohne Intervention (und Optimierung) sind. Denn wenn Individuen Besitzeffekte selbst korrigieren können, wirft das ein neues Licht auf die dogmatischen Vorschläge, die wir im Anschluss diskutieren wollen.

Bias-Self-Management lässt sich auch gezielt fördern. So schlagen Buccafusco und Sprigman vor, für die Lizenznahme von Urheberrechten eine unabhängige Vertretungsstelle einzurichten. Wer eine Lizenz erwerben will, solle sich an diese Stelle wenden. Bias-Self-Management würde schon ermöglicht, wenn die Stelle daraufhin dem Urheber das Interesse anzeigt und anbietet, den Urheber zu vertreten. Die Vertretung verpflichtend zu machen, installiert den Debiasing-Mechanismus fest; es würden dann aber auch jene verpflichtet, die sich ohne Vertretung geeinigt hätten und jene, die das Angebot aus anderen Gründen nicht annehmen wollen. Die Autoren schlagen sogar im Sinne einer Liability-Rule die Möglichkeit vor, die Vertretungsstelle könne den Vertrag auch selbständig ohne Zustimmung des Urhebers abschließen. Das ist offenbar ein weit schärferer Eingriff, der nach meinen Ergebnissen kaum erforderlich ist, um Besitzeffekte zu kontrollieren; diese rechtlichen Folgerungen besprechen wir unten im Detail.

D. Self-Nudging: Verlustaversion produktiv nutzen

Als letzte Bias-Self-Management-Strategie betrachten wir nun Self-Nudging-Verträge, die es Individuen erlauben, die eigene Verlustaversion zu ihrem Vorteil zu nutzen.

Verlustaversion bewirkt, dass Individuen härter arbeiten, um einen Verlust zu vermeiden, als um einen Gewinn zu erreichen. Wird also ein zu erreichendes Ziel an einen möglichen Verlust geknüpft, steigen die Kosten der Nichterreichung des Ziels mit der individuellen Stärke der eigenen Verlustaversion an. Ayres zum Beispiel lässt Testpersonen Geld einzahlen als Pfand für die Erreichung bestimmter Ziele. Wird das Ziel erreicht, erhält die Versuchsperson ihr Geld zurück, andernfalls nicht. Eine solche Selbstbindung zielt darauf ab, dass Präferenzen bzgl. eigener Arbeitsziele häufig inkonsistent über die Zeit sind. So mag ein Student antizipieren, dass er, wenn die Semesterarbeit geschrieben wird, weniger Arbeit wird investieren wollen, als er vor Beginn des Semesters für optimal hält. Eine Selbstbindung hilft dann die Präferenzen des gegenwärtigen Ichs beim zukünftigen durchzusetzen.

Self-Nudging und Verlustaversion ermöglichen aber mehr als eine einfache Selbstbindung. Sie entlasten die begrenzten Ressourcen der Selbstkontrolle. Verabredet sich etwa der Student jeden Tag in der Bibliothek, um nicht zu Hause Versuchungen zu erliegen, die ihn von der Arbeit abhalten, so muss er trotzdem, um die Selbstbindung aufrecht zu halten, jeden Tag die Selbstkontrolle aufbringen, nicht doch zu Hause zu bleiben. Ist dagegen die Verlustaversion einmal aktiviert, verlangt sie keine Selbstkontrolle mehr; der kognitive Prozess erhält die Selbstbindung automatisch aufrecht und treibt das Individuum an, dass Ziel zu erreichen. Die eigene Verlustaversion als Nudge einzusetzen, könnte also besonders effektiv sein.[6] Effektives Self-Nudging verlangt aber einiges an introspektiver Kompetenz: Die Nutzer müssen erkennen, dass sie verlustavers sind; um ihre Verlustaversion ausnutzen zu können, müssen sie auch verstehen, wie sie sie gezielt aktivieren können und sie müssen abschätzen, wie sie ihr Verhalten verändert.

Um zu prüfen, ob die Probanden das leisten können, stellen wir uns zunächst einen Vertrag vor, der nach Akkord vergütet wird. Der Vertrag regelt also die herzustellende Stückzahl und verspricht dafür die Zahlung eines Lohns. Der versprochene Lohn erzeugt eine Verdiensterwartung, die dann für den Arbeiter zum Referenzpunkt wird. Um ihre Verlustaversion zu aktivieren, müssen die Arbeiter den Lohn also nicht bereits erhalten haben, es reicht aus, dass sie erwarten, ihn zu bekommen. Taxifahrer zum Beispiel haben Verdiensterwartungen für besondere oder Durchschnittstage. Eine Studie konnte zeigen, dass sie an einem überraschend schlechten Tag (weniger Fahrten als erwartet), länger arbeiten, um ihre Verdiensterwartung trotzdem zu erfüllen, während sie an einem überraschend guten Tag kürzer arbeiten, weil sich ihre

[6] Natürlich gibt es auch harte Commitment-Devices, wie zum Beispiel eine Geldanlage, die auf bestimmte Dauer festgelegt ist und verhindern soll, dass das Geld zuvor angerührt ist. Die strikte Bindung nimmt aber jede Möglichkeit der Anpassung, und kann deshalb sehr ineffizient werden, wenn sich wichtige Umstände ändern.

Erwartung schneller erfüllt. Ein rationaler Taxifahrer würde dagegen genau umgekehrt handeln und mehr an einem überraschend guten und weniger an einem schlechten Tag arbeiten, und so in kürzerer Zeit mehr verdienen.[7]

Bei unserem Vertrag befindet sich der Arbeiter also, solange er die vereinbarte Stückzahl noch nicht erreicht hat, im Verlustframe, oberhalb im Gewinnframe. Obwohl die Bezahlung oberhalb wie unterhalb des Referenzpunkts gleich ist, zieht er also, wie der Taxifahrer, unterhalb des Referenzpunkts mehr Nutzen aus seiner Arbeit, weil er einen Verlust vermeidet.

In meiner Studie gebe ich den Probanden mehrere Verträge zur Auswahl, einen kostenlosen im Gewinnframe, und drei mit einem Verlustframing, die kostenpflichtig sind und verschieden anspruchsvolle Referenzpunkte setzen. Dazu messe ich, wie stark die individuelle Verlustaversion der Probanden ist. Die Ergebnisse zeigen, wer nicht oder kaum verlustavers ist, wählt mit größerer Wahrscheinlichkeit den Vertrag im Gewinnframe. Das ist konsistent, denn er hat ja keinen Bias, der kräftig genug ist, um ihm als Selbstbindung nutzen zu können und die anderen Verträge kosten etwas. Die anderen Probanden dagegen, nutzen ihre stärkere Verlustaversion als Nudge und profitieren davon: Je stärker ihre Verlustaversion ist, desto wahrscheinlicher wählen sie einen der Verträge, die einen Verlustframe haben, und desto anspruchsvoller ist das Leistungsziel des Vertrages, den sie abschließen.

Die Studie zeigt also, dass die Individuen nicht nur ihre eigene Verlustaversion erkennen und zur Selbstbindung einsetzen, sie können offenbar auch die Stärke der induzierten Selbstbindung einschätzen und streben mit zunehmender Verlustaversion entsprechend anspruchsvollere Leistungsziele an. Die Probanden sind mit ihrer Selbstbindung auch sehr erfolgreich: Beinahe alle erfüllen das von ihnen gewählte vertragliche Leistungsziel.

Auch die Autonomie ihrer Entscheidung fördert die Effektivität des Self-Nudgings. Weil die Probanden Verlustframing und Leistungsziele selbst wählen können, verfügen sie bei der Ausführung der Aufgaben offenbar über mehr intrinsische Motivation. Dass zeigt sich in einer zweiten Studie, in der ich die Probanden, die ihren Vertrag selbst ausgewählt haben mit Teilnehmern vergleiche, denen der Vertrag zugelost wird, dessen Leistungsvereinbarung sie präferieren; sie erhalten den von ihnen bevorzugten Vertrag also durch Zufall und nicht durch ihre autonome Entscheidung. Tatsächlich sind die Probanden, die sich selbst für das Self-Nudging entschieden haben, deutlich produktiver. Vor allem oberhalb des vertraglichen Leistungsziel arbeiten sie viel häufiger

[7] Das zeigt wie schwer der Verlust relativ zur Erwartung wiegt und wie gering der Anreiz ist, wenn die Erwartung erfüllt ist, also oberhalb des Referenzpunkts, im Gewinnframe weiter zu arbeiten; der Gewinn-Verlust-Nutzen kann also Verhalten stärker beeinflussen, als die tatsächliche Bezahlung.

weiter, anstatt abzubrechen. Das lässt sich gut mit intrinsischer Motivation erklären.

Die Effektivität des getesteten Self-Nudging-Vertrags ist bemerkenswert: Tatsächlich produzieren die Probanden bei gleicher Akkordvergütung eine doppelt so hohe Stückzahl, verglichen mit dem einfachen *Gewinnframe*-Vertrag; sie verdienen also doppelt so viel und erledigen für ihren Vertragspartner die doppelte Arbeit. Wie wir gleich im Detail sehen werden, sind die rechtlichen Anwendungsmöglichkeiten von Self-Nudging-Verträgen und Regulierungen enorm vielfältig.

Wir können also als Ergebnis festhalten, dass die Effektivität der analysierten Debiasing-Mechanismen und Strategien nahelegt, dass Verlustaversion und Besitzeffekt den privaten Güterverkehr weit weniger belasten als häufig angenommen. Die Evidenz über die verschiedenen Studien hinweg, zeigt zudem an, dass Bias-Self-Management eine effektive Ergänzung der regulativen Toolbox sein kann, vor allem auch was Self-Nudging-Verträge angeht

II. Darstellung der einzelnen Kapitel

A. Besitzeffekte und markttypisches Verhalten (Chapter 2)

Behindern Besitzeffekte den privaten Austausch von Gütern? Die Evidenz dafür scheint überragend zu sein, sowohl in Psychologie, Ökonomie aber auch der empirischen Rechtswissenschaft. In der Rechtswissenschaft wird der Besitzeffekt inzwischen vielfach entsprechend als Quasi-Tatsache behandelt. In meiner Arbeit analysiere ich verschiedene Debiasing-Mechanismen, die sich sowohl auf ein neues Verständnis der kognitiven Prozesse, die Besitzeffekte verursachen, als auch auf markt- und rechtstypisches Verhalten beziehen.

Zunächst will ich zeigen, dass sich viel der vorhandenen Evidenz womöglich nur schwer auf den echten Austausch von Gütern übertragen lässt, weil es kein markttypisches Verhalten untersucht. Die meisten Experimente zum Besitzeffekt sind aus methodischen Gründen als reine Bewertungsaufgabe designt. Der Proband erhält ein Gut und soll sich dann überlegen, für welchen Betrag er das Gut verkaufen will. Dabei erhält er Anreize, seine tatsächliche Wertschätzung für das zu verkaufende Gut zu offenbaren. Es kommt für seine Entscheidung also nur darauf an, wie er das Gut selbst bewertet. Diese Bewertungsaufgabe fokussiert den Probanden auf sein Gut und forciert damit den Besitzeffekt.

Tatsächlich sind reine Bewertungsaufgaben aber eher ein Artefakt. Bei einem Umzug kann sich etwa die Frage stellen, welche Dinge wir mitnehmen wollen und welche die Transportkosten nicht wert sind. Aber schnell mischen sich auch hier strategische Überlegungen ein, die die Entscheidung beeinflussen können: Wieviel ist für die Sache zu erlösen, wenn man sie verkauft anstatt sie mitzunehmen?

In Märkten besteht die typische Aufgabe darin, ein Gut möglichst teuer an jemand anderen zu verkaufen. Diese Aufgabenstellung induziert einen ganz anderen kognitiven Prozess als die besprochene Bewertungsaufgabe: Der Verkäufer fokussiert viel weniger auf seine eigene Wertschätzung für das Gut, wichtiger ist es, die Zahlungsbereitschaft potentieller Käufer richtig einzuschätzen und diese mit der eigenen Preissetzung möglichst auszuschöpfen. So interessieren sich Verkäufer für Marktpreise, für die Qualität der Güter, die andere anzubieten haben, Trends im Verhalten der Käufer usw. Auch wer den Preis selbst gar nicht beeinflussen kann, kann überlegen, wann der beste Zeitpunkt für einen Verkauf ist. Oft geht es auch um Vorteile, die weit über das aktuelle Geschäft hinausreichen, etwa wenn ein Verkauf eine andere Investition ermöglichen oder absichern soll.

Referenzpunkt beim Verkauf eines Guts oder Rechts ist oft der Besitz- oder Inhaberstatus. Wir werden später sehen, dass auch andere Faktoren als Referenzpunkte in Betracht kommen.[8] Relativ zu einem Referenzpunkt wird ein mehr als Gewinn, ein weniger aber als Verlust wahrgenommen. Die Kernaussage der Prospekt Theorie ist nun, dass die Verluste schwerer wiegen, als die Gewinne, so dass der Verkauf eines Guts als Verlust erfahren wird, der Kaufpreis dagegen als Gewinn; es kommt zum Besitzeffekt. Die Fokustheorie nimmt an, dass die Eigenschaften des Gutes mehr Aufmerksamkeit bekommen und deshalb die Verlustaversion in der Entscheidung schwerer wiegt, wenn der kognitive Fokus auf dem Besitzstatus liegt. Im Ergebnis wird das Gut dann überbewertet.

Wenn Probanden aber markttypische Anreize haben, dann versuchen sie, für ihr Gut einen möglichst guten Preis zu erhalten. Diese Tradingaufgabe lenkt den Fokus auf Präferenzen und Zahlungsbereitschaften möglicher Käufer. Dadurch verliert der Besitzstatus verliert seine Salienz und der Besitzeffekt reduziert sich. Die Aufgabe, die die Probanden zu lösen haben, bestimmt also wie ein Referenzpunkt, worauf sich Aufmerksamkeit bezieht.

Die Hypothese meiner ersten Studie lautet also, dass eine markttypische Tradingaufgabe den Besitzeffekt signifikant reduziert. Um diese Hypothese zu testen, habe ich eine Studie im 2x4 Design angelegt; dabei variiert jedes Treatment den Besitzstatus der Probanden, vergleicht also eine *Endowment*- und eine *No-Endowment*-Bedingung.

[8] Welche Eigenschaften genau sie zu Referenzpunkten machen, hat die Forschung allerdings bisher noch nicht verstehen können.

Im *Basis*-Treatment gibt ein BDM-Mechanismus den Probanden Anreiz, ihre tatsächliche Wertschätzung für ein Ticket der Eurojackpot-Lotterie zu offenbaren. Das Treatment stellt den Probanden also eine Bewertungsaufgabe und soll den Besitzeffekt replizieren. In der *Endowment*-Bedingung wird ihnen das Los übertragen und sie müssen angeben, zu welchem Preis sie bereit wären, es zu verkaufen. In der *No-Endowment*-Bedingung werden sie aufgefordert, zwischen dem Los und Preisen aus einer Liste zu wählen. In der Preisliste markieren sie den minimalen Preis, ab dem sie den Geldbetrag dem Los vorziehen. In beiden Treatments wird dann ein Preis ausgelost. Wollten die Probanden in der *Endowment*-Bedingung zu diesem Preis ihr Los verkaufen, erhalten sie den Geldbetrag, andernfalls behalten sie ihr Los; in der *No-Endowment*-Bedingung bekommen sie, je nachdem, was sie zum Zufallspreis ausgewählt haben, Geld oder Lotterielos.

Das *Strategie*-Treatment erlaubt den Probanden, von einer strategischen Preissetzung zu profitieren: Wenn sie ihr Los verkaufen, erhalten sie den Betrag, den sie gefordert haben, solange ihre Forderung den ausgelosten Zufallspreis nicht übersteigt. Damit ändert sich die Aufgabenstellung für die Probanden: Anstatt sich ganz auf ihre persönliche Wertschätzung für das Los zu konzentrieren, können sie strategisch überlegen, wie sie ihren Preis setzen wollen, um mehr zu verdienen, und welches Risiko, das Los nicht zu verkaufen, sie dafür eingehen wollen. Die Überlegungen sollten den Aufmerksamkeitsfokus der Probanden vom Referenzpunkt "Besitzstatus" ablenken und so den Besitzeffekt reduzieren. Das innovative experimentelle Design ermöglicht es mir, den Probanden strategische Anreize zu geben, den Besitzeffekt aber trotzdem sauber zu isolieren.[9] Das wird möglich, weil der strategische Anreiz in beiden Bedingungen *Endowment* und *No-Endowment* in gleicher Weise wirkt; ich halte ihn also über die Bedingungen hinweg konstant: Die Probanden können in der *Endowment*-Bedingung einen höheren Betrag verdienen, wenn sie den Verkaufspreis hochsetzen, und in der *No-Endowment*-Bedingung, wenn sie entsprechend den minimalen Preis, zu dem sie das Geld dem Los vorziehen, anheben.

Im dritten, dem *Interaktions*-Treatment, interagieren die Probanden mit einem realen Partner. Auch hier erhalten die Verkäufer in der *Endowment*-Bedingung den Preis, den sie für das Los gefordert haben. Sie verdienen also mehr, je höher sie den Preis setzen, solange ihre Forderung nicht höher ausfällt, als der Preis, bei dem ihr Partner das Geld dem Los vorzieht. Die Zahlungsbereitschaft einer realen Gegenpartei entscheidet also, ob sie ihr Los verkaufen. Das Treatment ähnelt damit viel mehr einem Marktaustausch. Die strate-

[9] In einem normalen Käuferdesign würden die Probanden dagegen versuchen, den Preis niedrig zu halten, um mehr zu verdienen; die Bedingungen wären dann nicht vergleichbar).

gische Interaktion sollte aber nicht nur den Aufmerksamkeitsfokus verschieben und den Besitzeffekt reduzieren. Sie setzt auch einer riskanten Preissetzung eine natürliche Grenze: Bei zufälliger Auslosung bleibt es immer möglich, das Los auch für einen sehr hohen Preis zu verkaufen. Der Proband kann also auf eine hohe Auslosung spekulieren und den Preis hochsetzen, um mehr zu verdienen. Eine reale Person dagegen wird nicht beliebig viel für das Los zu zahlen bereit sein. Wenn der Verkäufer das berücksichtigt, macht es für ihn keinen Sinn mehr, einen unrealistisch hohen Preis zu fordern. Wir werden sehen, dass die Preise in der *Endowment*-Bedingung tatsächlich statistisch nicht mehr auf das Niveau der Zahlungsbereitschaft im *Basis*-Treatment fallen.

Der Transaktionspartner in der *No-Endowment*-Bedingung hat wieder die gleichen strategischen Anreize, wie der Verkäufer: Er legt den minimalen Preis fest, bei dem er das Geld dem Los vorzieht. Das Geld bekommt er, wenn sein Preis nicht über dem liegt, den der Verkäufer für das Los verlangt. Das Design ermöglicht es also genau wie im *Strategie*-Treament, Preisunterschiede zwischen den Treatments eindeutig dem Besitzeffekt zuzurechnen (und nicht den strategischen Anreizen).

Die Ergebnisse zeigen, das *Basis*-Treatment repliziert wie erwartet den Besitzeffekt: in der *Endowment*-Bedingung bewerten die Probanden das Los mit €5.79, in *No-Endowment* dagegen nur mit €3.76. Der Hypothese entsprechend, reduziert das *Strategie*-Treatment den Besitzeffekt signifikant (€5.71vs. €5.11). Im *Interaktions-Treatment* reduziert sich der Besitzeffekt noch deutlicher (€4.54 vs. €4.14) und ist nicht mehr signifikant. Dabei fällt der Verkaufspreis auf das Niveau der Zahlungsbereitschaft im *Basis*-Treatment. Haben die Probanden also eine strategische und keine Bewertungsaufgabe zu lösen, scheinen Besitzeffekte Transaktionen nicht mehr relevant zu behindern.

Um diese Entscheidungsdaten weiter zu stützen, habe ich die individuelle Verlust- und Reueaversion der Probanden gemessen. Während die Daten im *Basis*-Treatment zeigen, dass Probanden mit stärkerer Reue- oder Verlustaversion signifikant höhere Preise für ihr Lotterielos verlangen, verschwinden diese Zusammenhänge in den strategischen Treatments vollständig.

Die Fokustheorie kann Verlust- und Reueaversion als Treiber von Besitzeffekten verstehen. Die Verlustaversion wird stärker oder schwächer in Abhängigkeit von der Salienz des Referenzpunkts. Bei einer Bewertungsaufgabe zum Beispiel, ist die Salienz des Besitzstatus besonders hoch, weil die Aufgabe die Aufmerksamkeit noch zusätzlich auf Gut und Besitz steuert (Für welchen Betrag willst Du DEINE Tasse verkaufen?). Bei einer Tradingaufgabe dagegen sinkt die Salienz, weil die Aufgabe die Aufmerksamkeit auf das zu ihrer Erfüllung erforderliche lenkt: Zu einem möglichst guten Preis zu verkaufen, verlangt die Einschätzung der Zahlungsbereitschaft potentieller Käufer, betrifft aber weniger die eigene Wertschätzung für das Gut. Damit zieht die Aufgabestellung Aufmerksamkeit vom Besitzstatus ab.

Reue wird empfunden über die entgangenen Vorteile einer nicht gewählten Entscheidungsoption. Zum Beispiel, wenn der Inhaber eine Aktie verkauft, die dann im Wert steigt, dass er sich nicht entschieden hat, die Aktie zu halten. Wenn der Inhaber also überlegt, ob er verkaufen will, dann antizipiert er sowohl, dass er bereuen wird, die Aktie nicht gehalten zu haben, wenn ihr Wert steigt, als auch, sie nicht abgestoßen zu haben, sollte ihr Wert sinken. Der Referenzpunkt "Besitzstatus" fokussiert dann die Aufmerksamkeit auf die Reue, die der Inhaber über den Verkauf zu empfinden erwartet, wenn der Wert der Aktie im Anschluss steigt. Der resultierende Besitzeffekt erschwert ihm dann den Verkauf. Die Tradingaufgabe führt wieder dazu, dass der Fokus vom Besitzstatus abgelenkt wird und sich damit die Aufmerksamkeit weniger auf den möglicherweise entgehenden Gewinn bei Verkauf der Aktien richtet, als darauf, als auf die potentielle Zahlungsbereitschaft der Käufer. Damit kommt das Reueempfinden über die beiden Entscheidungsoptionen wieder (mehr) in die Balance, der Besitzeffekt verschwindet und der Inhaber kann seine Aktien leichter verkaufen.[10]

Ob Verlust- oder Reueaversion dominant sind, hängt dann vor allem von der Natur der ausgetauschten Güter ab. Geht es um Risikogüter wird vor allem Reueaversion den Besitzeffekt antreiben, geht es dagegen um Konsumgüter mit weniger spekulativem Wert, tritt wahrscheinlich die Verlustaversion mehr in den Vordergrund.

Um diese Theorie zu testen, habe ich in einer Folgestudie – anstatt der Lose - Tassen der Universität Münster verwendet. Die Ergebnisse der beiden Studien entsprechen sich: Der Besitzeffekt mit den Tassen im *Basis*-Treatment tritt genauso ausgeprägt auf, wie in der Lotteriestudie, während er im *Strategie*-Treatment vollständig verschwindet.

Die bisherige Literatur verfolgt hingegen einen ganz anderen Ansatz und versucht zu zeigen, dass Märkte ihre Akteure unterrichten, und der Bias deshalb bei erfahrenen Marktteilnehmern weniger ausgeprägt sei. Dabei wird etwa angenommen, dass unerfahrene Marktteilnehmer sich weder der eigenen noch der Wertschätzung des Marktes sicher seien, etwa weil sie die wertbildenden Eigenschaften des zu veräußernden Gutes nicht genau kennen. Die Unsicherheit solle dazu führen, dass Verlustaversion größeren Einfluss auf ihre

[10] Wohlgemerkt, dass bedeutet nicht, dass der Inhaber keine Reue mehr über die beiden Entscheidung alternativen zu empfinden erwartet, wenn sie zu einem nachteiligen Ergebnis führen, sondern nur dass der Besitzeffekte die Balance zwischen den Entscheidungsoptionen nicht mehr verzerrt.

Entscheidung gewinnen kann, der mit zunehmender Sicherheit in der Preissetzung zurückgedrängt werde.

List etwa hat in einer Feldstudie gezeigt, dass nur sehr erfahrene Händler keinen Besitzbias zeigen. Seine Studie nutzt das Tauschparadigma: Er gibt seinen Probanden, die allesamt Akteure eines echten Marktes für Autogrammkarten von bekannten Sportlern sind, eine Spielerkarte. Dann bietet er ihnen an, diese Karte gegen eine zweite zu tauschen. Die Null-Hypothese lautet, dass eine Karte gleich häufig getauscht werden sollte, ob der Proband die Karte in seinem Besitz hat oder nicht. Zeigt sich hingegen, dass Karten seltener getauscht wurden, wenn sie im Besitz der Händler sind, dann haben die Händler einen Besitzbias. Nun teilt List die Händler daraufhin in Gruppen auf, ob sie erfahren oder unerfahren sind und kann so analysieren, ob in einer der beiden Gruppen der Bias kleiner ist.

Es ist leicht zu erkennen, dass das Tauschparadigma den Händlern eine reine Bewertungsaufgabe stellt.[11] Sie können keinen größeren Verdienst durch strategisches Verhalten erzielen. Es ist in ihrem besten Interesse zu offenbaren, für welche der beiden Karten sie eine höhere Wertschätzung haben. List sichert sogar noch weiter ab, dass sich den Händlern eine reine Bewertungsaufgabe stellt: Er hat Karten ausgewählt, die von gleichem wirtschaftlichem Wert waren. Damit hat er auch strategische Überlegungen abgeschnitten, ob eine Karte besser weiterzuverkaufen wäre als die andere. Da ihr Fokus im Tauschparadigma also auf der Spielerkarte liegt, die sie in Besitz haben, sollten die Händler einen Besitzeffekt zeigen. Und tatsächlich, auf die Gesamtpopulation der Händler gesehen, findet List einen ausgeprägten Bias. Nur in der Gruppe der ganz erfahrenen Händler findet er keinen Besitzeffekt. Sie hatten im Schnitt mehr als 10 Jahre Erfahrung in genau dem Markt für Autogrammkarten, in dem List ihr Verhalten getestet hat. Selbst die Gruppe der Händler, die einen starken Bias aufwiesen, hatte im Durchschnitt immerhin noch 6 Jahre Erfahrung in diesem Markt. Wenn Lists Theorie richtig ist, müsste es also im Markt für Autogrammkarten sehr viel zu lernen geben. [12]

Die Fokustheorie erklärt das Ergebnis anders: List hat mit Bewertungsaufgabe und der wirtschaftlichen Gleichwertigkeit der Güter den Besitzstatus sehr salient gemacht. Die meisten Händler fokussieren also auf das Gut und

[11] In gleicher Weise lässt sich auch für die anderen verwendeten Paradigmen zeigen, dass sie eine Bewertungsaufgabe stellen.

[12] Engelmann und Holland nehmen an, Marktteilnehmer müssten Erfahrung mit dem Prozedere des Marktes sammeln, um Besitzeffekte zu überwinden. Sie verwenden aber auch ein Tauschparadigma. Loomes et al. haben Probanden dutzende Runden von Auktionsspielen durchlaufen lassen, bis sich die Lücke zwischen WTA und WTP reduziert hat. Die Autoren haben daraus geschlossen, die Probanden hätten durch diese Erfahrung erst Angebot und Nachfrage besser verstehen müssen. Aber auch ihre Probanden waren incentiviert, ihre tatsächliche Wertschätzung zu offenbaren.

ihre persönliche Wertschätzung für die Spielerkarten und weisen dementsprechend einen Bias auf. Dass die erfahrensten Händler ihren Bias trotzdem neutralisieren, könnte daran liegen, dass sie – auch bei gleichem Wert der Karten - mit größerem Automatismus an die Marktgegenseite denken, und was sie wohl für die oder andere Karte zu zahlen bereit wären. Dass hingegen die weniger erfahrenen Händler auch nach durchschnittlich sechsjähriger Erfahrung die Eigenschaften der Güter noch nicht kennen, wirkt dagegen wenig plausibel.

Meine Studie schließt Erfahrung als Einflussfaktor aus. Das Marktspiel, an dem sie teilnehmen, hat nur eine Periode, der Besitzeffekt verschwindet also sofort, ohne dass die Probanden Gelegenheit hätten, Erfahrungen mit dem Gut oder ihren Präferenzen zu sammeln. Die Probanden haben zudem vorher auch an keinen anderen Marktexperimenten teilgenommen. Mit Erfahrung als Debiasing-Mechanismus sind die Ergebnisse also nicht konsistent. Die Fokusverschiebung durch die Tradingaufgabe dagegen, ist ein automatischer kognitiver Prozess, der unbewusst abläuft und nicht verlangt, dass die Probanden etwas lernen.[13]

Der Anwendungsbereich der Studie ist groß. Die meisten Marktransaktionen, haben zum Ziel, einen möglichst guten Preis zu erzielen, während der Vertragspartner möglichst wenig bezahlen will. Da der Debiasing-Mechanismus zudem an die kognitive Aufgabe und nicht an Markterfahrung geknüpft, sollte er die meisten Transaktionen und Marktakteure betreffen. Schon diese erste Studie legt also nahe, dass Besitzeffekte eine geringere Belastung für den Rechtsverkehr bedeuten könnten, als die rechtliche Literatur annimmt.

B. Sozialer Entscheidungskontext beeinflusst kognitiven Fokus (Chapter 3)

Wir haben jetzt gesehen, dass Referenzpunkte nicht immer in gleicher Weise salient für Individuen sind. So lenkt in Märkten die strategische Tradingaufgabe den kognitiven Fokus der Marktakteure vom Besitzstatus ab. Das gleiche lässt sich folgerichtig auch von anderen starken Motivationen vermuten, wie etwa für soziale Präferenzen, wenn Individuen Entscheidungen treffen, die das Wohlergehen anderer betreffen. Individuen fokussieren dann nicht allein, wie in einer Bewertungsaufgabe, auf das Gut und dessen Wert, sondern darauf, welche sozialen Konsequenzen ihre Verkaufsentscheidung für andere hat. Als ein Beispiel für eine Situation, in der nicht nur strategische Anreize, sondern auch soziale Präferenzen Besitzeffekte beeinflussen können, analysieren wir die Anticommons.

[13] Das bedeutet natürlich nicht, dass keine Erfahrung erforderlich wäre, um wirtschaftliche und am Markt durchsetzbare Preise zu setzen.

Die Anticommons ist ein soziales Dilemma, weil es spieltheoretisch für jeden Beteiligten die dominante Strategie ist, den eigenen Vorteil, der sich aus dem Projekt ziehen lässt, zu maximieren, so dass das Projekt nicht zustande kommt.[14] Ein prominentes Beispiel dafür ist die Entwicklung von Medikamenten, die die Nutzung von vielen Patenten erfordern, die häufig von verschiedenen Parteien gehalten werden. Die Patentinhaber können dann die Entwicklung des Medikaments verteuern, indem sie versuchen, die Hold-up Situation ausnutzen und den Preis für die Lizenznahme hochzutreiben. Versucht jeder den größtmöglichen Anteil des von dem Medikament zu erwartenden Gewinns für sich abzuschöpfen, scheitert die Entwicklung an den Kosten, und alle gehen leer aus. Ein anderes Beispiel sind öffentliche Entwicklungsprojekte, die häufig auf Landkauf von mehreren Parteien angewiesen sind. Das Hold-up Problem wird zwar durch die Eminent Domain begrenzt, die Eigentümer können aber das Verfahren verzögern und die Entschädigungsregeln lassen einigen Spielraum, einen strategischen Preis zu verlangen.

Die Forschung an sozialen Dilemmata zeigt aber, dass die meisten Individuen sich nicht so selbstbezogen verhalten. Sie konditionieren ihre Kooperation auf das Verhalten, das sie von den anderen Beteiligten erwarten und berücksichtigen die negativen Externalitäten, mit denen ihr Verhalten andere belasten kann, etwa das ein wichtiges Infrastrukturprojekt nicht zustande kommt. Umgekehrt sind sie aber auch bereit, andere für eine Verletzung der Kooperation zu bestrafen und treiben dann selbst den Preis in die Höhe. Das Verhalten in der Anticommons ist also stark von sozialen Präferenzen und Reziprozität bestimmt, in positiver Hinsicht, aber auch in negativer.

Schon die strategische Aufgabe, die die Beteiligten in der Anticommons zu lösen haben, sollte, wie wir oben gesehen haben, den Besitzeffekt reduzieren. Was uns aber an der Anticommons aber interessiert, ist nicht diese strategische Aufgabe, sondern ob auch soziale Präferenzen, also Rücksichtnahmen auf andere und Reziprozität den kognitiven Fokus so ablenken können, dass der Besitzeffekt verschwindet.

Das Studiendesign, das diese These prüfen soll, erzeugt die wechselseitige Abhängigkeit der Auszahlungen der Beteiligten, die die Anticommons charakterisiert, indem jeweils drei Probanden eine Eigentümergemeinschaft bilden und der Käufer nur die Güter aller drei Gruppenmitglieder zusammen kaufen kann, aber nicht allein; andernfalls kommt kein Geschäft zustande. Um zu

[14] Hellers ursprüngliches Beispiel für die Anticommons ist Eigentum an einem Gebäude, das so fragmentiert ist, dass viele Eigentümer Ausschlussrechte haben, mit denen sie sich gegenseitig an einer effektiven Nutzung hindern. Heller hat damit leere Schaufensterfronten in Moskau erklärt, in die lukrative Geschäfte hätten einziehen können, hätten sich die vielen Eigentümer, denen die Häuser in Gemeinschaftseigentum gehörten, auf eine bestimmte Nutzung und eine von allen akzeptierte Verteilung der aus der Nutzung fließenden Vorteile, einigen können.

messen, ob der Besitzeffekt verschwindet, vergleiche ich die *Endowment*-Bedingung, in der die Probanden je ein Lose erhalten und verkaufen können, mit der *No-Endowment*-Bedingung, in der sie sie wieder zwischen Los und Geldbetrag wählen können. Ein Zufallsmechanismus bestimmt den Preis.

Das *Summenpreis-Treatment* addiert die Einzelpreise, die die Probanden jeweils für ihr Los verlangen, zu einem Gemeinschaftspreis und vergleicht diese Summe mit dem ausgelosten Preis. Die Probanden bekommen den Preis für ihr Los, den sie persönlich gefordert haben. Das gilt allerdings nur, solange die *Summe* der Einzelpreise, die die Gruppenmitglieder fordern, den ausgelosten Zufallspreis nicht übersteigt. Ist das der Fall, verkauft keiner der Probanden sein Los. Strategischer Anreiz und soziale Präferenz sind in diesem Design also fest verbunden: Die Probanden können davon profitieren, einen hohen Preis zu verlangen. Mit einer übertriebenen Preissetzung können sie die anderen Probanden aber zugleich schädigen.

Im Ergebnis findet sich im *Summenpreis*-Treatment nur ein marginaler Unterschied zwischen *Endowment* und *No-Endowment*-Bedingung (€0.17); ein Besitzeffekt ist also nicht mehr nachweisbar. Als Vergleich dient ein *Kontroll*-Treatment, das einem BDM-Mechanismus verwendet, der den Probanden den Anreiz gibt, ihre tatsächliche Wertschätzung für das Los zu offenbaren (Bewertungsaufgabe). Das Treatment zeigt eine große Preisdifferenz zwischen WTA und WTP (4.07 versus 6.99), also - wie zu erwarten - einen starken Bias.

Das *Maximal-Preis*-Treatment soll den Einfluss sozialer Präferenzen auf den Besitzeffekt messen. Um den Sozialbezug der Verkaufsentscheidung zu erzeugen, hängen die Auszahlungen der Probanden wieder wechselseitig von den Entscheidungen der anderen ab. Das Treatment vergleicht aber nur den höchsten, der von den drei Gruppenmitgliedern aufgerufenen Einzelpreise, mit dem Zufallspreis. Übersteigt dieser den Zufallspreis, kann keiner der Probanden sein Los verkaufen. Wenn es zum Verkauf kommt, erhalten die Probanden immer nur den ausgelosten Preis und nicht den Betrag, den sie selbst verlangt haben. Das schließt strategische Anreize aus und ermöglicht es damit, den Einfluss der sozialen Präferenzen zu isolieren.

Im Ergebnis reduziert der Sozialbezug der Entscheidung den Besitzeffekt deutlich, die Preisdifferenz sinkt auf €1.01 und damit auf weniger als ein Drittel der Größe der Differenz im *Kontroll*-Treatment.

Um zu zeigen, dass es wirklich die sozialen Präferenzen der Probanden sind, die den Besitzeffekt reduzieren, misst ein Instrument ihre soziale Werteorientierung. Und in der Tat, der Besitzeffekt reduziert sich mit der Stärke ihrer sozialen Präferenzen. Im Einklang mit der kognitiven Fokustheorie legen die Ergebnisse also nahe, dass je sensibler die Probanden für die Konsequenzen ihrer Handlungen für andere sind, desto mehr verlagert sich der Fokus ihrer Aufmerksamkeit von dem Referenzpunkt "Besitz" auf den sozialen Kontext ihrer Entscheidung. Sind Transaktionen in einen sozialen Kontext eingebettet, verliert der Besitzeffekt also an Stärke.

Die Rechtsordnung kann das auch gezielt für ein Debiasing nutzen. So haben nach der Kelo-Entscheidung einige Staaten ihre Anforderungen daran, wie die sozialen Vorteile eines Entwicklungsprojects beschaffen sein müssen, damit es als Public-Use gelten kann und eine Eminent Domain rechtfertig, verschärft. Damit wird nicht nur (verfassungs-)rechtlichen Anforderungen entsprochen und die soziale Akzeptanz einer Enteignung erhöht, die höhere Salienz der sozialen Zweckbindung kann auch dazu führen, dass die Transaktionen leichter zustande kommen, weil Besitzeffekte abgebaut werden.

C. Debiasing und Self-Debiasing durch Vertreter und kollektive Entscheidungen (Chapter 4)

Marktteilnehmer nutzen häufig zur Umsetzung ihrer Rechtsgeschäfte Institutionen. Auch sie können ein Debiasing ermöglichen. Als Beispiele für Institutionen, die ein Debiasing ermöglichen, analysiere ich die Vertretung und kollektive Entscheidungen. Sie erlauben es, Verantwortlichkeit zu teilen und dadurch Reueempfinden über den schlechten Ausgang eines Geschäfts zu reduzieren.

Individuen können gezielt bestimmte Institutionen in ihre Transaktion einbinden, um ihren Bias zu überwinden. Ich spreche dann von "Self-Debiasing". Bisher hat die Literatur weitgehend angenommen, dass sich Individuen ihrer Verlustaversion kaum bewusst sind und deshalb etwa eine von Besitzeffekten gesteigerte Wertschätzung als ihre natürliche Präferenz erfahren; so sieht etwa viel der Forschung, die an Business Schools angesiedelt ist, ihre Aufgabe darin, Entscheidungsträger auf typische kognitive Fehler aufmerksam zu machen und Prozeduren zu entwickeln, wie sie diese Fehler vermeiden können. Bias-Self-Management setzt dagegen voraus, dass Individuen ihren Besitzeffekt erkennen (und auch bestimmte andere Biases).

Wir haben oben gesehen, dass erwartetes Reueempfinden über nicht gewählte Entscheidungsoptionen emotionale Transaktionskosten und Besitzeffekte auslösen können. Reueempfinden und Verantwortlichkeit sind eng verknüpft. Je grösser die Verantwortung ist, die ein Entscheidungsträger für eine Transaktion trägt, desto mehr Reue wird er darüber zu erfahren erwarten, wenn sie sich später eine bessere Alternative zeigt. Institutionen können nun in diesem Zusammenhang von Reueempfinden und Verantwortung eingreifen, weil sie häufig die Verantwortlichkeit für eine Transaktion zwischen vielen Entscheidungsträgern aufteilen. So wird der Vertreter für den Prinzipal tätig und übernimmt damit selbst bei klarer Anweisung eine Ausführungsverantwortung. Studien haben gezeigt, dass diese Ausführungsverantwortung als zentral wahrgenommen wird, da in einer Kausalkette häufig dem letzten Glied der größte Teil der Verantwortung zugeschrieben wird. Auch wenn in Ausschüssen kollektiv entschieden wird, wird die Verantwortung des Einzelnen aufgeteilt. Die zentrale Hypothese der Studie ist es, dass Reueaversion und Besitzeffekte sich signifikant abschwächen oder ganz verschwinden, wenn Vertreter in die Transaktion eingebunden werden oder über die Transaktion im Abstimmungsverfahren beschieden wird.

In meinem Experiment erhält jeder Proband zunächst ein Lotterielos, das entweder einen “Kopf” oder eine “Zahl” trägt. Ob das Los gewinnt, entscheidet ein Münzwurf. Gewonnen hat das Los, das das gleiche Symbol wie der Münzwurf zeigt, also beide “Kopf“ oder “Zahl“. Jedes Los hat damit die gleiche Gewinnchance von 50 Prozent. Gewinnt der Proband, so verdient er 8€, verliert sein Los, erhält er nichts. Die Probanden können ihr Los gegen ein zweites tauschen, das das entgegengesetzte Symbol trägt, sie tauschen also Kopf gegen Zahl oder umgekehrt und erhalten dafür einen Bonus von €0.25. Strikt rationale Probanden sollten also immer tauschen. Mit dem Tausch erhalten sie den gleichen Erwartungswert plus jene €0.25; deshalb offenbart, wer nicht tauscht, einen Besitzeffekt.

Das Lotterie-Design stellt sicher, dass sowohl der Vertreter als auch die anderen Teilnehmer im Abstimmungsverfahren über genau die gleichen Informationen verfügen, wie der entscheidende Proband. Naturgemäß ist der Ausgang der Lotterie zufällig und den Probanden ist bekannt wie alle anderen Beteiligten bezahlt werden. Wenn die Probanden sich also entscheiden, die Tauschentscheidung an den Vertreter oder die Mehrheit zu delegieren, dann tun sie das offenbar, um ihre Verantwortlichkeit aufzuteilen, und die Reue, die sie über einen Tauschvorgang mit schlechtem Ausgang zu empfinden erwarten zu reduzieren. Kontrollfragen bestätigen, dass sich die Probanden darüber klar sind, dass sie per Delegation an Vertreter oder Votum keine Expertise einholen können.

Das *Basis*-Treatment zeigt den erwarteten Bias: Mehr als 70% der Probanden tauschen nicht. Im ersten *Agent*-Treatment erhalten die Probanden einen Vertreter, der für sie entscheidet, ob das Los getauscht wird oder nicht. Der Vertreter wird abstrakt von der Transaktion und deren Ausgang mit €2 entlohnt, aber nur wenn er das Los tauscht, ansonsten bekommt er nichts. Der Prinzipal kann die Transaktion des Vertreters genehmigen oder ablehnen und dann selbst entscheiden. Wie erwartet, führt die Aufteilung der Verantwortlichkeit für den Tausch zwischen Prinzipal und Vertreter dazu, dass der Besitzeffekt gemildert wird und nun beinahe 69% der Probanden ihr Los tauschen. Das Ergebnis hat sich also umgedreht. Eine spätere Online-Studie repliziert dieses Resultat.

In einem weiteren Treatment weist der Prinzipal den Vertreter sogar *vor* dem Transfer explizit an, ob er das Los tauschen oder halten soll und der Vertreter erhält €2, wenn er diese Anweisung umsetzt. Handelt der Vertreter nicht seiner Instruktion entsprechend, kann der Prinzipal den Tausch an sich ziehen. Wieder tauschen mit mehr als 75% signifikant mehr Probanden ihr Los als im *Basis*-Treatment. Das Treatment zeigt die Effektivität des Debiasing, weil der Vertreter den Prinzipalen auch dann noch von Verantwortlichkeit entlastet, wenn er nur noch die reine Ausführungsverantwortlichkeit trägt.

Im *Voting*-Treatment schließlich, stimmen die Probanden darüber ab, ob sie ihre Lose tauschen wollen oder nicht. Dabei entscheidet die Mehrheit. 85% der Teilnehmer stimmen für den Tausch und 88% der Probanden tauschen letztlich ihr Los. Nur 12% machen von ihrem Veto-Recht Gebrauch, mit dem sie das Los entgegen der Mehrheitsentscheidung halten können.

Der zweite Teil der Studie analysiert, ob die Probanden auch ohne Intervention von sich aus die Institutionen nutzen, um ihren Bias selbst zu neutralisieren. Es geht als um Bias-Self-Management. Der Vertreter ist nun optional, die Probanden müssen ihn selbst einschalten. Ihn einzubinden hat Opportunitätskosten: Die Teilnehmer wissen, dass sie 10 Minuten auf die Entscheidung des Vertreters warten müssen, während sie andernfalls Transaktion und Experiment sofort abschließen können. Die Probanden absolvieren zunächst das *Optional-Agent*-Treatment und dann, mit neuen Instruktionen, das *Basis*-Treatment ohne Vertreter.[15] Das Within-Subject-Design erlaubt es, die Probanden zu identifizieren, denen es gelingt, ihr Los zu verkaufen, weil sie den Vertreter mit der Entscheidung beauftragen. Im *Optional-Voting*-Treatment können die Probanden die Entscheidung über den Tausch an ein Mehrheitsvotum übertragen und müssen auch dafür mit ihrer Zeit bezahlen.

Ungeachtet der Opportunitätskosten schalten mehr als die Hälfte der Probanden den Vertreter ein und 45% lassen die Gruppe über den Tausch abstimmen. In beiden Fällen führt das zu signifikant höheren Transaktionsraten verglichen mit dem *Basis*-Treatment: Mehr als 85% der Probanden im *Optional-Agent*- und im *Optional-Voting*-Treatment tauschen ihr Los.

Das Within-Subject-Design offenbart zusätzlich, wer tauscht, weil er die Institutionen eingebunden hat: 25% der Probanden, die ihr ursprüngliches Los im *Basis*-Treatment behalten, beauftragen den Vertreter, das Los für sie zu tauschen. Im *Voting*-Treatment sind es 18%, die auf die Institution zugreifen und dann tauschen. Die anderen Probanden ändern mit ihrem Zugriff auf die Institutionen zwar nicht die eigene Entscheidung, sie haben aber trotzdem einen Vorteil von der Delegation: Die Reue, die sie für den Fall zu erfahren erwarten, dass das Los zu behalten die bessere Alternative gewesen wäre, ist zwar auch ohne Vertreter nicht so groß, dass sie sie am Tausch ihres Loses

[15] Der Strategie-Methode entsprechend werden sie informiert, dass das Los darüber entscheiden wird, für welches der beiden Treatments sie bezahlt werden.

hindert, die Delegation senkt aber die emotionalen Kosten des Tausches. Dieses Ergebnis zeigt ein Survey-Instrument: Im *Basis*-Treatment antizipieren die Probanden eine größere Reueaversion über den Tausch des Loses, als darüber, es zu behalten. Wenn sie aber einen Vertreter beauftragen oder die Mehrheit die Entscheidung trifft, verschwindet dieser Zusammenhang. Das gilt vor allem für die Teilnehmer, die ihre Entscheidung durch die Delegation tatsächlich ändern und ihr Los tauschen.

Schließlich bestätigt ein zweites Instrument den angenommenen Zusammenhang zwischen Verantwortlichkeit und Reueempfinden. Wenn die Probanden ihre Verantwortlichkeit durch Delegation aufteilen können, schreiben sie sich eine signifikant geringere Verantwortung für einen negativen Ausgang des Tausches zu. Ihr Reueempfinden korreliert dabei stark mit dieser Zurechnung, und schwächt sich ab, je mehr Verantwortung sie bei anderen sehen. Die Studie zeigt also das Besitzeffekte in institutionellem Kontext an Bedeutung verlieren, und dass Menschen den institutionellen Rahmen gezielt nutzen, um ihren Bias auszuschalten.

D. Bias-Shifting (Chapter 5)

Self-Debiasing ist nur eine erste Strategie, die Entscheider nutzen können, um zu verhindern, dass ein Bias ihre Entscheidungen beeinträchtigt. Eine zweite Möglichkeit ist es, den Bias so zu verlagern, dass er nicht schadet. Diese Strategie bezeichne ich als Bias-Shifting.

Als Beispiel für diese Strategie soll das häufig an Märkten zu beobachtende Herdenverhalten dienen. Eine zentrale Eigenschaft von Märkten ist, dass sie Informationen bündeln. Marktteilnehmer sehen, wie sich andere verhalten, zu welchen Preisen sie anbieten, kaufen und verkaufen. Häufig stehen diese Informationen sogar unmittelbar zur Verfügung wie etwa auf Onlinehandelsplattformen. So kann es leicht zu Herdenverhalten kommen: Fällt etwa eine Aktie im Wert und einige Inhaber verkaufen sie, so stoßen schnell auch viele andere sie ab. Dabei kann es sich natürlich um eine rationale Reaktion handeln, wenn dem Kursverfall eine objektive Schwäche der Aktie zugrunde liegt, oder die Markteilnehmer vermuten, andere könnten über bessere Informationen oder Expertise verfügen als sie selbst.

Die Entscheidungen Anderer haben aber auch einen nicht rationalen Einfluss auf Marktteilnehmer. Menschen richten sich als soziale Wesen oft nach der Gruppe. In einer bekannten Studie etwa wird Probanden eine Box mit eindeutig unterschiedlich lang gezeichneten Strichen präsentiert; andere vermeintliche Teilnehmer, tatsächlich sind sie Assistenten des Versuchsleiters, signalisieren jedoch, die Striche seien gleich lang. Selbst gefragt, schließen sich die Probanden dann dieser Aussage an. Das Verhalten der Gruppe ist also ein natürlicher Referenzpunkt für unser Entscheiden. Das ergibt ein behaviorales

Motiv, auch in Märkten der Gruppe zu folgen: Die Marktteilnehmer antizipieren, dass sie es besonders bereuen werden, wenn sie vom Verhalten der Gruppe abweichen und sich diese Entscheidung später als nachteilig erweist.

Mit Regret- und Fokus-Theorie erklären wir das folgendermaßen: Reueempfinden bezieht sich auf den entgangenen Nutzen einer nicht gewählten Entscheidungsalternative. Stellen wir uns einen Entscheider vor, der sich entweder für A oder B entscheiden kann. Er antizipiert, dass er bereuen wird nicht B gewählt zu haben, wenn er sich für A entscheidet, und A schlecht ausgeht. Er sieht aber auch voraus, bei schlechtem Ausgang B zu bereuen, wenn er sich für A entschieden hat. Wie hoch die erwarteten Reuekosten für die beiden Entscheidungsalternativen ausfallen, hängt dabei vom Referenzpunkt des Entscheiders ab. Ist der Besitzstatus der Referenzpunkt, so wiegt die Reue, die unser Aktieninhaber darüber zu empfinden erwartet, dass die Aktie, die er hätte behalten können, im Wert gestiegen ist, schwerer. Das Ergebnis ist ein Besitzeffekt, der ihn gegen den Verkauf einnimmt. Beobachtet der Rechtsinhaber aber nun, dass viele andere am Markt ihre Unternehmensanteile verkaufen, so kann dieses dominante Marktverhalten so salient werden, dass es den Status quo als Referenzpunkt ablöst. Damit wiegt dann die Reue, die der Aktieninhaber darüber zu empfinden erwartet, wenn er entgegen der Mehrheit seine Aktie nicht verkauft, und sie dann im Wert fällt. Der Besitzeffekt verschwindet.

Die Bias-Shifting-Studie soll diesen Mechanismus belegen. Das grundsätzliche Design entspricht dabei dem der zweiten Studie zur Einbindung des Vertreters. Die Probanden erhalten wieder ein Lotterielos, dass sie gegen ein zweites Los plus Bonus tauschen können. Gemessen wird, wie häufig die Probanden ihr Los tauschen und wieviel Reue sie über den Tausch zu empfinden erwarten, sollte das abgegebene Los gewinnen und das eingetauschte verlieren. Das *Basis*-Treatment zeigt den erwartet ausgeprägten Besitzeffekt.

Im *Herden*-Treatment werden die Teilnehmer darüber informiert, dass sich in einer vorherigen Studie eine Mehrheit von Probanden für den Tausch ihrer Lose entschieden hat. Das Lotterie-Design macht dabei salient, dass die früheren Teilnehmer keine besseren Informationen über die Erfolgsaussichten der Lose gehabt haben können, als die nun dem *Herden*-Treatment zugeteilten Probanden. Die Probanden können also keine Expertise hinter der Entscheidung der Mehrheit vermuten. Kontrollfragen bestätigen, dass die Teilnehmer dieses zentrale Element der Studie verstanden haben. Um eine gegenseitige Einflussnahme zu verhindern, entscheiden die Teilnehmer jeder für sich in einem separaten Raum.

Wie erwartet, tauschen im *Herden*-Treatment mit mehr als 55% der Probanden beinahe doppelt so viele und signifikant mehr Probanden ihr Los als im *Basis*-Treatment. Zusätzlich hat sich auch ihr Reueempfinden verschoben: während die Probanden im *Basis*-Treatment mehr Reue über einen Tausch zu empfinden erwarten, kehrt sich der Zusammenhang im *Herden*-Treatment um. Das *Herden*-Treatment zeigt damit, dass an Märkten typischerweise mehr Referenzpunkte präsent sind, als allein der Status quo und zweitens, dass sich in einem solchem Umfeld Referenzpunkte ihrer relativen Salienz entsprechend verschieben können. Beides sind Voraussetzungen für ein Bias-Self-Management, das es dem Entscheider erlaubt, bewusst den eigenen Referenzpunkt zu verändern. So könnten Entscheider gezielt auf Markt-informationen zugreifen, wenn sie bemerken, dass ihre Reueaversion ihnen hohe Kosten für einen Verkauf auferlegt. Wenn Sie lernen, dass die dominante Entscheidung am Markt "verkaufen" ist, dann könnte sich mit dieser Information ihr Referenzpunkt verschieben und ihr Reueempfinden umkehren, so dass es sie zum Verkauf drängt.

Um die Bias-Self-Management-Hypothese zu testen, habe ich in einer weiteren Studie ein *Information*-Treatment durchgeführt. In diesem Treatment werden die Probanden nicht automatisch über die dominante Marktentscheidung informiert. Stattdessen können sie selbst entscheiden, ob sie auf die Information zugreifen wollen oder nicht. Weil die Probanden einige Zeit auf die Information warten müssen, verzögert sich das Experiment, so dass sie mit Opportunitätskosten für die Information bezahlen.

Im Ergebnis entscheiden sich trotz dieser Kosten die Hälfte der Probanden dafür, die Information einzuholen. Über 90% der Teilnehmer, die auf die Information zugreifen, tauschen später auch ihr Los, so dass insgesamt signifikant mehr Teilnehmer tauschen, als im *Basis*-Treatment (88% im Vergleich zu 55%). Zudem reduzieren die Probanden, die auf die Information zugreifen, die emotionalen Transaktionskosten des von ihnen präferierten Tausches signifikant gegenüber den Probanden im *Basis*-Treatment.

Schließlich unterstützen die Daten auch direkt, dass die Probanden auf die Information zugreifen, damit sie den Tausch vollziehen können. Zunächst erwarten die Teilnehmer, die die Information abfragen, bei negativem Ausgang signifikant weniger Reue über den Tausch zu empfinden, wenn er schlecht ausgeht, als die Probanden, die die Information nicht abfragen. Schließlich geben fast die Hälfte aller Teilnehmer an, dass sie auf die Information zugegriffen haben, weil sie erwartet haben, es dann eher zu tauschen.

Das zweite Bias-Self-Management-Treatment *Multiple-Referenzpunkte* präsentiert den Probanden die Tauschentscheidungen zweier Gruppen. In der ersten Gruppe haben die Probanden sich mehrheitlich gegen den Tausch ihrer Lose entschieden; in der zweiten Gruppe hatten die Probanden die Möglichkeit, einen Vertreter zu beauftragen und haben mehrheitlich ihr Los getauscht.

Die Mehrheitsverhältnisse an sich sind dabei sehr ähnlich, in beiden Gruppen jeweils etwa 70 zu 30 Prozent, einmal für den Tausch und einmal dagegen.

Jede der beiden Gruppen bildet also einen Referenzpunkt von gleicher Salienz. Die Frage lautet, fokussieren die Probanden nun zufällig auf die eine oder andere Gruppe, oder ist eine ihr vorrangiger Referenzpunkt? Die Bias-Self-Management-Hypothese lautet, dass die Probanden auf den Referenzpunkt fokussieren, der die von ihnen gewünschte Entscheidung begünstigt. Wenn die Probanden gezielt auf die Gruppe fokussieren, die in der Mehrheit ihr Los getauscht hat, dann sollten sie ihr Los in etwa so häufig tauschen können, wie die Teilnehmer des *Herden*-Treatments. Fokussieren dagegen einige auf die eine und andere Probanden auf die andere Gruppe, sollte die Tauschfrequenz unter der im *Herden*-Treatment beobachteten liegen.

Die Ergebnisse unterstützen die Bias-Self-Management-These: Mehr als 70% der Teilnehmer tauschen ihr Los, also signifikant mehr als im *Basis*-Treatment (55%); statistisch unterscheidet sich das Ergebnis nicht von den 72%, die im *Herden*-Treatment ihr Los tauschen. Das spricht dagegen, dass die Probanden zufällig auf einen der Referenzpunkte festgelegt sind. Zudem reduziert sich das Reueempfinden der Probanden über einen Tausch des Loses signifikant im Vergleich zum *Basis*-Treatment. Im Einklang mit der Bias-Self-Management-These legen die Ergebnisse also nahe, dass die Probanden, wenn multiple Referenzpunkte präsent sind, ihre Aufmerksamkeit gezielt auf einen dieser Punkte fokussieren können. Das scheint ihnen zu erlauben, ihren Referenzpunkt strategisch so zu verschieben, dass sie eine bessere Entscheidung treffen können. Im Ergebnis sollten also professionelle Märkte mit ihrem Informationsangebot die Bedeutung von Besitzeffekten weiter abschwächen.

Damit haben wir jetzt Evidenz für eine ganze Reihe verschiedener Debiasing-Mechanismen und Debiasing-Strategien gesammelt: Wer Güter verkauft in einer strategischen Marktsituation verkauft, sollte keinen großen oder gar keinen Besitzeffekt aufweisen (Studie 1). Der Bias wird auch reduziert, wenn Externalitäten eines Trades den Fokus vom eigenen Besitzstatus auf den sozialen Kontext der Entscheidung lenken (Studie 2). Besitzeffekte werden neutralisiert, wenn ein Vertreter in eine Transaktion eingebunden ist, oder über einen Transfer innerhalb eines Gremiums entschieden wird (Studie 3). Schließlich kann das Verhalten anderer den Besitzstatus als Referenz verdrängen. Das führt dann zur Neutralisierung des Bias, wenn die Mehrheit der anderen Marktteilnehmer eine rationale Entscheidung trifft (Studie 4). Der Effekt kann also die Wirkung anderer Debiasing-Mechanismen verstärken.

Zugleich haben wir auch Evidenz für Debiasing-Strategien gesehen, also dafür, dass Individuen ihren Bias erkennen, und ihm begegnen, indem sie gezielt Debiasing-Mechanismen ausnutzen. Wir haben dieses Verhalten als Bias-

Self-Management bezeichnet. So binden Individuen gezielt Vertreter in ihre Transaktion ein, oder ziehen es vor, in einem Gremium zu entscheiden, um ihren Bias zu kontrollieren (Studie 3). Mit dem Herdenverhalten sehen wir nun ein zweites Beispiel für eine Debiasing-Strategie, die es Individuen erlaubt, ihren Referenzpunkt so zu verschieben, dass der resultierende Bias ihre Entscheidung in die Richtung einer rationalen Mehrheit drückt (Studie 4).

E. Self-Nudging (Chapter 6)

Die letzte Studie analysiert die dritte und anspruchsvollste Bias-Self-Management Strategie: Self-Nudging. Hier neutralisieren die Probanden ihren Bias nicht einfach, oder verschieben ihn, um eine bessere Entscheidung zu treffen, sondern sie nutzen ihre Verlustaversion produktiv als Mittel der Selbstbindung.

Als Beispiel analysiere ich Verträge, die eine Zahlung für ein bestimmtes Leistungsziel vorsehen. Wird das Ziel verfehlt, verliert die Partei entsprechend Teile der vorgesehenen Zahlung. Wird das Leistungsziel dagegen übertroffen, erhöht sich die Zahlung entsprechend. Leistung und Zahlung verhalten sich also linear zueinander. Weil die Probanden unterhalb des Leistungsziels Teile ihrer Vergütung verlieren, befinden sie sich also im Verlustframe, bis sie mit ihrer Leistung das vereinbarte Ziel erreichen.

Referenzpunkt kann auch eine Verdiensterwartung sein, wie etwa die Einnahmen, die ein Taxifahrer an einem bestimmten Tag zu verdienen erwartet. Hier ist also die vertraglich vereinbarte Leistung Referenzpunkt (zumindest solange die Partei erwartet, den Vertrag zu erfüllen). Für einen strikt rationalen Probanden ist der Referenzpunkt bedeutungslos, da die Vergütung ja linear ist. Ist ein Arbeiter aber verlustavers, dann wiegen Verluste relativ zum Referenzpunkt für ihn schwerer als Gewinne. Anschaulich lässt sich der Nutzen, den der verlustaverse Arbeiter aus seiner Leistung zieht, in zwei Teile zerlegen: den Konsumnutzen und den Gewinn-Verlust-Nutzen. Während der Konsumnutzen der Bezahlung unterhalb und oberhalb des Referenzpunktes identisch ist, ist der Gewinn-Verlust-Nutzen eines zusätzlich produzierten Stücks unterhalb des Referenzpunktes grösser. Der Arbeitsanreiz ist deshalb bei einem Verlustframing des Vertrages unterhalb des Referenzpunkts deutlich höher, als darüber. Das Verlustframing zieht also die Arbeitsleistung bis zum Referenzpunkt hoch und kann deshalb auch zur Selbstbindung eingesetzt werden.

Damit kann das Verlustframing seines Vertrages auch für den Arbeiter vorteilhaft sein. Die Literatur zeigt, dass Präferenzen, insbesondere wenn eine anstrengende, unangenehme Arbeit ausgeführt werden muss, häufig niedriger ausfallen, als vom Arbeiter selbst ex ante gewollt. Das Verlustframing des Vertrages erhöht nun die Kosten für den Arbeiter, sollte er sein ex ante (selbst-) gesetztes Ziel verfehlen. Ist sich der Arbeiter bewusst, dass er verlustavers ist und zeitinkonsistente Präferenzen bezüglich seiner Arbeit hat, dann könnte er gezielt Verträge mit einem Leistungsziel vereinbaren, das als Verlust geframt ist. Seine eigene Verlustaversion wird so zu einem Instrument der Selbst- bindung, dass ihm erlaubt, angestrebte Ziele zu erreichen. Ich spreche deshalb von Self-Nudging, der dritten Strategie von Bias-Self-Management.

Um zu testen, ob Individuen tatsächlich Verträge favorisieren, die sie durch ihre Konstruktion einem Verlust aussetzen, biete ich Probanden an, einen Vertrag über eine definierte Arbeitsleistung abzuschließen, die sie im Labor zu erbringen haben. Die Arbeitsaufgabe verlangt von den Probanden zu zählen, wie häufig eine bestimmte Ziffer in einer Box dargestellt wird. Die Box enthält insgesamt 200 Zahlen. Wenn die Probanden die Zahl korrekt angegeben haben, dann können sie die Aufgabe mit der nächsten Box fortsetzen, oder die Aufgabe abbrechen. Ist ihre Eingabe falsch, so müssen sie die Aufgabe wiederholen. Die Probanden können Vertrag und Aufgabe auch ganz ablehnen, bekommen dann aber auch keine Bezahlung.

Die Treatments variieren nun das Leistungsziel, dass der Vertrag definiert und setzen es entweder auf 5 Boxen (Low-Bar-Vertrag), 15 Boxen (Stretching-Vertrag) oder 50 Boxen (Extreme-Effort-Vertrag). Zudem wird ein *Basis*-Treatment angeboten, das keinerlei Leistungsziel vorsieht. Die tatsächliche Bezahlung bleibt dabei immer gleich: €1 für jede Box, die der Proband korrekt bearbeitet. Die Verträge mit Leistungsziel haben ein Verlustframing: Wenn der Proband das Leistungsziel verfehlt, erhält er eine Vertragsstrafe, das bedeutet, dass er relativ zu seiner Erwartung, den vereinbarten Lohn zu bekommen, einen Verlust erleidet und dementsprechend einen höheren Anreiz hat, das Leistungsziel zu erreichen. Oberhalb des Leistungsziel erhält er einen linearen Bonus. Da der Bonus relativ zur Erwartung einen Gewinn darstellt, nimmt der Anreiz oberhalb des Leistungsziels weiter zu arbeiten ab.

In dem zentralen *Self-Nudging*-Treatment können die Probanden zwischen den drei Verträgen mit vereinbartem Leistungsziel und dem einen Vertrag ohne Referenzpunkt auswählen. Um zu ermitteln, ob die Probanden eine Zahlungsbereitschaft für die Selbstbindung ihres zukünftigen Ichs haben, müssen sie eine Gebühr entrichten, wenn sie einen Vertrag mit Leistungsziel auswählen. Das Treatment erhebt auch die Leistungserwartungen, die die Probanden für die unterschiedlichen Verträge haben und natürlich ihre tatsächliche Produktivität.

Das Ergebnis der Studie zeigt, dass mehr als 80% der Teilnehmer einen Vertrag auswählen, der ein Leistungsziel definiert und sie damit einem Verlust aussetzt. Beinahe alle dieser Probanden erwarten dabei, dass sie unter dieser Vertragsgestaltung produktiver sein werden. Für den Vertrag mit Verlustframing, den sie als den für sich effektivsten ansehen, erwarten sie eine Produktivität von durchschnittlich 20.5 Boxen. Im Vergleich dazu liegt ihre Erwartung für den Vertrag ohne Leistungsziel, der sie keinem Verlust aussetzt, bei weniger als 12 Boxen, also eine signifikant niedrigere Zahl. Die Probanden *erwarten* aber nicht nur eine bessere Leistung ihres zukünftigen Ichs, sie sind auch *tatsächlich* signifikant produktiver, wenn sie einen Vertrag mit Verlustframing abschließen. Die Selbstbindung ist dabei sehr effektiv: Die tatsächlichen Leistungen der Probanden entsprechen beinahe vollständig ihren Erwartungen.

Direkte Evidenz für das Self-Nudging liefert ein Standardinstrument, das die Verlustaversion der Probanden misst. Um den Einfluss der Verlustaversion darzustellen, teile ich die Probanden in zwei Gruppen, diejenigen, die stärker verlustavers sind und die, die weniger verlustavers sind. Konsistent erwarten die stärker verlustaversen Teilnehmer, dass sie signifikant mehr von einem Vertrag mit Verlustframing profitieren werden, als Probanden, die entweder nur eine geringere oder gar keine Verlustaversion aufweisen. Verlustaverse Teilnehmer erreichen auch tatsächlich mit im Durchschnitt 22.6 Boxen eine signifikant bessere Leistung verglichen mit den 14.5 Boxen der anderen Teilnehmer. Wichtig ist insbesondere, dass die stärker verlustaversen Probanden auch Verträge mit höheren Leistungszielen wählen, als die weniger verlustaversen Teilnehmer und sie tun das, weil sie erwarten, dass sie unter diesen Verträgen ein besseres Ergebnis erreichen werden – was sie dann auch schaffen.

Das Ergebnis bestätigt zum einen, dass die verlustaverseren Probanden tatsächlich das stärkere Selbstbindungsinstrument zur Verfügung haben und zum anderen, dass die Probanden dies offenbar bei der Entscheidung, welchen der Verträge sie auswählen auch berücksichtigen. Sie treffen also eine reflektierte Entscheidung, ihre Verlustaversion zum Self-Nudging und damit zu ihrem eigenen Vorteil einzusetzen. So wandelt sich ihr Bias von einem kognitiven Fehler, der rationales Entscheiden gefährdet, zum produktiven Instrument eines verhaltensrationalen Individuums, das Entscheidungen und Handeln zu verbessern hilft.

Self-Nudging hat aber noch einen weiteren Vorteil: Es gewährt den Probanden Autonomie. Diese Entscheidungsfreiheit können sie zum einen nutzen, um den Vertrag auszuwählen, der ihren Präferenzen und ihrem Leistungsvermögen entspricht, sie also nicht etwa über- oder unterfordert und dadurch demotiviert.[16] Zudem fördert die Autonomie ihre individuelle Produktivität aber noch auf andere Weise, die nichts mit Kenntnis eigener Präferenzen und Fähigkeiten zu tun hat: Die Erfahrung von Selbstdetermination steigert ihre intrinsische Motivation. Um diesen Effekt zu demonstrieren, erhalten die Probanden in einer weiteren Studie zunächst Anreize dafür zu offenbaren, welchen der drei Verträge sie persönlich bevorzugen. Dann bilde ich zwei Gruppen. Die eine Gruppe von Probanden wählt aus den drei Verträgen denjenigen aus, den sie bevorzugen. Der zweiten Gruppe wird ein Vertrag zugelost. Die Hälfte der Probanden erhält dabei durch das Los denjenigen Vertrag, den sie auch bevorzugen. Sie lassen sich identifizieren, weil die Probanden haben ja vorab offenbart haben, welchen der Verträge sie präferieren. In diesem experimentellen Aufbau lässt sich nun die tatsächliche Produktivität der beiden Gruppen vergleichen; Die Hypothese lautet, dass die erste Gruppe, die ihren Vertrag autonom ausgesucht hat, intrinsischen Nutzen aus dieser autonomen Wahl d ableiten kann und aufgrund höherer Motivation produktiver sein wird, als die zweite Gruppe, der der von ihnen bevorzugte Vertrag zugelost worden ist.

Die Ergebnisse bestätigen die Hypothese und fallen sehr deutlich aus: In der Autonomiegruppe erreichen die Probanden (*N*=153) ein Ergebnis von 24.4 Boxen; in der Kontrollgruppe, in der die Probanden (*N*=144) keine Autonomie erfahren, findet sich dagegen nur eine durchschnittliche Produktivität von 17.0 Boxen (Mann-Whitney *p*-value <0.01). Es zeigt sich neben diesem Autonomie-Effekt auch ein Effekt privater Information: Die Probanden, die den von ihnen bevorzugten Vertrag per Los erhalten haben, bearbeiten mit ihren 17.0 Boxen signifikant mehr, als die Teilnehmer, denen ein beliebiger der drei verlustgeframten Verträge zugelost worden ist (*N*=131; 11.2 Boxen; Mann-Whitney *p*-value <0.01).

Die Studie zeigt damit, dass Selbstdetermination intrinsische Motivation erzeugt und dass dieser Effekt unabhängig davon besteht, dass Präferenzen entsprochen wird. Der Literatur ist dieser Nachweis bisher nicht gelungen, weil sie beide Effekte nicht strikt auseinandergehalten hat. So haben Autoren Probanden auf verschiedene Weise (sozialer Druck, starke Anreize oder schlicht durch Verkürzung ihres Entscheidungsspielraums) in ihrer freien Entscheidung eingeschränkt, um dann zu messen, dass ihre Produktivität durch die Einschränkung abgenommen hat. Das allerdings kann sowohl daran liegen, dass die Probanden in der Umsetzung eigener Präferenzen behindert wurden

[16] Auch Arbeitgeber können durch die Wahl der Arbeiter Verträge und Leistungsziele besser anpassen. Auch Arbeitgeber können durch die Wahl der Arbeiter Verträge und Leistungsziele besser anpassen. Allerdings wird dieser Vorteil der Nutzung privater Information in der modernen Arbeitswelt kleiner. Das Monitoring von Arbeitsabläufen verschafft Arbeitgebern diese Informationen meist ohnehin und erlaubt nicht nur Rückschlüsse auf Geleistetes, sondern immer präziser auch auf das, was individuell zu leisten möglich wäre.

als auch daran, dass die Manipulation die Erfahrung von Selbstdetermination beeinträchtigt hat.

Das Ergebnis unterstreicht die Attraktivität von Self-Nudging-Verträgen auch für Arbeitgeber. Bei gleichen finanziellen Anreizen waren die Probanden doppelt so produktiv verglichen mit einem Standardvertrag, der keine Möglichkeit zum Self-Nudging bietet.

III. Rechtspolitische und Dogmatische Schlussfolgerungen

Nun schauen wir uns einige rechtspolitische und dogmatische Fragestellungen an, bei denen Besitzeffekte eine Rolle spielen und analysieren, wie die von mir untersuchten Debiasing Mechanismen und Debiasing Strategien diese Diskussionen verändern können.

A. Debiasing

1. Zuweisung von Verfügungsrechten

Die Schaffung von Verfügungsrechten (Property-Rights) hängt häufig mit neuen Technologien zusammen; soll zum Beispiel Google das Recht an Daten zugewiesen werden, die Google Analytics sammelt, oder soll dem User das Recht als Teil seiner Privatsphäre zustehen? Ebenso ändern sich Einstellungen. War es lange akzeptiert, dass es in großen Metropolen wie New York laut ist, erhalten Anrainer nun Immissionsschutzrechte, um Beeinträchtigungen abzuwehren. Der Gesetzgeber weist dabei Verfügungsrechte auf verschiedenen föderalen Ebenen zu, aber auch Richter verankern sie im Common Law.

Das Coase-Theorem zeigt, dass es nur in einer Welt ohne Transaktionskosten gleichgültig wäre, wem diese Rechte zugewiesen werden. Vermeiden ließen sich reale Transaktionskosten und Pfadabhängigkeiten, könnte man die Verfügungsrechte direkt denjenigen zuweisen, die sie (zu diesem Zeitpunkt) am höchsten bewerten. Dass diese Simulation von Private-Ordering anspruchsvoll ist, ist offensichtlich. Zumindest braucht es dafür Instrumente, die die Wertschätzungen der potentiell Begünstigten messen und vergleichen können.

Der Schwierigkeit der Aufgabe angemessen, werden ganz unterschiedliche Methoden werden für diese Aufgabe verwendet: Revealed-Preference-Analysen untersuchen Marktdaten, um Zahlungsbereitschaften für die in Frage stehenden Güter und Rechte abzuleiten (stellvertretend Viscusi 1993, Freeman 2003). Werden Güter nicht an Märkten gehandelt, wie etwa die körperliche Integrität, lassen sich Zahlungsbereitschaften auch indirekt ermitteln, zum Beispiel die Zahlungsbereitschaft für Krankenversicherungen, Gesundheitsprodukte, oder Sicherheitsausstattungen usw. Die Zahlungsbereitschaft für saubere Luft oder Ruhe lässt sich am Immobilienmarkt durch Vergleich der Preise für Häuser ermitteln, deren Lage sich durch Geräusch- und Luftimmissionen unterscheidet, die aber ansonsten nach Alter oder Bausubstanz gleich zu bewerten wären (etwa bei Bateman et al. 2002, Fujiwara & Campbell 2011, Korobkin 2013). Der Erholungswert von Natur lässt sich mit Hilfe eines Reisekostenansatzes schätzen, der misst, wieviel Individuen für den Zugang zu Parks, Wäldern oder Seegebieten zu zahlen bereit sind. Bei anderen (öffentlichen) Gütern stößt die Methode aber an Grenzen. Wie sieht etwa die Zahlungsbereitschaft für die Existenz einer Spezies aus, oder für eine Naturschutzzone, die nicht betreten werden darf? Individuen können hier das Gut zwar wertschätzen, aber weil sie es nicht nutzen können oder dürfen, offenbaren sich ihre Präferenzen und Zahlungsbereitschaften nicht in ihrem Verhalten. Auch verkaufen Menschen solche (öffentlichen) Güter nicht, ihre willingness to accept (Kompensation, die sie fordern, um das Gut aufzugeben) lässt sich also kaum ermitteln und sie weicht in (bisherigen) Choice-Experimenten empirisch weit von der willingness to pay ab, also der Zahlungsbereitschaft für ein Gut, das man erst erwerben muss.

Diese Fragen haben in der Ökonomie die Entwicklung einer Survey-Methode vorangetrieben, der Conditioned-Valuation-Methode (Bateman et al 2002; Carson 2012, Fujiwara & Campbell 2011)[17], die es erlaubt neben Zahlungsbereitschaften auch WTAs für Güter und Rechte unter den verschiedensten Bedingungen und Kontexten abzufragen, ob sie an Märkten gehandelt werden oder nicht. Unten wird sich zeigen, dass gerade WTA-Bewertungen bei vielen Rechtsfragen eine wichtige Bedeutung zukommt. Zielgruppen, etwa Hauseigentümer, Internetnutzer oder Kreativschaffende usw. geben ihre WTA- und WTP-Bewertungen für bestimmte Verfügungsrechte an, also zum Beispiel ihre Zahlungsbereitschaft dafür, dass eine Fabrik ihr Grundstück nicht mit Immissionen belastet, oder umgekehrt, für welchen Preis sie bereit wären, solche Immissionen zu akzeptieren (siehe Jourden & Rachlinski 1998).

Der hypothetische Character dieser Survey-Technik führt allerdings zu einigen Problemen (Baron 1991), zum Beispiel überschätzen Befragte ihre Zahlungsbereitschaft, oder sie machen strategische Angaben. Experimentelle Forschung hat für einige dieser Probleme Lösungen entwickelt (Blumenschein et al. 2009), so werden Budgetgrenzen salient gemacht und die möglichen politischen Auswirkungen der Forschungsergebnisse verdeckt. Nicht gelöst ist bisher aber, wie sich WTA-Wertschätzungen unverzerrt von Besitzeffekten und emotionalen Transaktionskosten messen lassen (Kahneman, Knetsch & Thaler 2008, Daniel Fujiwara & Ross Campbell 2011). Einem Debiasing-Ansatz

[17] Und natürlich auch von Real-Choice-Studien, wie meinen Experimenten.

am nächsten kommen vielleicht List und Shogren (2003), die WTA-Preise rechnerisch um einen aus Metaanalysen abgeleiteten, durchschnittlichen Bias korrigieren wollen. Rechtspolitisch sind Besitzeffekte so relevant, weil WTA-Preise WTP-Preise um das Drei- bis Vierfache übersteigen können (Metanalysen von Horowitz & McConnell 2002, Sayman & Oncluer 2005, Tunçel & Kammitt 2011).

Die rechtliche Literatur diskutiert seit Jahrzehnten, ob WTP oder WTA-Wertschätzungen der Zuweisung von Rechten zugrunde gelegt und welche Preise dabei verglichen werden sollen. Einige wollen ausschließlich Zahlungsbereitschaften heranziehen, die auf tatsächlichem Verhalten gründen (etwa Viscusi 1993, Arrow 1993). Einkommenseffekte machen das aber problematisch (Frank 2000, Hovenkamp 1994). Stellen wir uns ein Kraftwerk vor, das Anwohner durch seine Emissionen stark belastet. Deshalb liegt die WTA der Anwohner dafür, dem Betreiber die Tätigkeit zu erlauben, deutlich höher als die WTA des Unternehmens dafür, die Produktion einzustellen. Allerdings hat das Unternehmen eine höhere Zahlungsbereitschaft für ein Weiterführen der Produktion als die Anwohner dafür, dass die Belastung unterbleibt, weil die Anwohner über die geringeren Geldmittel verfügen. Wird nur die WTP der Anwohner bei der Zuweisung der Abwehr- oder Verschmutzungsrechte berücksichtigt, drohen also Wohlfahrtsverluste. Immerhin lässt sich für Einkommenseffekte in Grenzen kontrollieren. So befragen Conditioned-Valuation Surveys Probanden über ihr Einkommen, ihren Beruf und Familie usw.; zum durchschnittlichen Einkommen relative Zahlungsbereitschaften lassen sich auch durch den Vergleich verschiedener Regionen abschätzen.

Andere Autoren halten aus normativen Gründen allein Zahlungsbereitschaften für relevant (Carson et al 2001, 2012). So sei die WTA nur der richtige Maßstab, wenn Verfügungsrechte schon existierten und abgegeben werden müssten, aber nicht, wenn sie erst geschaffen und zugewiesen werden. Unser Beispiel demonstriert jedoch auch, dass Eigentum immer ein Bündel aus zugeschnittenen Verfügungsrechten ist und je nachdem, welche Rechte das Bündel enthält, kann der Berechtigte eine bestimmte Belastung abwehren oder eben nicht. Wenn der Gesetzgeber z.B. Verschmutzungsrechte zuteilt, so kann der Anwohner eine sich im Rahmen dieser Rechte haltendende Immission nicht rechtlich, sondern nur durch eine Zahlung verhindern. Aber es ist natürlich wahrscheinlich, dass die WTA der Anwohner dafür, Immissionen zuzulassen, typischerweise deutlich höher ist, als die Zahlungsbereitschaft eines Dritten oder des Marktes. Die WTA als Maßstab macht deshalb auch bei der Schaffung von Rechten Wohlfahrtseffekte sichtbar.

Hovenkamp (1994) will deshalb Property-Rights sogar ausschließlich anhand der WTA der potentiell Begünstigten zuweisen. Das könnte auf den ersten Blick auch ein pragmatischer Weg des Umgangs mit Besitzeffekten sein:

Kein Debiasing wie ich es in meinen Studien entwickelt habe oder eine rechnerische Korrektur wie Showgren und List sie vorschlagen, sondern eine Art Konstanthalten des Bias zwischen den zu vergleichenden potentiell Begünstigten. Allerdings sind Besitzeffekte nicht homogen. Typischerweise zeigen Organisationen wie Firmen, juristische Personen, Körperschaften usw. wesentlich geringere WTA-Wertschätzungen als Individuen (Arlen, Spitzer & Talley 2002). Dieses Ungleichgewicht könnte zu Wohlfahrtsverlusten bei der Zuerkennung von Rechtspositionen führen. In unserem Fall etwa sollten die Besitzeffekte der Anwohner größer sein als die des Kraftwerksbetreibers. Zudem kann natürlich auch nur eine Partei das Verfügungsrecht bekommen, die andere hingegen erhält es nicht. Will man dieses Ergebnis vollständig bewerten, müsste die WTA-Wertschätzung des Begünstigten mit der Zahlungsbereitschaft dessen verglichen werden, der leer ausgeht. Die WTA beider potentiell Begünstigter zu vergleichen, bewertet also eine Situation, die durch die Zuweisung der Rechte gar nicht entstehen kann.

Besitzeffekte machen aber auch den direkten Vergleich von WTA mit WTP-Werten sehr schwierig. Weil sich WTAs auch in Surveys bisher nicht ohne Verzerrung durch Besitzeffekte und emotionale Transaktionskosten messen lassen (Kahneman, Knetsch & Thaler 1991, Fujiwara & Campbell 2011; auch in Real-Choice Experimenten ist das fraglich siehe oben), wird es aufgrund der Größe des Bias bei Messungen häufig dazukommen, dass die WTA beider potentiell Begünstigter die WTP des jeweils anderen übersteigt. Das führt zu dem konzeptionellen Problem, wer dann das Verfügungsrecht bekommen soll. Besitzeffekte, zumal hypothetische, die Artefakte der (Methoden der) Messung sind, sollten nicht darüber entscheiden, wer die Verfügungsrechte erhält.

Deshalb schlägt Kennedy (1981) vor, sowohl WTA als auch WTP-Bewertungen der potentiell begünstigten Parteien zu vergleichen. Stellen wir uns also zwei Parteien A und B vor, dann will er sowohl die hypothetische WTA der A mit der WTP der B, als auch die WTA B`s mit der hypothetischen WTP der A vergleichen. Pragmatisch soll dann der Durchschnitt von WTA und WTP-Preisen darüber entscheiden, wem die Rechte zuerkannt werden. Einen fundamentaleren Ansatz verfolgt Korobkin (1994): Er will nicht Transaktionenkosten vermeiden, indem Verfügungsrechte demjenigen zuerkannt werden, der sie am höchsten bewertet. Er will Besitzeffekte unterdrücken, indem er keine eindeutigen Property-Rights mehr zuerkennt, sondern verwischt, wem sie zustehen und wie weit sie reichen. In unserem Fallbeispiel von oben schlägt er also vor, nicht genau zu definieren, ob und ggf. wie viel das Kraftwerk emittieren darf, sondern er will nur einen weichen Standard setzen (keine unverhältnismäßige Belastung), der unklar lässt, wie weit das Abwehrrecht der Anwohner und wie weit das Verschmutzungsrecht der Fabrik reicht, wer also in einem konkreten Fall im Besitz des Verfügungsrechts ist. Er nimmt an, dass sich

Kraftwerk und Anwohner so leichter darauf einigen können, wie sie die Belastung verteilen wollen (Luftfilter für Kraftwerk oder Privathaus), auch um eine eindeutige und für die eine oder andere Seite teurere Gerichtsentscheidung zu vermeiden. Eine solche Unklarheit der Verfügungsrechte kann aber auch selbst Transaktionskosten verursachen (siehe dazu unten Bar-Gil & Engel 2020).

In meiner Arbeit habe ich stattdessen verschiedene Debiasing-Strategien entwickelt. Auf Grundlage meiner Ergebnisse ließen sich Surveys so framen, dass sich ein Einfluss von Besitzeffekten auf die Messung der WTA weitgehend reduzieren lässt. Bisher wird die WTA üblicherweise in einer Bewertungsaufgabe abgefragt. Die Befragten müssen also etwa angeben, in Fortführung des Beispiels von oben, für welchen Betrag sie bereit wären, die Immission des Kraftwerks zuzulassen (etwa Carsons 2012, Fujiwara & Campbell 2011). Stattdessen schlage ich ein Verhandlungsparadigma vor, das den Befragten aufgibt, sich vorzustellen, mit dem Betreiber über das Verschmutzungsrecht zu verhandeln, und dafür, dass sie die Belastung akzeptieren, einen Preis festzusetzen. Damit denken sie über Zahlungsbereitschaft und Präferenzen der anderen Partei usw. nach und ihr Aufmerksamkeitsfokus wird vom Besitzstatus abgelenkt. Wird ihre WTP abgefragt, so müssten sie überlegen, wieviel sie dem Betreiber anbieten würden, damit er die Belastung vermeidet. DS Lässt aber noch Raum für strategische Verzerrungen der WTA. Wie das zu lösen ist, sehen

Werden Verfügungsrechte durch Gerichte zugewiesen, finden hypothetische Valuationsmethoden unmittelbar Eingang in die Formulierung der Instruktionen, mit denen Richter die Jurymitglieder in ihrer Entscheidung anleiten. Die Instruktionen weisen dabei zwar die Besonderheit auf, dass die Jurymitglieder die Perspektive der Parteien übernehmen und sich in deren Situation versetzen sollen. An den zu erwartenden Biases ändert das aber nichts. So zeigen MacCaffery, Kahnemann, und Spitzer (1995), dass Juryinstruktionen einen starken Besitzeffekt in der Zuerkennung von Schadensersatz induzieren. Die Instruktionen leiten die Jurymitglieder dabei im Bewertungsparadigma an und fragen, für welche Summe das Jurymitglied als der Geschädigte bereit wäre, die Verletzung zu akzeptieren.

Stattdessen könnte das Gericht die Mitglieder der Jury anweisen, sich vorzustellen, mit dem Kraftwerksbetreiber über den Verkauf eines Verschmutzungsrechts zu verhandeln und festzulegen, welchen Preis sie als der Eigentümer des Grundstücks von dem Kraftwerksbetreiber für das Verschmutzungsrecht erzielen wollen. Dieser erste Teil der Instruktion minimiert zwar Besitzeffekte, die WTA kann aber, anders als bei einer reinen Bewertungsaufgabe, nun strategisch verzerrt sein.

Strategische Preise lassen sich aber in einem zweiten Schritt korrigieren: Wie in meiner ersten Studie gesehen, gibt es nämlich einen Spillover-Effekt;

wenn die Probanden zunächst ihre WTA für einen Verkauf der Lose im Verhandlungsparadigma bestimmen, dann zeigen sie im Anschluss auch dann keinen Besitzeffekt mehr, wenn in einem anreizkompatiblen Verfahren ihre tatsächliche WTA offenbaren sollen, wenn ihnen also eine Bewertungsaufgabe gestellt wird. Das Debiasing lässt sich damit erklären, dass sich der kognitive Fokus der Probanden offenbar nicht so schnell wieder ändert. Für die Formulierung der Juryinstruktionen ließe sich das nutzen, indem in einem zweiten Schritt die Jurymitglieder angeleitet werden, den minimalen Preis anzugeben, für den sie aus Sicht des Grundstückseigentümers bereit wären, das Verschmutzungsrecht zu akzeptieren. In diesem zweistufigen Design verzerren Besitzeffekte die letztlich auf der zweiten Stufe der Instruktion ermittelte WTA nicht mehr (Tontrup 2020). Hinzu kommt, dass Jurymitglieder im Verhandlungsparadigma (auf der ersten Stufe) sowohl die Rolle des Käufers als auch die des Verkäufers einnehmen, wenn sie WTA und WTP-Preise der beiden festlegen; diese Konfrontation mit einem alternativen Referenzpunkt sollte wie in meiner Bias-Shifting-Studie zu sehen (dazu unten mehr), Besitzeffekte ebenfalls reduzieren (so auch Zamir & Ritov 2010).

Denkbar wäre es auch, um emotionale Transaktionskosten gering zu halten, das Szenario in den Instruktionen um einen Vertreter zu ergänzen. Die Jurymitglieder würden dann aufgefordert sich vorzustellen, als Grundstückseigentümer mit dem Betreiber in Verhandlung zu stehen, und einen Vertreter anzuweisen, welchen Preis sie zu akzeptieren bereit wären.

Wenn Rechte nicht zugewiesen, sondern entzogen werden sollen, liegen die Probleme etwas anders. Darum geht es im nächsten Abschnitt.

2. Entziehung von Verfügungsrechten - Eminent Domain

Eine Eminent-Domain (Enteignung) entzieht bestehende Rechtpositionen, um Infrastrukturprojekte, die im öffentlichen Interesse stehen, umsetzen zu können. Hier gibt es drei Verfahrensabschnitte, für die die Messung der WTA und damit Besitzeffekte eine Rolle spielen. Zunächst ist festzustellen, ob die Umverteilung der Verfügungsrechte (wie oben ihre Zuteilung) die allokative Effizienz fördert; wenn dem so ist, muss eine Entschädigung festgelegt werden, über die Vorhabenträger und bisherige Eigentümer verhandeln; wenn sie sich nicht einig werden, entscheidet schließlich ein Gericht.

Wer Verfügungsrechte innehat, bewertet diese regelmäßig höher als ein Dritter. Viele Autoren nehmen deshalb an, dass nur dann echte Wohlfahrtsgewinne die Enteignung tragen, wenn die WTP derjenigen, die die Intervention begünstigen soll, die WTA der bisherigen Rechtsinhaber übersteigt (MacDonald & Bowker 1993; Radin 2003; Epstein 1985). Aufgrund des regelmäßig großen Unterschieds zwischen WTA- und WTP-Werten führt das zu einem starken Schutz bestehender Verfügungsrechte, mit Blick auf Besitzeffekte und emotionale Transaktionskosten aber auch potentiell zu Wohlfahrtsverlusten. Vor allem kann das bei größeren Projekten der Fall sein, bei denen sich die WTAs von vielen Beteiligten aufsummieren, wie ich in meiner Anticommons Studie analysiert habe. WTA-Wertschätzungen haben also eine noch hervorgehobenere normative Bedeutung bei der Umverteilung von Rechten[18]; für die Wohlfahrtsbewertung der Umverteilung stellen sich in Bezug auf Besitzeffekte aber letztlich die gleichen Fragen, die wir oben besprochen haben. Im Weiteren geht es deshalb um die Festlegung der Kompensation für die Eminent Domain.

Ist das Projekt als wohlfahrtssteigernd genehmigt, muss über die Entschädigung der bisherigen Eigentümer entschieden werden. Der Common-Law-Standard dafür ist die Just-Compensation (Armstrong v. United States), die sich als fairer Marktpreis versteht. Es handelt sich also grundsätzlich um einen WTP-Maßstab. "Fair" verweist dabei allerdings auf eine normative Korrektur: So soll nicht der wahrscheinlichste, sondern der bestmögliche am Markt zu erzielende Preis zu Grunde gelegt werden.[19]

Viele Autoren argumentieren aber trotzdem, dass die Entschädigung nach Marktpreis den Enteigneten nur unzureichend und deshalb ungerecht kompensiere, da ihm seine persönliche Wertschätzung für das verlorene Eigentum nicht ersetzt werde (Epstein 1985, Dana & Merrill 2002, Radin 1988, Fennell 2004, Fee 2006, Lee 2013). Es werden auch Public-Choice Begründungen (z.B. Bell & Parchomovsky 2007) dafür gegeben, die Enteigneten entsprechend ihrer WTA zu entschädigen.[20] Ungerecht sei eine Entschädigung nach Marktpreis zum Beispiel, wenn der Eigentümer schon lange auf seinem Grundstück gelebt habe, so dass eine besondere Bindung zu Land und Haus entstehe (u.a. Ellickson 1993, Epstein 2005, Radin 1988). Ein solches Attachment unterscheidet sich indes von Besitzeffekten. Zwar wird der langjährige Eigentümer das Land meist nur zu einem höheren Preis verkaufen wollen als ein Dritter. Dieser Unterschied zeigt sich aber nicht nur in der WTA, sondern auch in seiner Zahlungsbereitschaft. Stellen wir uns dafür den Eigentümer einer in der Familie vererbten Uhr vor. Hat er die Uhr verliehen und findet sie später in

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[18] Manche wollen die Verletzung der Autonomie des Eigentümers, er kann ja nur vor dem Hintergrund der Eminen-Domain entscheiden, ob er das Angebot des Vorhabenträgers annehmen will, nicht erst in die Kompensation, sondern schon in die Wohlfahrtsanalyse des Projekts einbeziehen (Radin 1988).

[19] Dabei gibt es viele Unterschiede zwischen den Jurisdiktionen, die großen Einfluss auf die tatsächliche Entschädigung haben.

[20] Andere wollen die Betroffenen auch entsprechend ihrer WTA entschädigen, dies jedoch aus Public-Choice Gründen. Farber (1992) etwa versteht die Entschädigung als Schutz vor Korruption. Die Nachteile der Enteignungen konzentrierten sich meist auf wenige Eigentümer, während der Nutzen sich auf viele Gruppen verteile. Das gebe den stark betroffenen Eigentümergruppen einen größeren Anreiz, unzulässigen Einfluss zu nehmen. Die Kompensation ihrer tatsächlichen subjektiven Wertschätzung soll diesen Anreiz beseitigen. Bell und Parchomovsky ergänzen, dass auch die Verwaltung korrumpiert und motiviert sein könne, durch Enteignung Renten abzuschöpfen. Durch die Kompensation der WTA sollen diese Renten verschwinden.

einem Laden angeboten, so wird er mehr für sie zu zahlen bereit sein als ein Dritter. Besitzeffekte sind dagegen an den Besitzstatus als Referenzpunkt gebunden. Ein Eigentümer bewertet die Uhr dann höher, weil und solange er ihren Besitz als Referenzpunkt für seine Bewertung verarbeitet; der Referenzpunkt kann sich aber mit der gestellten Aufgabe (Bewertungs- vs. Verhandlungsparadigma) oder dem Kontext jederzeit ändern, wie wir gesehen haben; die übersteigerte Wertschätzung verschwindet dann. Das gleiche gilt für emotionale Transaktionskosten, die verschwinden, wenn sich Verkäufer vertreten lassen und sich damit weniger Verantwortlichkeit für einen Trade zurechnen. Die Unterscheidung verdeutlicht, warum man die Entschädigung persönlicher Werte als angemessen und gerecht ansehen kann, sich zugleich aber in der Entschädigung einer durch Biases verzerrten WTA eine Überkompensation erkennen lässt.

Um die Kompensation zu begrenzen, wollen einige deshalb am Konzept der Fair-Compensation festhalten, aber ein in der Höhe begrenztes subjektives Premium anerkennen. So schlägt Epstein (1985) vor, die zusätzliche Entschädigung dürfe nicht 10% des fairen Marktwerts übersteigen, Lee (2013) will die Gesamtentschädigung auf 130% des Marktpreises begrenzen, andere wollen die subjektive Entschädigung in Jahren bemessen, die jemand auf dem Land und in der Community verbracht hat (Ellickson 1993).[21]

Auf Grundlage meiner Studienergebnisse könnte die Enteignung sich auch nach subjektivem WTA-Maßstab entschädigen lassen, ohne dass Besitzeffekte die Entschädigung übersteigern müssten. Schauen wir uns die Verfahrensschritte der Eminent-Domain noch einmal an, so macht der Vorhabenträger dem Eigentümer entsprechend einer Schätzung des Grundstückswertes (bisher je nach Jurisdiktion nach Maßgabe der Fair-Compensation mit oder ohne subjektives Premium) ein Kaufangebot und dann treten die Parteien in Verhandlung. Kommt es zu keiner Einigung, legt später eine Jury die Entschädigung fest.

Bei der Verhandlung zwischen den Parteien sollten, ganz unabhängig vom verwendeten Entschädigungsmaßstab, Besitzeffekte keine Rolle spielen (dazu die Ergebnisse meiner Anticommons-Studie). Schon das dogmatische Konzept des fairen Marktwertes lässt viel Spielraum für strategische Verhandlungen: So kann etwa der Marktwert gemäß der bestmöglichen Nutzung des Grundstücks in seinem gegenwärtigen Zustand geschätzt werden, aber es können auch lukrativere Nutzungen einbezogen werden, die zunächst Änderungen am Grundstück erforderten. Es stellen sich dann viele Schätzfragen, etwa

[21] Korobkin (1994) will prüfen, ob im konkreten Fall, deutliche Anzeichen für Einkommenseffekte oder für Besitzeffekte vorliegen, und dann je nachdem entweder WTP- oder WTA-Preise als Maßstab für die Wertschätzungen der Rechtsinhaber und potentiell Begünstigten heranzuziehen.

wie hoch und wie sicher der Ertrag der hypothetischen Nutzung wäre usw. Würde nach subjektiven WTA-Maßstab entschädigt, erweiterte sich der Verhandlungsspielraum noch zusätzlich.[22] Würde der Fall vor Gericht gehen, bliebe diese Unsicherheit über den Ausgang des Verfahrens für die Parteien natürlich bestehen, hinzu träten noch die Länge der Zeitverzögerung und die Gerichtskosten als weitere Unsicherheitsfaktoren. Zudem ist zu erwarten, dass die Eigentümer spätestens nach Anzeige des Vorhabens und vor Verhandlungsbeginn anwaltlich vertreten sind, was ihre Verantwortlichkeit aufteilen und dadurch Besitzeffekte und emotionale Transaktionskosten weiter reduzieren sollte (Ergebnisse meiner Vertreter-Studie). Daraus lässt sich schließen, dass auch bei Entschädigung eines subjektiven Premiums Besitzeffekte keine größere Rolle spielen und die Verhandlungen nicht eher scheitern sollten.

Interessanterweise könnte ein solches Verfahren, anstatt die Parteien durch Besitzeffekte in ihren Preisvorstellungen auseinanderzutreiben, sogar leichter zu einer Einigung führen. So haben Nadler und Diamond (2008) gezeigt, dass der Marktpreis typischerweise als unfairer Ausgleich empfunden wird und es deshalb zu deutlichen Verzögerungen und Kostensteigerungen im Verfahren kommen kann. Sie argumentieren, dass ein Verfahren, das expressiv subjektive Werte anerkennt und entschädigt, eher zu einer Einigung führen könnte, weil Eigentümer den Verkauf als gerechter empfinden (siehe dazu auch allgemein Tyler 2006). Das könnte auch dann noch gelten, wenn die Entschädigung des subjektiven Premiums abstrakt in der Höhe begrenzt wird.

Geht das Verfahren vor Gericht, legt eine Jury die Kompensation fest. Die Jury-Instruktionen sollten für diese Entscheidung – wie oben gesehen - nicht im Bewertungs-, sondern im Verhandlungsparadigma gestaltet werden. Die Instruktionen leiten dann die Mitglieder an, sich eine hypothetische Verkaufssituation vorzustellen und zu überlegen, wie sie als Eigentümer mit Interessenten auf Grundlage der vorliegenden Gutachten verhandeln, und welchen Preis sie von einem Käufer dabei erzielen wollten (zum Vergleich kommentierte Jury-Instruktion für Eminent Domain in North Dakota: Lynch 1974; aktuelle Version unten[23]). Im zweiten Schritt würden die Jurymitglieder aufgefordert, den minimalen Preis festzulegen, zu dem sie als Eigentümer bereit wären, an den Vorhabenträger zu verkaufen. Dieser Preis präsentiert dann die unverzerrte WTA.

[22] Der Eigentümer kann versuchen auszunutzen, dass für das Projekt sein Land benötigt wird (holdout). Umgekehrt kann die Behörde einen niedrigen Anker für die Verhandlungen setzen wollen.

[23]https://govt.westlaw.com/wciji/Browse/Home/Washington/WashingtonPatternJuryInstructionsCivilCriminal/WashingtonPatternJuryInstructionsCivil?guid=Ida26d520a96511da8de7aa0ea4dc7e9b&bhcp= 1&bhhash= 1&transitionType=Default&contextData=%28sc.Default%29#I2cd1276be10d11dab058a118868d70a9.

Dieses Framing der Instruktionen sollte verhindern, dass Besitzeffekte die Entscheidung relevant beeinflussen, weil es den Fokus der Jurymitglieder vom Besitz als Referenzpunkt ablenkt.[24] Zugleich wird im zweiten Schritt für strategisches Überpreisen kontrolliert. Eine persönliche Bindung an Land und Community, die neben der WTA des Eigentümers auch seine WTP steigert (Strahilevitz & Loewenstein 1998), wird in der Entschädigung hingegen berücksichtigt (Tontrup 2020).

Die von mir entwickelten Debiasing-Strategien können damit helfen, die Nutzung des WTA-Maßstabs bei Zuweisung und Umverteilung von Rechten zu rationalisieren, vor allem auch in der Juryanwendung. Zudem stärken die normativen Positionen, die eine Entschädigung der subjektiven Wertschätzung bei der Eminent Domain für angemessen oder aus Public-Choice Gründen für geboten halten, da Verzerrungen durch Besitzeffekte und emotionale Transaktionskoten reduziert werden.

3. Der Schutz von Verfügungsrechten

Bewertungsmaßstäbe spielen auch bei der Frage eine Rolle, wie Rechte geschützt werden sollen. Wird eine Rechtsposition durch eine Property-Rule geschützt, so kann sie nur mit Zustimmung des Rechtsinhabers übertragen oder genutzt werden. Er kann also Aneignung oder Nutzung seines Rechtsguts verhindern. Liability-Rules schützen dagegen Eigentumsrechte nicht absolut, ein Interessierter kann sich ein Rechtsgut auch ohne Erlaubnis des Inhabers aneignen. Er muss dann aber Schadensersatz leisten, den ein Gericht festlegt, wenn die Parteien sich nicht selbst einigen.

Wir haben oben gesehen, dass Besitzeffekte Parteien daran hindern können, objektiv effiziente Transaktionen zu erkennen. Da unter einer Property-Rule der Rechtsinhaber in die Übertragung seiner Rechtsposition einwilligen muss, können also beidseitig vorteilhafte Transaktionen unterbleiben. Die Literatur hat deshalb überlegt, ob Besitzeffekte unter einer Liability-Rule die allokative Effizienz weniger behindern (Korobkin 2003, 2013). Unter einer Liability-Rule kommt es zunächst ja nur darauf an, ob der Interessierte selbst den Nutzen, den er aus der Aneignung der Rechtsposition ziehen will, höher bewertet als die Entschädigung, die er im Gegenzug zu zahlen hätte. In Fällen, in denen eine Transaktion objektiv effizient ist, sollte die Rechtsposition unter der Liability-Rule also tatsächlich übergehen, weil sie sich derjenige, der sie höher bewertet, selbst aneignen kann. Allerdings müsste er dann, wenn Besitz-

[24] Auch Self-Assessment Methoden wie von Levmore und Fenell (mit Einschränkung auch Bell & Parchomovsky) vorgeschlagen, führen dazu, dass der Eigentümer bei Festlegung seiner Wertschätzung berücksichtigen muss, was er in einer alternativen Markttransaktion erhielte. Auch er sieht sich also strategischen Anreizen gegenüber, die Besitzeffekte reduzieren sollten.

effekte die Pareto-Verbesserung verdecken, den Weg über die Gerichte beschreiten, was aufgrund der Verfahrenskosten (der Schadensersatz muss ermittelt werden) die geplante Nutzung ineffizient machen kann. Dazu trägt er das Risiko, dass mehr Schadensersatz zugesprochen wird, als er erwartet (Polinsky 1980).

Die Vorteile der Liability-Rule wären deshalb deutlicher, wenn ein Liability-Regime Besitzeffekte im Vergleich zur Property-Rule auch abschwächen würde. Das versuchen Jourden und Rachlinsky (1998) zu belegen. In ihrer Studie sollen sich die Probanden in die Rolle einer Umweltschutzorganisation versetzen. Im ersten Treatment geben sie ihre WTA dafür an, ein Wattgebiet an ein Unternehmen zu verkaufen, das es durch die geplante Nutzung zerstören würde. Im zweiten Treatment zeigen sie ihre Zahlungsbereitschaft dafür an, das Wattgebiet zu kaufen, um seine Zerstörung zu verhindern. Nun variieren die Autoren in beiden Treatments, wie das Watt geschützt ist, einmal absolut durch eine Property-Rule, oder durch eine Liability-Rule, was die Aneignung des Wattgebiets bei Entschädigung erlaubt.

Im Ergebnis finden Jourden und Rachlinsky keinen Unterschied zwischen WTA- und WTP-Preisen unter dem Liability-Regime, während sich unter der Property-Rule ein deutlicher Besitzeffekt zeigt. Sie begründen ihr Resultat damit, der Eigentümer wisse, dass er seine Verfügungsrechte unter dem Liability-Regime verlieren oder ein anderer das Eigentum mit einer Externalität belasten könne. Dadurch reduziere sich die Bindung an das Rechtsgut. Die Autoren nehmen deshalb an, unter dem Liability-Regime solle es häufiger zu objektiv effizienten Transaktionen kommen und schlagen deshalb vor, wenn Besitzeffekte ausgeprägt sein könnten, Eigentumsrechte nur durch eine Liability-Rule zu schützen. In dieser Überlegung sind ihnen Autoren in Rechtsgebieten gefolgt, die eine stark verzerrte WTA erwarten lassen (Buccafusco & Sprigman 2011 sog. Kreationseffekt des Urhebers).

Ich vermute zwar ebenso wie Jourdan und Rachlinsky, dass Liability-Rules den Einfluss von Besitzeffekten beschränken können, dies aber aus anderem Grund. Meine Arbeit zeigt wie oben besprochen, dass Vertragsverhandlungen den Aufmerksamkeitsfokus des Rechtsinhabers von Gut und Besitzstatus ablenken. Nun ist anzunehmen, dass unter einer Liability-Rule die Wahrscheinlichkeit, dass über einen möglichen Trade verhandelt wird, höher ist, da eine andere Partei sich das Gut auch ohne Zustimmung des Inhabers aneignen und nutzen kann. Will er seine Verfügungsrechte also behalten, müsste der Inhaber die andere Partei durch eine Zahlung von Aneignung und Nutzung abhalten (Ayres & Talley 1995). Damit hat er ein stärkeres Interesse, eine Einigung mit der Gegenpartei zu erreichen, als unter einer Property-Rule. Da sich durch die Tradingaufgabe Besitzeffekte reduzieren, sollten sich die Parteien also unter einem Liability-Regime häufiger einigen.

Das sagt aber noch nichts darüber aus, wie groß der Unterschied in der Prävalenz von Besitzeffekten unter den beiden Regelungstypen tatsächlich ist. So reduzieren die Mechanismen, die ich analysiert habe, Besitzeffekte natürlich auch unter einer Property-Rule. Allerdings könnte sich der Inhaber hier passiver verhalten, da die Property-Rule ihn nicht herausfordert, zu verhandeln und sich zu einigen. Meine Daten zeigen aber auch, dass Inhaber in der Lage sind, ihren Bias selbst zu erkennen und ihr Entscheidungsumfeld so zu arrangieren, dass sie ihren Besitzeffekt neutralisieren können (These des Behavioral-Self-Managements). Sie lassen sich zum Beispiel beraten, um ihre emotionalen Transaktionskosten zu senken, oder verschaffen sich Informationen, um aktiv ihren Referenzpunkt so zu verschieben, dass er ihre Entscheidung in die gewünschte Richtung lenkt.

Ob Liability-Rules also wirklich einen relevanten Vorteil im Hinblick auf Besitzeffekte und allokativer Effizienz von Private-Ordering haben, müsste noch genauer untersucht werden. Dabei wären auch die Effizienzverluste zu berücksichtigten, die aus einem schlechteren Schutz von Besitzinteressen resultieren (Rose 1997).[25]

4. Schadensersatz

Jurytrials sprechen den Inhabern eines verletzten Rechtsguts Schadensersatz zu. Um die Höhe des Schadensersatzes zu bestimmen, formulieren Richter fallbezogene Instruktionen, die die Mitglieder der Jury anleiten sollen. Wie schon oben gesehen, diskutiert die Literatur, nach welchem Maßstab der Schaden ausgeglichen werden und wie der Richter die Instruktionen framen sollte. McCaffery, Kahneman und Spitzer (1995) etwa zeigen in einer experimentellen Studie, dass Probanden in der Rolle eines Jurymitglieds für eine erlittene Rechtsverletzung ganz unterschiedlich hohe Schadensersatzbeträge zusprechen, je nachdem welchen Maßstab die Instruktionen verwenden: Instruktionen, die der "Make Whole" Doktrin folgen, fragen das Jurymitglied, welcher Schadensersatz zu leisten sei, um den Geschädigten so zu stellen, als wäre sein Rechtsgut nicht verletzt worden. Der Fokus liegt hier also auf einer Zahlung (WTP), die nach der Verletzung bestimmt wird. Die alternative Formulierung

[25] Kaplow & Shavell (1996) sehen bei Liability- und Property-Rules relative Effizienzvorteile bei unterschiedlichen Regulierungsaufgaben. Liability-Regime seien vorteilhaft, wenn Externalitäten eine große Gruppe Betroffener beeinträchtigen, oder bei der Haftung für Unfälle, wenn sich ex ante die Menge potentiell Beteiligter nicht eingrenzen lässt. Ein anderes Beispiel sind unklare Property-Rights (siehe aber Bar-Gil & Engel 2016). In diesen Fällen wären die Kosten für bilaterale Transaktionen erheblich, oder schon die Vertragspartner nicht identifizierbar.
Wenn es dem Recht hingegen darum geht, Besitzinteressen zu schützen, dann seien Property-Rules effizienter. Da der Schutz unter Liability-Rules nicht vom Einverständnis des Eigentümers abhängt, könnte der Inhaber andere von der Nutzung seines Gutes abhalten müssen. Unkalkulierbare Kosten und drohende Besitzwechsel untergraben dann einen wesentlichen Zweck des Eigentumsschutzes, Anreize zu geben, Arbeit und Ressourcen in ein Gut zu investieren, um es zu optimal zu nutzen (Rose 1994). Parteien könnten sich auch Güter wiederholt voneinander aneignen (Bar-Gil & Engel 2020).

fordert die Probanden auf, festzulegen, für welchen Betrag der Geschädigte bereit gewesen wäre, die Verletzung des eigenen Rechtsguts zuzulassen (WTA). Im Ergebnis sprachen die Versuchspersonen dem Geschädigten in der WTA-Fassung der Instruktionen einen vierfach höheren Schadensersatz zu. McCaffery, Kahneman und Spitzer halten die Restitutionszahlungen (WTP) für den richtigen Maßstab, da die Inhaber der verletzten Rechtsgüter andernfalls mehr erhielten, als sie bei einer Markttransaktion verlangt hätten.

Die Literatur hingegen lässt das Argument der Autoren zwar bei einer Vertragsverletzung gelten, nicht aber bei einem deliktisch verursachten Schaden. Sie unterscheidet denn auch je nach der Natur der Ansprüche, welche Maßstäbe für effizient und angemessen erachtet werden. Für eine Strict-Liability-Haftung etwa soll eine Kompensation nach dem WTA-Standard erfolgen, da dem Geschädigten in Bezug auf die gefährliche Handlung das Recht an der Unversehrtheit seiner Rechtsgüter zugewiesen sei (Arlen 1985, Epstein 1994, 1995). Vor allem für Schäden an der körperlichen Unversehrtheit sehen das viele auch dann so, wenn die Haftung an die Verletzung von Sorgfaltsmaßstäben geknüpft ist, denn der deliktisch Handelnde nehme dem Verletzten die Möglichkeit zu entscheiden, welches Risiko er für seine Gesundheit zulassen will (Korobkin 1998). Ähnlich wird argumentiert, dem Geschädigten würde neben der physisch-materiellen Beeinträchtigung auch ein psychologischer Schaden zugefügt, da der Schädiger seine Rechte missachte (Evidenz dafür Depoorter & Tontrup 2022). Die WTP hingegen sei ein geeigneter Maßstab, wenn der Schaden durch beidseitig riskantes Verhalten von Schädiger und Geschädigtem entstanden sei (Geistfeld 1995) und im Bereich der Produkthaftung, weil die Geschädigten mit ihrer Zahlungsbereitschaft selbst eine Entscheidung über die relative Sicherheit ihrer Produkte getroffen hätten (Arlen 1992; Geistfeld 1988).

WTA-Preise werden also vor allem bei deliktischem Handeln als gerechter Ausgleich immaterieller und besonderer materieller Schäden angesehen. Dabei könnten Besitzeffekte zu einer ungewollten Überkompensierung des Geschädigten führen was, wie gesehen, von McCaffery, Kahneman und Spitzer und anderen kritisiert wird. Denn natürlich können auch Jurymitglieder diesen Bias haben. Wenn sie also Schadensersatz festlegen und sich dabei in den in seinen Rechtsgütern Verletzten hineinversetzen sollen, dann finden diese Besitzeffekte und emotionalen Transaktionskosten auch Eingang in die WTA, die sie festlegen. Wie die experimentelle Evidenz von McCaffery, Kahneman & Spitzer (2003), die ich unten noch genauer bespreche, zeigt, können diese emotionalen Kosten den Schadensersatz, den die Jury zuspricht, leicht verdreifachen.[26]

Aus meiner Sicht kommt es für ein Debiasing wieder darauf an, wie die Instruktion für die Jurymitglieder gestaltet sind. So sollte die Jury, wenn sie nach WTA-Maßstab entscheidet, den Schadensersatz wieder im Verhandlungsparadigma festlegen. Damit sollten Besitzeffekte die Jury in ihrer Entscheidung nicht wesentlich beeinflussen (im Gegensatz zu McCaffery, Kahneman & Spitzers Studie). Im Falle einer Urheberrechtsverletzung zum Beispiel, könnte das Gericht die Mitglieder der Jury anleiten, sich vorzustellen, als der Geschädigte mit Verlagen über den Verkauf von Verwertungsrechten zu verhandeln und zu überlegen, welchen Preis sie für die Verwertungsrechte vom Verlag erzielen wollen. Im zweiten Schritt sollte wieder gefragt werden, zu welchem minimalen Preis das Jurymitglied als Geschädigter verkaufen würde. Wie schon gesehen sollte das Verhandlungsparadigma Fokus und Aufmerksamkeit vom Recht und dessen Verletzung zu den potentiellen Lizenznehmern und ihrer Zahlungsbereitschaft für die Lizenz verschieben; für strategische Effekte sollte die zweite Abfrage der minimalen WTA kontrollieren.

Der von mehreren Autoren angesprochenen psychologische Schaden durch Verletzung oder Missachtung eigener Rechte sollte besser als eigenständige dogmatische Kategorie behandelt und die Jury dafür mit einer separaten Instruktion angeleitet werden.

5. Beweislast

Ähnliche Fragen werden auch zur Beweislast diskutiert. Wir stellen uns zwei Parteien vor, die darum streiten, wem eine Sache zusteht. Nun möge A die B auf Herausgabe der Sache verklagen. In diesem Fall hat vor Gericht die Beklagte die Verfügungsgewalt über die Sache inne. Zamir und Ritov (2012) zeigen nun in mehreren Studien, dass die bestehenden Besitzverhältnisse erheblich beeinflussen, wem Jurymitglieder die Sache zusprechen. Obwohl die Probanden wissen, dass sowohl Bestätigung wie Ablehnung des Anspruchs nur ein einfaches Überwiegen der Gründe verlangen (Preponderance of Evidence), sind ihre Jus studierenden Probanden nur bereit, dem Herausgabespruch stattzugeben und damit den Besitzstatus zu verändern, wenn ganz überwie-

[26] Damit der Schadensersatz nicht durch Besitzeffekte aufgebläht wird, will Korobkin (2013) das Gericht je nach Fall entscheiden lassen, welcher Maßstab anzuwenden ist. Zeigt sich ein ersatzfähiger subjektiv geprägter Schaden, so soll der WTA-Maßstab angewendet werden, andernfalls die WTP. Besitzeffekte finden so zwar nicht Eingang in den Schadensersatz, solange dieser nach WTP-Maßstab festgelegt wird; entscheidet der Richter aber, dass ein ersatzfähiger subjektiver Schaden vorliegt, so würde die Jury nach WTA-Maßstab entscheiden und Besitzeffekte würden die Entscheidung beeinflussen.

gende Gründe für den Anspruch des Klägers sprechen. Sie messen diese Divergenz zwischen Maßstab und Entscheidung, indem sie die Probanden die Beweiskraft zunächst nur auf einer Skala bewerten lassen, so dass Werte über 50 eine überwiegende Evidenz anzeigen. In einem zweiten Treatment lassen sie andere Probanden entscheiden, ob dem Herausgabeanspruch stattzugeben ist oder nicht. Den Grund für den Bias sehen Zamir und Ritov darin, dass ihre Probanden den Status Quo vor Prozessbeginn als Referenzpunkt verarbeiten (ähnlich Rachlinski 1996, Guthrie 2003) und deshalb aus ihrer Sicht der Beklagten einen Schaden zufügen, wenn sie den Herausgabeanspruch als begründet ansehen. Der Klägerin verweigern sie hingegen relativ zum Referenzpunkt nur einen Vorteil. Damit verknüpft sich, dass die Mitglieder der Jury passiv bleiben, wenn sie den Status quo nicht verändern, und sich deshalb für eine möglichweise falsche Entscheidung weniger Verantwortung zuschreiben (Baron & Ritov 2003).

Normativ wollen Zamir und Ritov nicht den möglichen Bias in der Entscheidung korrigieren, sondern im Gegenteil dogmatisch das Beweismaß im Hinblick auf die Verlustaversion der Parteien anpassen. Weil ein Schaden für die Beklagte schwerer wiege, da sie die Herausgabe der Sache als Verlust betrachte, während die Klägerin die Herausgabe als Gewinn auffasse, solle der Klage nur bei deutlich überwiegender Evidenz stattgegeben werden. Ansätze, den Referenzpunkt zu verwischen, indem der Richter in den Juryinstruktionen betont, die Sache unterstehe während des Prozesses (quasi) der Verfügungsgewalt des Gerichts, haben den Bias der Jurymitglieder nicht eliminieren können (Korobkin 2013).

Meine Studien zeigen, dass Probanden, mit einem alternativen Referenzpunkt konfrontiert, ihren Bias verlagern. Statt auf ihren Besitzstatus fokussieren sie zum Beispiel auf Marktakteure, die ihre Assets verkaufen, und vollziehen durch den Wechsel des Referenzpunkts ein für sie vorteilhaftes Rechtsgeschäft. Dieses Ergebnis legt nahe, dass richterliche Instruktionen den Jurymitgliedern einen alternativen Referenzpunkt präsentieren, um ihnen zu helfen, ihren Bias zu korrigieren. So könnten die Instruktionen die Rolle der Verfahrensbeteiligten als Kläger und Beklagter und damit ihren Besitzstatus variieren. Die Jury würde aufgefordert, auf Grundlage der Beweise, die je für den A und den B als rechtmäßigen Eigentümer sprechen, den Fall einmal mit A als Kläger und B als Beklagtem mit dem Gut im Besitz, zu bewerten, und einmal mit vertauschten Rollen. Damit wären die Jurymitglieder besser in der Lage, die Beweise, die die Parteien vorbringen, von den unterschiedlichen Referenzpunkten abzulösen und zu bewerten. Einen ähnlichen Vorschlag machen Roese und Vohs (2012) für die Korrektur des Hindsight-Bias.

Eine weitere Ursache für die verzerrte Gewichtung der Beweise sehen Zamir und Ritov im Omission-Bias, dem aber nach meinen Ergebnissen in Juryentscheidungen damit begegnet wird, dass die Mitglieder der Jury gemeinsam entscheiden, ihre Verantwortlichkeit also teilen. Die Probanden in Zamir und Ritovs Studie haben dagegen allein entschieden.

Ihren dogmatischen Vorschlag, das Beweismaß zugunsten des Besitzers zu verschieben, stützen Zamir und Ritov auf die angenommene Verlustaversion der Parteien. Meine Studien zeigen aber auch, dass Verlustaversion sich auf ganz verschiedene Referenzpunkte beziehen kann, und deshalb sehr veränderlich ist. So könnte auch der Kläger, wenn er sich als rechtmäßiger Eigentümer sieht, Verlustaversion bzgl. der Sache empfinden. Der Referenzpunkt der Verfahrensbeteiligten könnte sich auch jederzeit während des Prozesses verschieben, je nachdem, welchen Ausgang sie für sich erwarten (zu Erwartungen als Referenzpunkt meine Self-Nudging Studie und Kozsegi & Rabin 2006, Abeler et al. 2011). Eine zwingende und abstrakte Begünstigung des Besitzers, etwa auch wenn er die Sache nur kurz im Besitz oder selbst Zweifel an seiner Berechtigung hatte, macht deshalb normativ wenig Sinn. Stattdessen sollte es – wie oben skizziert - das Ziel sein, die Juryentscheidung so anzuleiten, dass der Bias korrigiert wird. Der Ausgleich zwischen Besitzinteressen und Eigentum lässt sich zudem angemessener im materiellen Recht zum Beispiel mit Zulässigkeit und Grenzen der Ersitzung regeln.

6. Besondere Besitzeffekte – Dispositive Regelungen

Die meisten Bestimmungen des Vertragsrechts sind dispositiv, Parteien können also von ihnen abweichen. Wenn sich beide Parteien auf eine abweichende Regelung einigen, führt das unter Rationalitätsannahmen zu einer Pareto-Verbesserung. Das dispositive Recht sollte also nur dann Eingang in einen Vertrag finden, wenn die Parteien keine Regelung identifizieren, die sie zu einer Pareto-Verbesserung führt.

Besitzeffekte und emotionale Transaktionskosten können nun Pareto-Verbesserungen maskieren (siehe oben), so dass Vertragsverhandlungen unterbleiben. Korobkin (1998, siehe auch Marcin & Nicklisch 2017) etwa zeigt in mehreren Experimenten, dass dispositives Recht Vertragsinhalte beeinflussen und Pfadabhängigkeit schaffen kann. In seinen Studien verarbeiten die Parteien die dispositiven Regelungen als Referenzpunkt, so dass die von den Bestimmungen bessergestellte Vertragspartei selbst dann nicht von ihnen abweichen will, wenn eine andere Klausel vorteilhaft für sie wäre. Korobkin nimmt an, dass die Probanden ein Aufgeben der dispositiven Regelung als Verlust auffassen und sie deshalb in Verhandlungen als wertvoller gewichten. Objektive Pareto-Verbesserungen werden deshalb nicht immer bewirkt.

Um diesen Effekt sichtbar zu machen, legt Korobkin seinen Probanden einen Vertrag mit zwei Vertragsklauseln A und B vor, wobei A die eine und B die andere Vertragspartei begünstigt. Er vergleicht zwei Treatments: Im ersten bezeichnet er A als dispositive Bestimmung, im zweiten B. Nun fragt er die von der dispositiven Regelung begünstigte Partei, für welchen Betrag sie bereit wäre, die alternative Klausel zu akzeptieren; die andere Partei fragt er, welchen Betrag sie zu zahlen bereit wäre, die für sie günstigere, alternative Klausel zum Vertragsinhalt zu machen. Es zeigt sich, dass die Probanden eine Klausel dann höher bewerten, wenn das Treatment sie als "dispositive Bestimmung" ausweist. Im Ergebnis wird also zu selten von der dispositiven Regelung abgewichen, es gibt Pfadabhängigkeiten und Effizienzverluste.

Rechtspolitische Vorschläge, um diese Effizienzverluste zu vermeiden, gibt es viele: Zunächst wird vorgeschlagen, wenn das vertraglich zu regelnde salient erscheint, keine dispositiven Regelungen vorzugeben, sondern nur die Gegenstände zu benennen, die ein gültiger Vertrag festzulegen hat. Wenn diese Regelungen fehlen, soll der Vertrag nicht qua Recht durchsetzbar sein. Damit soll vermieden werden, dass die dispositiven Regelungen einen Status quo schaffen (Ayres & Gertner 1989). Ein zweiter Vorschlag will das dispositive Recht konkretisieren und so deutlicher auf bestimmte Situationen zuschneiden. Wenn die Parteien sich nicht auf eigene Klauseln einigen, sollen so nicht allgemein gehaltene dispositive Regelungen, die für die Parteien in ihrer Situation wenig effizient erscheinen, Vertragsinhalt werden, sondern Bestimmungen, die ihrem Regelungsbedürfnis besser entsprechen (Korobkin 2009). Willis (2013) wiederum will, um die Effizienz dispositiven Rechts zu beurteilen, auch danach fragen, welche Folgen es erzeugt, sollten die Parteien das Recht auch dann nicht ersetzen, wenn es in ihrer Lage ineffizient ist. Jolls, Sunstein und Thaler (1997) gehen noch einen Schritt weiter und sehen die Funktion von dispositivem Recht aufgrund seiner "Stickiness" nicht mehr darin, Verträge zu komplementieren oder Transaktionskosten zu sparen, sondern als Steuerungsinstrument, als Nudge. Wenn der Gesetzgeber bestimmte Rechtsgüter fördern wolle und aus seiner Sicht diesem Zweck bestimmte vertragliche Regelungen besser gerecht würden als andere, dann solle er diese als dispositives Recht verankern. Da anzunehmen sei, dass die Parteien die Regelung zumeist nicht ersetzten, werde sie typischerweise Vertragsinhalt und fördere damit das Rechtsgut. Die dispositive Natur der Bestimmungen sichere zugleich, dass die Präferenzen der Parteien berücksichtigt blieben.

Meine Ergebnisse lassen vermuten, dass es auf Kontext und Akteure ankommt, inwieweit Besitzeffekte oder emotionale Transaktionskosten eine Ursache für eine "Stickiness" dispositiver Bestimmungen sein können. Wenn die Parteien aushandeln, welche Regelungen Eingang in ihren Vertag finden sollen, stellt sich ihnen eine strategische Aufgabe; sie müssen abschätzen, ob die Gegenpartei eine bestimmte Regelung akzeptieren wird und was sie im Gegenzug verlangen könnte. Die strategische Aufgabe sollte also ihren Aufmerksamkeitsfokus auf ihren Verhandlungspartner lenken und Besitzeffekte reduzieren. Besitzeffekte am dispositiven Recht sollten auch gering ausfallen, wenn die Parteien in ihren Vertragsverhandlungen vertreten werden. Korobkins Ergebnisse widersprechen dem nicht, denn er hat seinen Probanden eine reine Bewertungsaufgabe gestellt; sie sollten nur den Wert, den die dispositive Regelung für sie hat, anzeigen. Sein Studiendesign fokussiert die Probanden also auf den Status quo.

Das heißt aber nicht, dass dispositives Recht keinen Einfluss auf Vertragsinhalte hat. Bei alltäglichen Geschäften werden Parteien selten verhandeln oder sich vertreten lassen, dann greifen die beschriebenen Debiasing-Mechanismen nicht. Das mag besonders für Parteien zutreffen, die über wenig Expertise verfügen, um Kosten und Nutzen möglicher Regelungen zu beurteilen. Umgekehrt wird bei den meisten dieser Geschäfte der Verkäufer ein professioneller Akteur sein. Regelungen werden dann typischerweise ohnehin durch Formverträge vorgegeben. Neben Besitzeffekten könnten auch expressive Gründe Parteien an den Regelungen festhalten lassen: Parteien könnten im Gesetz ein Signal sehen, dass das dispositive Recht angemessen und unparteilich ist.

Ob die verbleibenden Konstellationen die rechtspolitischen und dogmatischen Vorschläge tragen, wäre zu diskutieren. So erhöht es Transaktionskosten, keine dispositiven Regelungen vorzugeben; das Gleiche könnte auch für eine stärkere Konkretisierung dispositiver Regeln gelten. Das Potential, dispositive Bestimmungen als Nudge einzusetzen, wie Jolls und Sunstein vorschlagen, könnte auch kleiner sein, als die Autoren vermuten, vor allem, wenn professionelle Akteure beteiligt sind. So geht es bei der Frage, ob zum Beispiel für Arbeitsverträge eine Just-Clause als Default vorgesehen wird, wohl weniger darum, dass Verlustaversion die Arbeiter in einem meist ungleichen Machtverhältnis dazu bringen könnte, auf dieser Klausel zu bestehen (wie von Sunstein vorgeschlagen), als eher um expressive Effekte und Verschiebungen dessen, was von der Gesellschaft als legitim angesehen wird (Kahan 1999, Lessig 2002).

B. Self-Nudging

Rechtspolitische Anwendungen für meinen Self-Nudging Ansatz finden sich in verschiedenen Rechtsgebieten. Am naheliegendsten ist eine Anwendung in der Gestaltung von privat- oder öffentlich-rechtlichen Verträgen, die von einer Partei eine Anstrengung verlangen, also etwa einen Arbeitseinsatz, das Anlegen finanzieller Mittel für die Altersversorge, das Einsparen von Umweltressourcen oder die Teilnahme an gesundheitlichen Förderprogrammen. Einer der Vorzüge des hier vorgeschlagenen Self-Nudging-Designs besteht darin, dass beide Vertragsseiten von ihm profitieren.

1. Private Verträge

Unternehmen haben Verlustframing bisher kaum als Instrument zur effizienten Gestaltung ihrer Arbeits- oder Werkverträge genutzt (Baker et al. 1988, Lazear 1991). Gründe dafür sind offenbar, dass der negative Verlust-Gewinn-Nutzen potentielle Vertragspartner von einer Vereinbarung abschrecken kann (Luft 1994), ebenso wie die Vermutung der Vertragsgegenseite, ihr Auftrags- oder Arbeitgeber wolle ihre Verlustaversion zum eigenen Vorteil ausnutzen (siehe Alterbaum 2004 und Luft 1994). Beide Gründe können nachhaltig Motivation zerstören (Kornhauser, Lu & Tontrup 2020) und führen im Experiment zur häufigen Ablehnung von Verträgen, die im Verlustframing gestaltet sind. Ebenso kann der Frame dazu führen, dass der Arbeitseinsatz in Erfüllung des Vertrags deutlich geringer ausfällt, vor allem dann, wenn die Vertragsziele Arbeitnehmer über- aber auch unterfordern (Brooks, Stremitzer & Tontrup 2017). Vorteile und Kosten einer Gestaltung von Verträgen im Verlustframing sind damit schwer zu kalkulieren, auch wenn es prima facie scheint, als sei höherer Einsatz ohne zusätzliche Kosten zu erreichen (Hossain & List 2012).

Meine Arbeit legt nun nahe, dass der Schlüssel für die Effektivität des Framing darin liegt, dass auch die Gegenpartei selbst die Vertragsgestaltung bevorzugt. Das kann etwa der Fall sein, weil das Verlustframing seines Arbeits- oder Werkvertrages es dem Vertragspartner erlaubt, sein zukünftiges Ich zu der Leistung zu animieren, die er selbst für optimal hält. Er kann also das Framing des Vertrages und die eigene Verlustaversion als Instrument zur Selbstbindung nutzen, um damit zeitinkonsistenten Präferenzen bezüglich der eigenen Arbeitsleistung zu begegnen. Die vertraglich vereinbarten Ziele zu erreichen, kann dann sowohl für ihn als auch für den Anbieter des Vertrages vorteilhaft sein.

Self-Nudging-Verträge setzen auf die autonome Entscheidung des Vertragspartners, dem Arbeitsverträge mit unterschiedlichen Leistungszielen und Vertragsgestaltungen angeboten werden. Die eingeräumte Entscheidungsautonomie hilft dabei nicht nur, dass der Vertragspartner den Arbeitsvertrag auswählen kann, der eigenen Präferenzen am relativ besten entspricht, sondern die gewährte Autonomie motiviert ihn auch zu mehr Einsatz und Leistung.

2. Regulierte Verträge: 401(k) – Altersvorsorge

Ein weiteres Anwendungsbeispiel ist die Regulierung der betrieblichen Altersvorsorge. Mit einer auf Self-Nudging basierten Regulierung könnte der Gesetzgeber versuchen, die Effektivität der angebotenen Programme zu verbessern.

In den USA soll die betriebliche Vorsorge die nur als elementare Alterssicherung ausgelegte Sozialversicherung (Old-Age, Survivors und Disability Insurance) ergänzen. Der Gesetzgeber fördert die betriebliche Altersvorsorge deshalb mit Steuervorteilen und regelt entsprechende Ausnahmen von der Einkommensteuer im Internal Revenue Code Abschnitt 401(k). Für die daraufhin 401(k) genannten Pläne lagert der Revenue-Code die Besteuerung der abgeführten Beiträge nach, der Zinseszins verbleibt also bei den Arbeitnehmern, die damit Kapital akkumulieren sollen; auch bleiben ihre Vermögensgewinne aus Anlageverkäufen steuerfrei. Dem Arbeitgeber erlaubt die gesetzliche Regelung einen Zuschuss zur Altersvorsorge zu leisten, den er voll von der Steuer absetzen kann. Die Höhe des Zuschusses, den er zu leisten hat, bemisst sich dabei in Prozenten an der von den Arbeitnehmern aufgebrachten Einzahlung. Aufgrund der steuerlichen Vorteile können Unternehmen also mit den Vorsorgeplänen besser ausgestattete, finanziell attraktivere Arbeitsverträge anbieten ohne die Kosten dafür (vollständig) tragen zu müssen. Sie haben deshalb einen Anreiz, freiwillig 401(k)-Vorsorgeprogramme einzurichten und sie zu bezuschussen.

401(k)-Programme sollen vor allem Arbeitnehmer dazu bewegen, in ihre Altersvorsorge zu investieren, die von sich aus aufgrund von Verlustaversion und Present-Bias nicht genug Vermögen zurücklegen würden. Der positive Zusatzeffekt dieser steuergeförderten Vorsorgepläne auf die Altersvorsorge der meisten Amerikaner hat sich bisher jedoch als begrenzt erwiesen. Das liegt vor allem daran, dass die Teilnahme an einem 401(k)-Plan für Arbeitnehmer bedeutet, dass sich unmittelbar Teile ihres heutigen Konsumnutzens in die (weite) Zukunft verschieben. Steueranreize und betriebliche Zuschüsse sollen diesen Verlust zwar attraktiver machen, ihre Konstruktion weist aber eine grundlegende Schwäche auf: Die betrieblichen Zuschüsse bemessen sich an den Vorsorgebeiträgen, die die Arbeitnehmer schon zuvor geleistet haben müssen. Auch die Vorteile durch die nachgelagerte Besteuerung wirken sich erst spät aus, wenn das Anlagekapital spürbar durch den Zinseszins angewachsen ist. Ihr Present-Bias sorgt deshalb dafür, dass die Arbeitnehmer von den Anreizen nicht so stark angesprochen werden wie vom Gesetzgeber angenommen und sie weiterhin zu wenig in ihre Altersvorsorge investieren. Tatsächlich nehmen im Ergebnis kaum mehr als 40% der Arbeitnehmer an den ihnen angebotenen betrieblichen Programmen teil und verpassen damit deutliche Vermögensvorteile. Chetty et al. (2014) betonen, dass die steuerrechtlichen und betrieblichen Anreize die Aufwendungen zur Vorsorge kaum zu erhöhen scheinen. Ihre Daten zeigen, dass für jeden aufgewendeten Dollar an Steuergeldern, der Betrag, den die Arbeitnehmer aufgrund des Programms zusätzlich für ihre Altersvorsorge ansparen, nur bei einem Cent liegt.

Dieses schlechte Ergebnis wird auch dadurch verursacht, dass der Gesetzgeber den Arbeitgebern bisher nicht rechtlich vorgibt, welche Vorsorgebeiträge sie in den von ihnen offerierten 401(k)-Plänen als Default festlegen dürfen. Bei Abschluss des Arbeitsvertrages betonen Arbeitgeber zwar die Höhe der möglichen Zuschüsse, setzen die Defaults für die tatsächlich abzuführenden Beiträge dann aber oft sehr niedrig an. Das liegt daran, dass die Höhe der betrieblichen Zuschüsse, die der 401k-Plan sie zu leisten verpflichtet, sich prozentual nach den von den Arbeitnehmern tatsächlich abgeführten Beiträgen richtet. Chetty et al.`s Daten bestätigen dann auch, dass viele Arbeitnehmer die niedrigen Defaults nicht aktiv ändern und damit betriebliche Zuschüsse verpassen. Mit der Setzung niedriger Defaults unterwandern die Arbeitgeber also das Ziel der gesetzlichen Förderung, um eigene Ausgaben zu sparen (siehe Bubb et al. 2014a, b; Batchelder 2018, Madrian and Shea 2001).

Benartzi und Thaler (2004) wollen mit ihrem Vorschlag “Saving-More-Tomorrow“, die Effektivität der 401(k)-Programme erhöhen. Ihr Vertragsdesign soll vermeiden, dass der Vorsorgeplan Present-Bias und Verlustaversion der Arbeitnehmer aktiviert, weil sie unmittelbar Konsumnutzen verlieren, den der Plan in die Zukunft verschiebt. Um beide Biases zu neutralisieren, verlagern Benartzi und Thaler deshalb die Last der Vorsorgebeiträge in die Zukunft. So sollen Arbeitnehmer ihre Aufwendungen nicht sofort leisten, sondern erst, wenn sie eine Lohnerhöhung erhalten haben; damit wird vermieden, ihren Present-Bias zu triggern. Damit auch ihre Verlustaversion sie nicht davon abhält, an einem Programm teilzunehmen, sollen sie nicht fürchten müssen, durch eine Teilnahme an Lebensstandard zu verlieren. Deshalb werden die Aufwendungen für die Altersvorsorge nicht nur zu Beginn, sondern dauerhaft aus Lohnerhöhungen finanziert und je nach Zunahme des Einkommens automatisch gesteigert. Im Ergebnis soll das die Arbeitnehmer ermächtigen, rational – also unbeeindruckt von ihren Biases - über ihre Teilnahme am Förderprogramm zu entscheiden.

So verhindern Benartzi und Thalers zugleich, dass die Arbeitnehmer das Programm später wieder verlassen, sobald die Aufwendungen für die Altersvorsorge tatsächlich einbehalten werden. Denn die Aufwendungen belasten den Arbeitnehmer nicht zusätzlich, da er aufgrund der Finanzierung durch Loherhöhungen ja nicht über weniger Mittel verfügt, als zuvor. Zum anderen soll ihn Inertia abhalten, das Programm zu beenden. Denn auch wenn sich seine Beiträge mit steigendem Einkommen automatisch erhöhen, bleibt der Arbeitnehmer im Programm, ohne dass er ihm noch einmal zustimmen müsste. Die Arbeitnehmer müssten also selbst aktiv werden, um das Programm zu verlassen oder ihre Beiträge später noch zu senken.

Ein wichtiges Problem bleibt aber auch bei "Save-More-Tomorrow" bestehen: Die finanziellen Anreize, wie die Steuerfreistellung und die Arbeitgeberzuschüsse, realisieren sich weiterhin erst in der Zukunft und bleiben deshalb bei Arbeitnehmern mit ausgeprägtem Present-Bias in ihrer Wirkung beschränkt. Auch hält Inertia die Arbeitnehmer zwar offenbar davon ab, Programme zu verlassen, denen sie einmal beigetreten sind (Chetty et al. 2014). Dies gilt aber vor allem dann, wenn Arbeitgeber den Defaultbeitrag, den die Arbeitnehmer abzuführen haben, niedrig halten. Bei steigenden Beiträgen drohen dagegen gerade jene Arbeitnehmer den Plan zu kündigen, auf die die Intervention abzielt: Arbeitnehmer, deren Present Bias so stark ist, dass sie auch Lohnerhöhungen direkt konsumieren wollen, anstatt sie für ihre Altersvorsorge anzusparen.

Der Self-Nudging-Ansatz, den ich hier vorschlagen will, versucht, anders als Benartzi und Thaler nicht, Verlustaversion und Present-Bias zu neutralisieren, sondern will erreichen, dass Arbeitnehmer sie zu ihrem Vorteil nutzen. Dafür müsste der Gesetzgeber die Rahmenbedingungen für die 401(k)-Pläne in einem wesentlichen Punkt ändern: Während nach gegenwärtiger Rechtslage die steuergeförderten Arbeitgeberzuschüsse erst geleistet werden, wenn die Aufwendungen des Arbeitnehmers für das abgeschlossene Steuerjahr einbehalten sind, könnten Arbeitgeber ihre Zuschüsse (monatsweise) vorab leisten. Dann matched der Arbeitgeber also nicht am Ende eines Steuerjahres die durch den Arbeitnehmer abgeführten Aufwendungen, sondern umgekehrt, der Arbeitnehmer matched mit seinen Beiträgen die vom Arbeitgeber vorab geleisteten Zuschüsse. Dabei bemessen sich die zu leistenden Zuschüsse an den Vorsorgezielen, die der Arbeitnehmer für sich selbst festlegt. Das bedeutet, je ambitionierter der Arbeitnehmer seine Vorsorgeziele wählt, desto größer sind auch die betrieblichen Zuschüsse, die unmittelbar auf sein 401(k)-Konto eingezahlt werden. Führt er dann niedrigere Beiträge ab, als in seinen Vorsorgezielen definiert, so gehen ihm Zuschüsse und Zinsen wieder verloren. So treibt die eigene Verlustaversion den Arbeitnehmer dazu an, seine Vorsorgeziele tatsächlich zu erreichen. Das Verlustframing könnte die Anreizwirkung der Zuschüsse und Steuervorteile so deutlich verstärken (eine weitere Studie dazu ist geplant).

Das zweite Element des Self-Nudging-Ansatzes ist die autonome Entscheidung der Vorsorgeberechtigten. Zwar können die Arbeitnehmer auch bisher Vorsorgeziele selbst auswählen, allerdings dürfen Arbeitgeber einen Default für die Beiträge vorgeben. Diese Regelung hat mehrere Nachteile. Zunächst nutzen die Arbeitgeber sie aus, um mit einem niedrig angesetzten Default geringe Beiträge zu provozieren. Zum anderen macht die Regelung die freie Entscheidung der Arbeitnehmer nicht salient, da sie häufig einfach den vorgebebenen Default akzeptieren. Stattdessen sollten die Arbeitnehmer eine aktive Entscheidung treffen. Der Autonomienutzen, den sie aus dieser selbstbestimmten Entscheidung ziehen, könnte sie, wie die Daten meiner Studie zeigen, ambitioniertere Vorsorgeziele anstreben lassen als sie zu akzeptieren bereit sind, wenn ihnen ihr Arbeitgeber ein bestimmtes Planziel vorgibt.

Wichtig für die rechtliche Regulierung der 401(k)-Pläne ist dabei ein weiteres Ergebnis meiner Studien: Der Autonomieeffekt bleibt im Experiment auch dann bestehen, wenn die Probanden ihr Arbeitsziel nicht allein festlegen, sondern ihnen Arbeitsverträge mit unterschiedlichen Zielen angeboten werden, zwischen denen sie wählen können. Auf die Regulierung der 401(k)-Programme übertragen, könnte es deshalb effektiv sein, eine rechtliche Vorauswahl der Vorsorgeziele zu treffen und den Arbeitnehmern Programme (Batchelder spricht von "Smart Defaults" 2018) anzubieten, die nur Vorsorgeziele zur Auswahl stellen, die die gewährten Steuervorteile und Arbeitgeberzuschüsse weitgehend ausschöpfen und dauerhaft zu einer ausreichenden Absicherung des Arbeitnehmers führen. Um die Autonomieeffekte zu nutzen, sollte kein Default-Beitrag vorgegeben werden, damit die Arbeitnehmer selbst eine aktive Entscheidung treffen. Um sicherzustellen, dass Arbeitgeber ihren Arbeitnehmern effektive Beitragssätze mit entsprechenden Zuschüssen zur Auswahl anbieten, müsste der Gesetzgeber vorgeben, welche Beitragsziele in 401(k) Plänen als Minimum vereinbart werden dürfen.

Der hier skizzierte Self-Nudging-Ansatz kann leicht mit dem Save-More-Tomorrow-Programm von Benartzi und Thaler kombiniert werden. Save-More-Tomorrow nutzt Smart-Defaults. Anstatt Arbeitnehmern aber nur einen Smart-Default vorzugeben, sollten sie selbst zwischen verschiedenen als effektiv angesehenen Beitragssätzen wählen, um Autonomieeffekte zu generieren. Wie oben gesehen, will Save-More-Tomorrow die Aufwendungen durch zukünftige Gehaltssteigerungen der Arbeitnehmer finanzieren. Anstatt aber zunächst die Aufwendungen einzubehalten, könnten die Arbeitgeberzuschüsse vorab auf das 410(k)-Konto eingezahlt werden. Dabei würden die Zuschüsse anders als bei Save-More-Tomorrow an den von den Arbeitnehmern gewählten Beitragssätzen bemessen. In diesem Fall hielten dann sowohl Inertia als auch Verlustaversion bzgl. der Arbeitgeberzuschüsse die Arbeitnehmer davon ab, den 401(k)-Plan zu kündigen, oder die eigenen Beiträge zu senken. Die Beiträge würden so in die Zukunft verlagert, wie von Save-More-Tomorrow vorgeschlagen, zugleich aber würden Autonomieeffekte und Verlustaversion bzgl. der Arbeitgeberzuschüsse aktiviert.[27]

[27] Es sei angemerkt, dass Save-More-Tomorrow insoweit die Lebenswirklichkeit vieler Amerikaner nicht trifft, als dass viele nicht mit regelmäßigen Gehaltserhöhungen rechnen können, wie wesentlich über einen Inflationsausgleich hinausgehen.

Eine Weiterentwicklung der 401k-Pläne zeigt die aktuelle Neufassung des Pension-Protection-Act (2022). Sie erlaubt es Unternehmen, Arbeitnehmer ohne ihre Zustimmung automatisch einem 401(k)-Plan zuzuordnen und Vorsorgebeiträge von ihnen einzubehalten. Die Arbeitnehmer können dem Abzug der Beiträge von ihrem Lohn nachträglich widersprechen und erhalten dann ihre Aufwendungen erstattet. Aufgrund von Inertia soll so die Anzahl der an den Programmen teilnehmenden Arbeitnehmer steigen. Interessant ist, ob eine automatische Teilnahme Autonomieeffekte zerstört oder ob sie bestehen bleiben, wenn die Arbeitnehmer zwar zunächst einem 401(k)-Programm ohne ihre Zustimmung zugeordnet werden, ihnen aber im Anschluss die Wahl zwischen alternativen Vorsorgezielen gegeben wird. Alternativ ließe sich die automatische Teilnahme durch eine Regelung ersetzen, die den Arbeitnehmern eine aktive Entscheidung über ihre Teilnahme an einem 410(k)-Plan abverlangt (Thaler & Sunstein 2008). Die Arbeitnehmer müssten eine Teilnahme also aktiv ablehnen, anstatt das Angebot einfach unbeachtet zu lassen.

Self-Nudging lässt sich in ähnlicher Weise auch für viele verwandte Regulierungsaufgaben nutzen. Im Folgenden schauen wir uns zwei Beispiele aus dem deutschen Recht an.

3. Leistungsverwaltung – Abfallrecht

Auch Ziele des Abfallrechts könnten sich durch eine Regulierung, die Self-Nudging nutzt, besser erreichen lassen. Die Regelungen des Abfallrechts sowohl auf europäischer, bundes- und landesrechtlicher Ebene haben die Abfallvermeidung als zentrales Ziel. Um dieses Ziel zu erreichen, sollen Anreize gesetzt werden, die Bürger und Unternehmen zur Abfallvermeidung animieren (z.B. §3 KrW-/AbfG Bln). Kommunale Gebührensatzungen konkretisieren diese Zielsetzung, indem sie nicht lineare Preissteigerungen für die Entsorgung größerer Müllmengen vorsehen. Das Anreizsystem der meisten Kommunalsatzungen weist dabei aber deutliche Schwächen auf und führt zu Zielkonflikten: Sie sehen vor, dass Haushalte zunächst eine von mehreren unterschiedlich dimensionierten Abfalltonnen auswählen und dann die Gebühren zahlen, die für die ausgewählte Größe anfallen. Damit setzen die Satzungen den Haushalten keinen Anreiz, ihre Abfallmenge unter das Volumen der einmal gewählten und bezahlten Tonnengröße zu senken. Zudem kostet der spätere Wechsel der Tonnengröße meist eine zusätzliche Gebühr. Ein kontinuierlicher Anreiz zur Abfallvermeidung besteht deshalb kaum. Da sich die Gebühren nach der ausgewählten Tonne richten, haben die Haushalte zudem einen finanziellen Anreiz, die Behältergröße knapp passend zu wählen und Müll anderweitig zu entsorgen, wenn die Tonne überfüllt ist. Um zu vermeiden, dass es vermehrt zu einer illegalen Müllentsorgung kommt, setzen die Kommunen sodann bewusst eher moderate Anreize zur Müllvermeidung (Engel 2002).

Um Self-Nudging zu nutzen, müssten die Kommunen ihre Gebührensatzungen anpassen und auf ein Verlustframing umstellen. Dafür könnte die Satzung etwa jedem Haushalt auferlegen, für ein jeweils neues Gebührenjahr eine Jahresgebühr zu leisten, die qua festgelegtem Prozentsatz über der Gebühr liegt, die der Haushalt effektiv im Vorjahr (Rückzahlungen eingerechnet) entrichtet hat. Auf diese Jahresgebühr bezogen, bestimmt sich dann der monatliche Abschlag, den ein Haushalt zu leisten hat. Wenn das neue Gebührenjahr beginnt, werden den Haushalten verschiedene Entsorgungspläne vorgelegt, die abgestuft nach der Menge des zu entsorgenden Abfalls überproportional günstiger werden. Die Haushalte können zwischen den angebotenen Plänen wählen und auf ihrem Konto wird eine dem ausgewählten Plan entsprechend hohe Rückzahlung verbucht. Zugleich wird nach jeder Leerung die im laufenden Jahr bereits entsorgte Abfallmenge auf ihrem Konto gespeichert (der abgelieferte Abfall wird am Laster gewogen). Überschreitet der Haushalt am Ende des Jahres die im gewählten Plan enthaltene Abfallmenge, so geht dem Haushalt die verbuchte Rückzahlung verloren. Lässt der Haushalt sogar mehr entsorgen, als durch die bezahlte Jahresgebühr abgedeckt, könnten entsprechend Gebühren nachgefordert werden. Diese Gebühren würden zudem auch die für das nächste Gebührenjahr festgelegte Jahresgebühr erhöhen.

Dieses Self-Nudging-Design hat gegenüber den bisherigen Regelungen der Gebührenordnungen mehrere Vorteile: Zunächst sollte Verlustaversion die Haushalte antreiben, nicht mehr Abfall zu entsorgen als in dem von ihnen gewählten Plan vorgesehen ist, um die eingebuchte Rückzahlung nicht zu verlieren. Da die Gesamtmenge des entsorgten Abfalls jederzeit auf ihren Konten einsehbar ist, haben die Haushalte zudem einen kontinuierlichen Anreiz, Abfall zu vermeiden. Wie in meiner Self-Nudging-Studie könnte die Rückzahlung auch linear gekürzt werden, der Menge entsprechend, die die Haushalte über ihrem Plansoll liegen. Umgekehrt könnte auch die Rückzahlung erhöht werden, wenn die Haushalte weniger Abfall entsorgen lassen, als in ihrem Plan enthalten. Damit bleibt der Anreiz zur Müllvermeidung unvermindert bestehen, auch wenn die Haushalte absehbar das Planziel über- oder unterschreiten werden.

Weil sich die finanziellen Anreize zur Müllvermeidung zudem auf die tatsächlich entsorgte Abfallmenge beziehen, und nicht auf die gewählte Behältergröße, haben die Haushalte keinen finanziellen Grund, sich für eine möglichst kleine Abfalltonne zu entscheiden. Ein größerer Behälter ist seltener überfüllt und gibt deshalb weniger Anlass, Abfall illegal zu entsorgen. Das erlaubt den Gemeinden, die Anreize zur Abfallvermeidung zu stärken ohne das Risiko illegaler Müllentsorgung zu erhöhen. Der Konflikt zwischen den beiden gesetzgeberischen Zielen entschärft sich damit.

4. Mitgliedschaft: Bonusprogramme der Krankenkassen

Ein weiteres Anwendungsgebiet für den Einsatz der hier vorgestellten verhaltenssteuernden Instrumente, könnte die Gesundheitsvorsorge sein. Viele gesetzliche Krankenkassen setzen in Bonusprogrammen (§ 65a SGB V) ihren Versicherten Anreize, Vorsorgeuntersuchungen oder Impfungen wahrzunehmen. Die Versicherten sollen dafür Bonushefte führen, in denen die Untersuchungen und Maßnahmen, die sie absolviert haben, dokumentiert werden. Erfüllen sie die Vorgaben des Bonusprogramms, so erhalten sie Zahlungen von bis zu €300 oder entsprechende Sachprämien.

Diese Bonusprogramme ließen sich leicht auf eine Regelung umstellen, die Self-Nudging und Verlustframing nutzt, um ihre Effektivität zu verbessern. In einem entsprechend gestalteten Bonusprogramm, wählen die Patienten zunächst selbst einen Katalog von Maßnahmen aus, die sie wahrnehmen wollen. Daraufhin wird der entsprechende Bonus unmittelbar auf ihrem Mitgliedschaftskonto eingebucht. Im folgenden Versicherungszeitraum müssen die Mitglieder die gewählten Vorsorgemaßnahmen tatsächlich wahrnehmen. Wenn sie das tun, wird ihnen der eingebuchte Bonus ausgezahlt (wohl weniger effektiv, der Krankenkassenbeitrag des nächsten Jahres reduziert sich), wenn nicht, ist der Bonus verloren. Um eine solche Regelung umzusetzen, müssten die Krankenkassen lediglich die Bonusprogramme in ihren Satzungen neu regeln.

C. Methodische Schlussfolgerungen

Dass Ökonomie und Psychologie eigene Forschungsfragen haben und der Rechtswissenschaft keine Readymades liefern, die direkt auf rechtliche Probleme antworten, ist nicht überraschend. Ob Verlust-, oder Reueaversion zu einer Behinderung des Güteraustausches führen, hängt von der den Akteuren gestellten Aufgabe, dem sozialen Kontext, den involvierten Institutionen und davon ab, dass Menschen auch selbst ihren eigenen Biases begegnen können. Die Judgment-und-Decision-Making-Literatur analysiert die Faktoren, weil sie vor allem die kognitiven Grundlagen der Verhaltenseffekte verstehen will, oder weil sie optimales Entscheidungsverhalten analysiert, anstatt der imperfekten Strategien, die Menschen tatsächlich benutzen, um ihren Biases zu begegne, die aber für das Recht viel interessanter sind. Auch die Interaktion der verschiedenen Debiasing, und Bias isolierenden Faktoren und Strategien, ist vor allem praktisch und damit rechtspolitisch wichtig, denn sie beantwortet die Frage, welche Auswirkung die Biases tatsächlich im Austausch von Gütern entfalten. Insoweit gibt die Analyse von Verlustaversion und Besitzeffekten ein deutliches Beispiel für die Besonderheit empirisch rechtlicher Arbeit.

Hinzukommt, dass empirische Probleme für die begriffliche Fallanalyse häufig unsichtbar bleiben. Besitzeffekte sind dafür ein schönes Beispiel. Transaktionen, die wegen eines Bias nicht zustande gekommen sind, werden als fallpraktisches Problem nicht sichtbar. Trotzdem gefährden sie das rechtliche Ziel eines effektiven Güteraustauschs. Man kommt deshalb kaum ohne Rechtsempirie aus, die es ermöglicht, tatsächliche Wirkungen zu prognostizieren und zu messen. Sie ist deshalb praktisch überall auf der Welt ein anerkanntes Fach der Rechtswissenschaft.

Chapter 1 – Systematic Overview of the Project

I. Introduction

A. Why Law and Private Ordering are concerned with Loss Aversion

Over the last decades, hundreds of law review articles have argued that Loss Aversion and the Endowment Effect compels the law to intervene in private ordering. In free markets, sellers and buyers should ideally exchange goods until they are owned by those who value them the most, absent transaction costs (Coase, 1960). But transaction costs are not the only impediment to beneficial exchange. If their decisions are biased, the owners themselves can stand in the way of efficient trade. The Endowment Effect's size suggests that the bias is a severe threat to trading: Prices demanded in the state of ownership often exceed buyers` willingness to pay by a factor of 3 or 4. When the standard experimental protocols are employed, the bias is robust and replicates reliably. The implications for private ordering are fundamental. As such, the Endowment Effect suggests that initial allocation matters strongly, unlike in a Coasian world and beyond transaction costs. Owners will keep their entitlements, even when other market participants would value the property considerably more if they owned them (see for example, Thaler, 1980; Knetsch and Sinden, 1984; Knetsch, 1989; Tversky and Kahneman, 1991; Kahneman et al., 1991). Voluntary transactions, however, cannot realize, and cannot even discover these gains from trade, because they exist only when both parties are in the same state of possession. By contrast, when one market participant owns the entitlement and the other considers buying it, the same gains from trade can be invisible. Thus, unless owners themselves overcome their Endowment Effect, it seems the bias calls for legal intervention. And the previous legal literature has assumed that owners have no incentive to overcome their bias, arguing that the Endowment Effect elevates owners` true valuations for their property, such that they would not want to sell beyond their elevated preferences. Consequently, legal scholars have suggested interventions or adaptations of the applicable law for a wide range of important entitlements, including intellectual property, contractual default rules, real property, legal settlements, corporate control, consumer debt, employment, and environmental protection.

They advocate interventions to reallocate entitlements, to alter contractual default rules, or to weaken people's sense of endowment by replacing property rules with liability rules or bright- line rules with standards (for example, Sunstein, 1986; Coates and Subramanian, 2000; Jolls et al., 1998; Korobkin, 1998; McCaffert et al., 1995; Rachlinski and Jourden, 1998; Buccafusco and Sprigman, 2011; for more than 1,600 legal articles citing the endowment effect, see Korobkin, 2014).

B. Empirical Evidence Supporting the Endowment Effect

With its claims for intervention, the legal literature relies on an extremely large body of experimental evidence, stemming both from economics and psychology. Not only have hundreds of independent experiments shown the effect, it has also been reported for a huge variety of objects, ranging from simple consumer goods (such as mugs, pens, and chocolate bars; e.g., Brown, 2005; Johnson, Häubl, and Keinan, 2007; Nayakankuppam and Mishra, 2005) to objects with risky or uncertain outcomes, such as lottery tickets (e.g., Ashby et al., 2012; Buccafusco and Sprigman, 2010; Cook and Wu, 2001; Eisenberger and Weber, 1995; Harless, 1989; Inder and O'Brien, 2003; Knetsch and Sinden, 1984; Peters et al., 2003; van de Ven, Zeelenberg and van Dijk, 2005; Van Dijk and Van Knippenberg, 1996) and intellectual property rights (Buccafusco and Sprigman, 2010, 2011; Sprigman et al., 2013). As the effect has been shown to increase in tandem with involvement in the creation of the good that is to be traded, it seems to be particularly relevant for patenting (cf. IKEA effect; Norton et al., 2012). Overall, the evidence indicates that the phenomenon is very robust – and even though there are considerable differences in effect sizes, the endowment effect seems to at least double the owner's valuation in most cases (for an overview, see Horowitz and McConnell, 2002).

C. Methodological Challenges of the Evidence

However, both the evidence for the Endowment Effect and policy recommendations based on it have been challenged recently. Plott and Zeiler criticized many studies for producing mere methodological artefacts. For example, experimenters often did not allocate the goods randomly. Plott and Zeiler show that subjects value a good more, when they have the feeling that it was selected particularly for them. Furthermore, many studies imposed different transaction costs on buyers and sellers. Buyers had to perform an action to acquire the good, while owners kept the good if they simply remained inactive. Beyond the different transaction costs, the design also induces an omission bias: remaining inactive reduces felt responsibility and regret over a potentially negative outcome to the trade. However, while Plott and Zeiler`s analysis has effectuated a lasting improvement of Endowment Effect studies' experimental protocol, the bias has since been reproduced attending to their critique, as shown, for example, by Isoni et al. (2011), Heffitz and List (2013), Arlen and Tontrup (2015 a, b), and others.

D. Four Debiasing Mechanisms

In contrast to Plott and Zeiler, the goal of this work does not question that loss aversion and ownership bias valuation. I do not argue that these phenomena are experimental artefacts, as suggested by Plott and Zeiler, but that previous experiments have not been trimmed to address the legal concerns in relation to private exchange. I will present evidence for four mechanisms that I expect to ease and often eliminate sellers` loss aversion. First, I will show that previous studies in Economics and Psychology, but also in law, when they analyze the endowment effect, implement a valuation task, not a trading task, one in which people trade goods for profit. The valuation task elevates the bias, as it focuses owners on their entitlement, while trading focuses them on potential buyers, their WTP and gains from trade.

Second, I will show that previous studies have abstracted from the institutions typically used by people when acting within a market; institutions like principal agency relationships that as the evidence will show strongly reduce ownership bias.

Third, I will show that the social context of the market, abstracted by prior studies, debiases owners. In markets, sellers can observe the behavior of other market participants. I will present evidence that, instead of the status quo, they can take the behavior of other market participants as their reference point. As such, whenever trading seems to predominate in the market, I suggest herding to ease owners` loss aversion. Social preferences can also impact loss aversion. I analyze an Anticommons dilemma that triggers social preferences because sellers do not only affect their own outcome when they push up the price they request, but also the outcome of the other Anticommons owners. Their social preferences shift their attention from their entitlement to their social context, easing their loss aversion.

Fourth, the literature has not considered the biased decision-makers themselves as a source of debiasing. Legal scholars typically assume that entitlement holders have no ability or no incentive to overcome their own biases. In contrast, I will present evidence that people deliberately use debiasing strategies to manage their own biases. In fact, I will show that they use the same strategies that BLE scholars recommend lawmakers should use. The first strategy is self-debiasing, which aims to remove the bias; I will present evidence that people deliberately involve agents in their transactions in order to ease their regret and loss aversion over trading. To the second strategy I refer as self-insulating, as it aims to deprive owners' bias of its effects. The evidence will show that, when owners' loss aversion and regret biases them against trading, owners strategically access market information, replacing the status quo as their reference point with other market participants' predominant choices. Taking the new reference point shifts their bias, pushing owners in the direction the market suggests. The final strategy I will analyze is self-nudging, referring to individuals employing their own bias to make their future-self reach a better performance and outcome.

The evidence I present suggests that subjects deliberately choose loss-framed over gain-framed contracts to nudge their future selves into investing more effort. Self-nudging highlights the awareness and understanding people can have of their own bias, which allows them to purposefully turn their bias into a productive commitment device. As it describes strategies that people use effectively as a means to reach better outcomes, my concept of bias-self-management pertains specifically to the law's policy interests. This allows me to show that bias-self-management can complement external regulations, as people can reach outcomes on their own that external regulation may otherwise seem necessary to achieve.[28]

In the following sections I will analyze the four debiasing mechanisms my studies have identified in detail: (1) shift of focus from entitlement to gains from trade because of the trading task; (2) institutions involved in trading; (3) social context and social preferences; and (4) bias-self-management.

II. Study 1: Trading for Profit is not a Valuation Task

A. Theory

1. Trading for Profit

Legal analysis of the EE concerns the effectiveness of private exchange. It focuses on facilitating trade. Trading is inherently interactive. Traders do not simply determine their personal valuations for goods, and price the goods they want to sell accordingly. Typically, their personal valuations only amount to well-hidden reservation prices, constituting just one element of the cognitively more complex task of trading. Rather than thinking about their personal valuations, most sellers will be concerned with correctly estimating what WTP potential buyers might have. They will study market prices, also considering the quality of the goods other sellers might offer. Often, gains from trade reach far beyond the concrete transaction. Traders must take the potential for future deals into account and fulfilling profit expectations may be crucial for a personal promotion they are aiming for. I assume that the complexity of this trading task directs the sellers` attention from their entitlements' valuable properties to the transactions' social and economic context This change of attentional focus reduces loss aversion and the bias in their valuations of the property.

Economics and Psychology do not consider this strategic nature of private exchanges and therefore can mislead legal recommendations to overestimate the impact of the bias on market transactions. On the assumption that it will confound the experiment's results, they incentivize participants to state

[28] By contrast, as I will explain below the business literature's primary interest is developing optimal debiasing strategies; they intend to advise others as to how to improve, rather than describing people's actual behavior.

their true valuations for the entitlements the participants are buying and selling. Where strategic effects are not excluded, their effect on the EE is not systematically measured. Systematic measurement of the EE would require both eliciting subjects` true valuations in a standard BDM design *and* placing these subjects in a strategic market to compare their EE, all else being equal. Most of the previous EE studies use one of two experimental paradigms: a random price mechanism or a random allocation design. The random allocation design assigns subjects either the role of an owner - who receives the good - or the role of a buyer, who is given a monetary endowment. Assuming the valuation for the good is randomly distributed across the owners and buyers, half of the owners should sell, while the other half should keep the good. The fewer people trade, the more strongly does the evidence for the Endowment Effect emerge. The design is supposed to suppress strategic pricing considerations. If sellers and buyers trade for the market clearing price, then sellers act as price takers, and do not benefit from pushing the price. In the BDM design, subjects either receive the good or a randomly drawn price, but not what they actually demand. Therefore, with their pricing decision they cannot influence the amount they receive.[29]

With strategic considerations pushed aside, subjects must only process and weigh the value- increasing and -decreasing attributes of the good; other information is not relevant and is not provided to the participants. As a result, both paradigms focus subjects` attention on the valuations of their entitlements. This parsimonious design is deliberate. It is well-suited to demonstrate both that valuation depends on reference points and is influenced by loss aversion. The design strips down the decision-making context, such that ownership alone can affect subjects valuations of the good. The participants have no cognitive tasks besides valuing and pricing their entitlements.

My goal in this study is twofold: first, I want to show that the debiasing effect in the trading task is not driven by accumulating experience by the traders, how often they have repeated the task in auction (Loomes et al. 2003) or how familiar they are with the traded goods (List 2003). Indeed, my experiment presents inexperienced student subjects with a one-shot task, they have not seen before. Their attentional focus is shifted by the trading task from their endowment to what is required to succeed in trading. This shift of attention is an automatic cognitive process, it is not a learning process that has to be completed over many repetitions. The market does not teach subjects to change their trading behavior.

[29] By demanding a sum higher than their true valuation, the subjects cannot increase their payment, because if they are paid they always are paid the random price. Rather, they only increase the probability that they will keep the good asking for more than the random price, when they would have preferred to have sold for the random price instead.

Second, the results demonstrate that the valuation design can mislead legal policy by elevating the endowment bias compared to a strategic market context. Trading in real markets is not a simple valuation task. The goal of market exchange is typically to earn a profit, which presents sellers with a different cognitive task. Sellers` behavior is often strategic, and they must process information holding little connection to the relevant good's attributes and the valuation they personally accord to it: market prices and trends; their personal reputations in business and in their communities; the option value of future deals; or strategic situations like hold-ups. Their true valuation of goods are only one cue among the many relevant to their ultimate pricing decision. Professional sellers are a prime example, because they hold the good only to sell it – in the end and if necessary, even for a price below their valuation. I assume that the trading task shifts the sellers` attention. Rather than focusing on their entitlement's valuable properties, they focus on the buyer, gains from trade, and many other elements of the transaction's social and economic context. I conclude that, in comparison to a valuation task in which subjects have to value and price their entitlement – as typically tested in Economics and Psychology, real private exchange – the task the law is concerned with - is much less impacted by the endowment effect.

2. Beyond Prospect Theory – Attentional Focus on the Forgone

I base my experimental predictions on a cognitive theory of attention. The original explanation for the Endowment Effect is Prospect Theory`s concept of loss aversion (Kahneman and Tversky, 1979): the owner experiences selling a good as a loss, while the buyer perceives adding the same good to his endowment as a gain. The loss looms larger than the gain, and as a result ownership biases preferences and sales prices. As such, an individual values the same good more in the role of a seller than in the role of a buyer, leading to a large gap between WTA and WTP prices (Thaler, 1980). Original Prospect Theory, however, black-boxes the cognitive process causing the differing experience of losses and gains. Therefore, Prospect Theory does not explain why social factors tend to strongly impact the magnitude of the Endowment Effect without changing endowment status. For instance, personal attachment (Strahilevitz and Lowenstein, 1998) and self-association with the good up for trade (Maddux et al., 2010) elevate an owner`s Endowment Effect. Given how strongly they can elevate the Endowment Effect`s size, both attachment and self-association must affect the cognitive process that causes the bias. The same holds for the strategic markets I analyze here: I expect them to reduce the Endowment Effect, but without altering the owners` endowment status. Apparently, in order to analyze the influence of profit-orientated behavior on the Endowment Effect, a better understanding of the specific cognitive process driving loss aversion is needed.

Recent work has tried to provide this understanding.[30] Carmon and Ariely (2000), for example, propose that the Endowment Effect is caused by a cognitive focus on the foregone. According to Carmon and Ariely, sellers focus on the good they would be giving up in the exchange, whereas buyers focus on the money they would have to spend to make the trade. Ashby et al. (2012) use eye-tracking to show that loss aversion focuses sellers on the positive, value-increasing aspects of their entitlements, while negative aspects receive little attention. Buyers, in turn, focus their attention on the opportunity costs they have to bear should they ultimately obtain the good. Johnson et al. (2012) show that the retrieval of positive and negative attributes from memory is also biased by endowment status. In consequence, when sellers and buyers weigh the positive and negative aspects of a transaction, endowment status induces the characteristic gap between WTA and WTP prices.

Attention-based theories predict that social factors can increase or reduce the Endowment Effect by impacting the seller`s attentional focus. For example, as personal memories increase owners' focus on their entitlements, they increase the Endowment Effect. I argue strategic market incentives do the opposite: they draw the sellers` focus away from their entitlement and towards the profit they might earn upon the accomplishment of a lucrative deal. I therefore expect strategic markets to effectively debias owners, loosening their loss aversion-driven fixation on their entitlement and switching their attentional focus to gains from trade.

I assume that the Endowment Effect is caused by a reference-dependent focus on the entitlement and its attributes, leading the owner to a biased weighting of the transaction's positive and negative aspects (Carmon and Ariel, 2000; Nayakankuppam and Mishra, 2005; Johnson et al., 2007; Ashby et al., 2012). When owners decide whether or not to sell goods, they focus on their entitlement's value-increasing aspects, thus giving them more weight in their decision-making. Conversely, buyers focus on the transaction's opportunity costs: the money they could otherwise use for alternative causes. This process biases decision makers towards their status quo. The literature has identified two effect channels driving the Endowment Effect.[31] The first is attachment, referring mostly to physical goods with qualities like beauty, elegance or usability. As endowment focuses owners' attention on these attributes, they become attached to their entitlements, and their valuations for their goods consequently increase. By contrast, the uncertainty of their future value stands as the characteristic attribute of stocks and patents (Lemley and Shapiro, 2005).

[30] See also the overviews by Morewedge, et al., 2015 and Brown, 2005.

[31] I assume that the underlying cause is the same for both channels.

With their salient win- or-lose structure, stocks trigger anticipated regret over the trading choice. The uncertainty can be caused by the properties of the good itself, or the market price. It can also result from a vague preference of the owner. Owners not sure how valuable their goods are to them, may anticipate feeling regret over a sale, should they realize, after the trade that they would have preferred to hold on to their entitlements.[32]

In my studies I have analyzed both effect channels separately. In the main experiment I endowed the subjects with lottery tickets, The tickets should cause anticipated regret to be the dominant behavioral channel of loss aversion and the Endowment Effect (Bar-Hillel et al., 1996; van Dijk and Zeelenberg, 2005). Therefore, I will base my experimental predictions below on regret theory (Kahneman and Tversky, 1982). However, I also have investigated the second effect channel of attachment and sentiment by conducting an otherwise identical separate study with the classic University mugs. The results are the same in comparison to the ones I will report here: in fact, the EE for the mugs disappears entirely, when subjects trade for profit (in more detail below).

So far, I have assumed that biased attention causes the good`s positive attributes to achieve a higher cognitive availability, increasing the values owners ascribe to their entitlements (Kahneman et al., 1986; Ashby et al., 2012; Johnson, et al., 2007). The relationship between loss aversion and regret costs is now straightforward: The more valuable the entitlements appear to their owners, the more regret they will anticipate over selling them (see Bell, 1982; Loomes and Sugden, 1982; Connolly and Butler, 2006; Zeelenberg and Pieters, 2004). For example, a patent- holder who considers selling his or her entitlement might focus on the patent's economic potential, increasing the regret felt over selling it. Loss aversion, with its focus on the benefits of the goods, makes regret costs asymmetric. As gains from trade receive comparatively less attention and weight in the decision-making process (see Ashby et al., 2012 with direct evidence from eye tracking), the patent holder will anticipate experiencing more regret from selling than from keeping his or her entitlement. The asymmetry of regret costs – I will speak of differential regret costs - biases owners against trading (Loomes and Sugden, 1982; Sugden, 1985; Landman, 1987; Baron and Ritov, 1994; Connolly and Butler, 2006; Nicolle et al., 2011). As applied to my study, I expect for subjects to experience more regret over selling their lottery tickets than over keeping them. Subjects in the seller role should seek compensation for their asymmetric regret costs by asking for higher sales prices.

[32] See also Weaver and Fredrick (2012). They find that people reject selling for a price that falls short of the assumed market price, even when it exceeds their true WTA. Sellers anticipate that they will feel regret over selling for their true WTA, should an option later arise to sell at market price.

B. Regret Costs and the Nature of Goods

People anticipate regret over parting with an entitlement whose future value is uncertain. The uncertainty may either be rooted in the market and the value it could attach to the good in the future, or in the owner`s personal valuation (Plott, 1996). If the owners` preferences are only vague, they may anticipate regret over trading, should they realize after their transactions that they would have preferred to keep their entitlements. As the future value of many entitlements is uncertain, anticipated regret can induce an endowment effect in a wide range of entitlements. These include real, intellectual, and personal property; corporate control; legal settlements; securities; and material contract clauses.

But regret costs can explain the endowment effect even for simple consumer goods, such as mugs and pens, whose value is known and stable. The rationale is that people anticipate regret over a sale at their true WTA, should the transaction turn out to be a bad deal compared to the market price. Therefore, even when they cannot sell the entitlement as a result, owners placing values lower than the market price on their entitlement are not willing to sell below market price, demanding prices higher than their actual WTA (Weaver and Frederick, 2012).

C. Study Design

To test my theory, I endowed subjects with tickets of a real public lottery, the Eurojackpot.[33] I informed the participants as to the probabilities that the ticket might win a prize, allowing them to determine the ticket`s expected payoff. My 2x4 design varied the endowment status of the subjects and, for each of the treatments, compared a pair of *Endowment* and *No-Endowment* conditions. In the *Baseline* treatment I used a BDM mechanism incentivizing participants to reveal their true valuations for the entitlement. In the second treatment I modified the BDM mechanism in a way that allowed subjects to benefit from strategic trading behavior. In the third treatment, subjects traded with a real transaction partner, who also had incentives to act strategically; in the final treatment, participants acted in a strategic market through an agent.

In the *Endowment* conditions, I presented subjects with a price list, asking them for each price whether they would prefer to sell or to keep their lottery tickets. Next, I compared the subjects` WTA with either the random price or the price offered by their assigned partner. If their WTA did not exceed that price, the subjects sold their tickets and received the random price or the offer

[33] The official website of the lottery: https://www.lottoland.com/en/eurojackpot/results-winning-numbers

in exchange. Otherwise, they kept their tickets. In the *No-Endowment* conditions, I used a choosing design (Johnson et al., 2007). Subjects had to choose, for each price in the list, whether they would prefer to receive the ticket or the money. The actual outcome depended on whether the subjects had chosen money or ticket at the random price or at the price offered by their partner.

In the *Strategy* treatment, I modified the random price mechanism, such that, participants were not paid the random price, but rather the amounts they demanded. As is typical in real markets, they could benefit from setting strategic prices.[34] The higher the amount they asked for, the higher was the degree of profit they can earn. In turn, however, strategic pricing created the risk that the deal failed at a price for which the owners would have preferred to have sold their tickets.

In the *Interaction* treatment, I strengthened the strategic character of the trading task, moving my design closer to that of a real market. I replaced the price mechanism with a real transaction partner who could likewise benefit from acting strategically, such that the subjects could try to outsmart each other. In the *Endowment* condition, subjects sold their tickets for the prices offered by their partner, did their WTA not exceed that offer; otherwise, they kept their tickets. In the *No-Endowment* condition, subjects received either the ticket or the money, depending on which of the two they had chosen for the prices offered by their partners for the tickets.

The Interaction with a partner complicates the trading task. When they form a belief about their partner's WTP for the ticket, subjects must consider that their partner may strategically lowball his or her offer. I assume that this interaction draws the subjects' attention to strategic considerations. Two psychometric tests support the debiasing effect with additional direct evidence: a regret and an incentivized loss aversion measure.

D. Hypothesis and Results

In the *Baseline* treatment, I expected ownership to focus the subjects` attention on the positive attributes of their lottery tickets. The fixated attention should lead to the characteristic gap between WTA and WTP prices revealing a bias. As expected, I replicate a strong Endowment Effect replicates in my *Baseline* treatment. Subjects demand an average €5.79 for selling their lottery ticket, while their choosing price in the *No-Endowment* condition is only €3.76.

As compared to in the *True Valuation Baseline* treatment, I assumed the strategic markets should reduce or eliminate the Endowment Effect. I expected

[34] Assume subjects demand €8 for the ticket while their true WTA is €5. In this case, they are paid €8 if the BDM price is not smaller than the €8 they asked for.

them to direct the sellers` attention from their entitlements towards the gains from trade, and buyers' potential WTP. The biased attention should lead subjects to assign more weight to the potential prize the lottery ticket may win, as opposed to the money that they would earn from the ticket's sale. The results support my attention-based theory, showing that the strategic market decreases the Endowment Effect. As expected, in the *Endowment* condition of the *Strategy* treatment, subjects demanded a mean WTA of €5.71 and in the *No-Endowment* condition, I find a mean choosing price of €5.11. The comparison yields only a small and insignificant bias (Mann-Whitney *p*-value 0.33; cohen`s *d* -0.24). Indeed, in the *Interaction* treatment, the WTA /WTP gap (*Endowment* condition: €4.54; *No-Endowment:* €4.14) shrinks further, to less than one-fifth of the magnitude I measured in the *Baseline* treatment; the remaining effect size is marginal (*p*-value 0.45 Mann Whitney; cohen`s *d* -014).

The results of both additional measures I conducted – the Likert scale items that elicit anticipated regret and the lotteries that reveal subjects` degree of loss aversion – support my hypothesis. In my *Baseline* treatment, the measures indicate that the subjects were biased. Owners experienced significantly more anticipated regret over selling their tickets than that regret felt by participants in the *No Endowment* condition over choosing the money instead of the ticket.

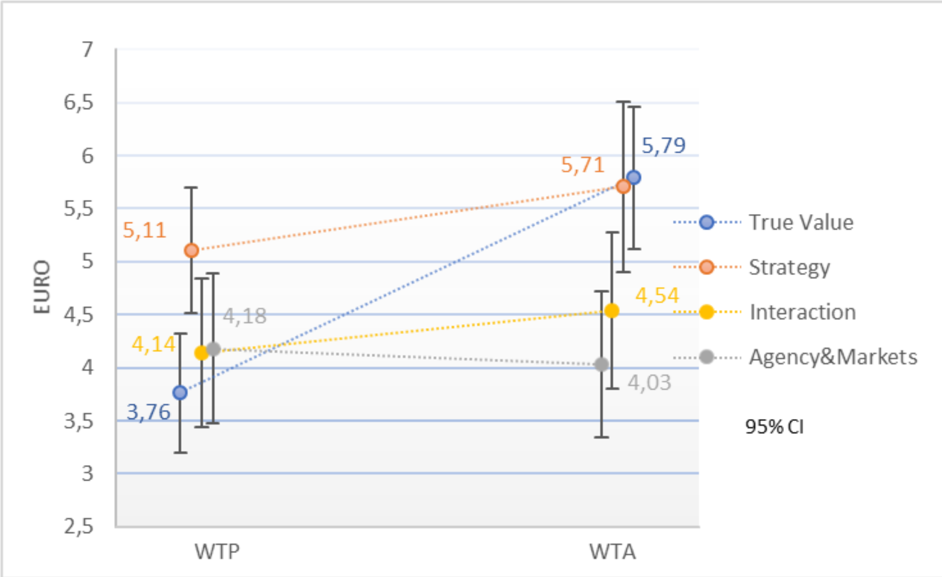
My loss aversion measure also reveals the bias. The subjects the measure identifies as loss averse have a significantly stronger Endowment Effect than the subjects categorized as rational. Consistent with the data on the pricing decisions presented above, the strategic market drives out the bias in both regret and loss aversion data. In the *Strategy* treatment, I find the difference to be insignificant between subjects' anticipated regret over selling their tickets and the regret they experience over choosing the money, respectively in the *Endowment* and *No Endowment* conditions. The difference disappears entirely in the *Interaction* treatment.[35] The same result holds for the loss- aversion data: neither in the *Strategy* nor in the *Interaction* treatment did loss averse (or rational) subjects reveal an Endowment Effect. Both measures thus suggest that the task of trading in strategic markets is a powerful debiasing mechanism.

For understanding the policy importance of this result, please note, that the debiasing effect in the trading task does not depend on the experience of the traders, how often they have repeated the task or how familiar they are with the traded goods. Indeed, the experiment presents subjects with a one-shot task, they have never seen before. The attentional focus is shifted by the differently structured task subjects have to solve when they trade rather than

[35] To support the results further I estimate a linear model that includes all data (see the result section).

evaluate an object. The shift of attention is an automatic cognitive process, it is not a learning process. The market does not teach subjects to change their trading behavior.

Figure 1: Debiasing Effect of Strategic Market Treatments



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John List (2003) by contrast reports in his sports cards field experiment, where he lets real treaders take part in experiments, designed in the typical exchange paradigm (=valuation task) that only by the most experienced traders accomplish to trade (almost) without an endowment bias. Engelman and Hollard (2010) assume that markets might not provide sufficient incentives to make traders learn to trade in the market without exchange symmetry.

III. Study 2: Market Institutions Debias Traders

A. Theory

The trading task itself is not the only mechanism that debiases sellers. The institutions involved in trading also ease loss aversion and regret over trading. Regret and decision-making responsibility are strongly correlated: the more responsibility a decision-maker assumes for the sale, the more regret he or she will anticipate experiencing over a possible negative outcome to the transaction. The literature shows that responsibility is a necessary prerequisite to experiencing regret (see, for example, Zeelenberg et al., 1998). People only feel regret and exhibit the bias when they feel responsible for making the relevant trading decisions. The relationship between responsibility and regret reveals a mechanism by which institutions can systematically debias entitlement holders. Institutions, such as agency and voting, involve others in the process of trading. This divides responsibility for the transaction among all of the involved actors, accordingly reducing the regret those actors expect to experience over trading. Therefore, I expected that transacting through these institutions should reduce the size and frequency of the endowment effect.

The study tests two institutions to support the theory of responsibility sharing: principal-agent relationships and voting. Principal-agent relationships distribute decision-making responsibility between the two parties, limiting the personal responsibility of each. The principal provides instructions, retains veto power, or both, but the agent decides upon and typically carries out the transaction. Evidence shows that people rank the responsibility for an outcome according to the involved actors' respective contributions. They tend to attribute the greatest responsibility to the last affirmative action in a causal chain, as it is this action that makes the outcome inevitable (see Spellman, 1997). A subsequent inaction, by contrast, is not given much weight, a phenomenon referred to as omission bias (see Baron et al., 2008). Thus, when agents execute the last affirmative action for the trade, principals share the responsibility and should therefore anticipate less regret. Voting has a similar effect. Under a majority rule, it divides the responsibility for the transaction between all voters and thus reduces the anticipated regret of the individual decision maker.

B. Study Design

I conducted the study in the laboratory and online. Each subject was endowed with a lottery ticket marked either "heads" or "tails" that had a 50% chance of winning the lottery. Subject won €8 if the ticket they held at the end of the session matched the lottery's outcome; otherwise, they earned nothing. Subjects could trade their tickets for a ticket with the alternate symbol (heads for tails or tails for heads), plus a monetary bonus of €0.25 for the exchange. Each lottery ticket had the same expected value; thus, expected earnings were higher if sub-jects decided to exchange their ticket for the alternative ticket. Since both tickets had the same probability of winning the 8€ payoff, neither risk aversion nor uncertainty about the true value of the goods exchanged could affect the subjects` decisions. The design allows me to identify the subjects who were biased: a rational subject should have traded, while any subject who retained his or her ticket exhibited an endowment effect. I used this basic setup as a benchmark treatment, *Baseline*, against which I measured the debiasing effects of agency and voting. *Baseline* intended to replicate the endowment effect.

The online experiment was largely identical with the laboratory study, except for that a winning lottery ticket paid €4 instead of €8. The bonus for trading remained the exact with €0.25. I instructed the subjects that they had been randomly assigned either a heads or a tails ticket through a code hidden in the instructions they received for the experiment. The code was to be revealed after the session was over. To assure the subjects that the lottery's outcome was truly determined by chance, the subjects themselves decided whether heads or tails won the lottery. The frequency of wins and losses was in fact consistent with the frequency determined by chance. In addition to measuring the trading frequency, I also elicited the sense of responsibility and regret felt by each subject, following decisions to trade or alternatively keep the ticket, over a potential negative outcome to the lottery. For this item, I used a 10-point Likert scale ranging from 1 (very little responsibility and regret) to 10 (very strong responsibility and regret). Online, 44.4% of the subjects did not trade their tickets. I observed more trading online than in the lab, likely because the lottery's stakes were lower online and subjects did not have physical possession of their lottery tickets. Supporting that regret causes an endowment effect, I found that subjects in the online *Baseline* condition anticipated significantly more regret if they traded than if they kept their ticket (7.2 versus 6.5; *p*-value =0.02).

In the first experimental treatment, I assigned each subject to an agent who made the trading decision; if the principal did not veto the decision, it came into effect. Since principals could not choose whether or not they wanted to involve the agent, this treatment is referred to as the *Mandatory* treatment. I implemented two additional treatments addressing potential confounds. The first control condition, *Default*, is designed to show that the debiasing results are neither driven by a shift in participants' reference points nor by omission bias. In the *Default* condition, I replaced the agent with a computer. The computer traded the subjects` tickets automatically, unless the subjects vetoed the trade. The subjects could not share responsibility for their decisions, as there was no other player who could assume some of the responsibility. If, sharing responsibility in the principal-agent relationship indeed debiases subjects, then in *Default* no debiasing should have occurred.

The second treatment ruled out information as an alternative explanation of a debiasing effect. Principals might assume that the agent's choice reveals valuable information to them and therefore confirm the agents' decision leading them to trade more often. I ruled out this objection with the *Guided Agent* treatment. In this treatment, I assigned each principal an agent who decided whether the ticket would be traded, but the principal could incentivize the agent's choices. If the principal had decided to incentivize trading, then the agent received €2 in exchange for the principal's ticket, and nothing if he or she rejected the trade. If the principal had incentivized the agent to keep the ticket for him or her, the agent earned €2 if he decided not to trade, and nothing if he exchanged the ticket. The agent was incentivized, but not bound, and the principal could not veto the agent's decision. The incentive payments were made by the experimenter. Since the principal decides *ex ante* whether to incentivize the agent, either to trade or to keep the ticket, an information confound was impossible.

In the voting treatments, subjects determined by majority decision whether all tickets should be traded or kept. In one treatment, subjects were bound by the majority's vote; in the other, participants could veto the majority decision, in respect to their personal ticket only. For all these treatments, I obtained direct evidence about responsibility and regret.

C. Hypotheses and Results

Consistent with prior evidence, more than 70% of the laboratory subjects indeed exhibited an endowment effect in the *Baseline* treatment providing me with a benchmark to test the debiasing effect of the principal-agent relationship against (see, for example, Knetsch and Sinden, 1984; Bar-Hillel and Neter, 1996; Isoni, et al., 2011; Korobkin, 2014). The central hypothesis suggests that the principal-agent relationship should mute the regret triggering loss aversion and the endowment effect. People experience regret over losses caused by decisions for which they feel responsible. The agency treatments divide the responsibility for the decision to trade between the principal and his or her agent. In *Mandatory*, the agent makes the initial decision and the principal decides whether or not to veto. In *Guided Agent,* the principal provides the agent with instructions and the agent, incentivized to follow these instructions, decides whether or not to trade. In both treatments, agents take the last affirmative action. Thus, compared with *Baseline,* I expected principals to trade their tickets more often and also anticipate less responsibility and regret over trading.

Supporting the theory that responsibility-sharing mutes the endowment effect, I found that subjects in *Mandatory* are significantly more willing to trade than those in *Baseline*. In the laboratory, 68.9% of the *Mandatory* subjects traded, compared with 29.7% of the participants in *Baseline* (p-value <0.01). Online 77.8% of the *Mandatory* participants traded, whereas only 55.5% exchanged their ticket in *Baseline* (p-value <0.01).

The additional treatments *Guided Agents* and *Default* I conducted only online, therefore they will be compared to online *Baseline* treatment. In *Guided Agent,* 75.3% of the principals incentivized their agent to trade their ticket. Thus, significantly more principals intended to trade than in the online *Baseline* treatment (55.5%; p-value <0.01).

In *Default,* the debiasing effect of the principal agent relationship should disappear. Just like the agent, the computer traded the ticket for the subjects unless they vetoed. However, with the computer, the subjects could not share their decision- making responsibility and therefore the computer agent could not reduce the level of their regret costs. Results were in line with this hypothesis. Significantly more participants traded in *Mandatory* (77.8%, p-value =0.03) than in *Default* (61.1%), while as expected, *Baseline* (55.5%) and *Default* did not differ significantly from each other (p-value 0.54).

In the *Guided Agent* treatment, 75.3% of the principals incentivized their agents to trade their tickets. Thus, significantly more principals wanted to trade than in the *Baseline* treatment. As principals take action, omission bias cannot affect results in *Guided Agent.* A shift of reference point is also implausible, as the principals themselves predetermined the choices of their agents, as they set the agents' incentives for either trading or keeping the ticket in the principals' name. Also, principals could not derive any information from their agent`s choice, as they have to make their decisions before observing the agent`s action.

When I compare reported regret and responsibility across treatments, I find that subjects indeed experienced significantly less responsibility for, and anticipated less regret over, trading in *Mandatory* (responsibility: 4.72; regret: 6.12), where subjects could share responsibility, than in *Base* (responsibility: 6.35, p-value <0.01; regret: 7.2, p-value <0.01). As theorized, the level of responsibility and regret was also lower than in *Default* (responsibility: 6.17, p-value =0.01; regret: 6.66, p-value =0.16), in which the computer made the trading decision. I find the same result pattern in *Guided Agent.* Compared with *Baseline, s*ubjects experienced less regret over (6.24, p-value =0.02) and responsibility for (5.64 p-value =0.09) a decision to trade.

I also elicited direct evidence supporting that subjects share responsibility in the principal-agent relationship. In *Mandatory,* assuming that having exchanged the tickets ultimately lead them to lose the lottery, principals attributed an even larger part of the responsibility for the negative outcome to their agent (6.23, *p*-value <0.01) than to themselves. Meanwhile, in *Guided Agent* they still attributed an equal share to their agent (4.8), even though they have incentivized their agent`s trading choice.

Table 1: Results Agency and Voting

	Total *N*	Keep	Trade	Fisher Exact 2-tailed
Baseline (Lab)	64	45 (70.3%)	19 (29.7%)	
Mandatory (Lab)	45	31 (68.9%)	14 (31.1%)	
Baseline (Online)	90	40 (44.4%)	50 (55.6%)	
Default (Online)	90	35 (38.9%)	55 (61.1%)	*p* =0.54
Mandatory (Online)	81	18 (22.2%)	63 77.8%)	*p*<0.01 *p*<0.03
Guided Agent (Online)	81	18 (24.7%)	63 (77.8%)	*p*<0.01 *p*<0.03
Voting (without Veto) (Online)	91	19 (20.9%)	72 (79.1%)	*p* <0.01 *p* =0.01
Voting with Veto (submitted votes) (Online)	88	13 (14.7%)	75 (85.3%)	*p*<0.01 p<0.01

(All *p*-values report comparisons to *Baseline* and *Default*; both Lab and Online results are reported)

In the voting treatments, subjects should debias and trade more often than in *Baseline* because each voter shares responsibility with the others. Whatever a voter's expectation of the majority decision`s outcome, each voter knows that his or her own vote is unlikely to be pivotal in this outcome. Therefore, each voter shares responsibility with the others.

Supporting the predictions I presented above, voting strongly increased subjects' willingness to trade: significantly more subjects, at 85.3% of the participants in *Voting with Veto* and 79.1% in *Voting without Veto,* voted to trade their ticket than in the *Baseline* treatment (55.5% *p*-value <0.01). In *Voting with Veto*, 88.6% (*p*-value <0.01) confirmed the majority's vote and traded their ticket; in *Voting without Veto* the majority rule was responsible for making everyone trade – a collective outcome completely uninfluenced by the endowment effect. Additionally, subjects in *Voting with Veto* reported that they expected to feel less responsibility (5.37) and regret (5.89) over trading than in *Baseline* (*p*-value <0.01). The results for *Voting without Veto* confirmed this effect.

In summary, this study shows that responsibility-dividing market institutions reduce or even completely drive out the endowment effect. The results support my general methodological claim that a legal- and policy-oriented analysis of potentially biased trading behavior must consider the institutional context in which trading decisions take place.

IV. Study 3 - Multiple Institutions Reinforce Debiasing

A. Theory

So far, I have demonstrated a strong debiasing effect for two separate institutions. When people trade, they often involve multiple institutions of private ordering in their sales. For example, they sell their goods in a strategic market to gain a profit and they may delegate carrying out the transaction to an agent. As I have shown above, both institutions have a strong debiasing effect and it is plausible to assume that multiple institutions may reinforce each other`s debiasing impact. Therefore, policy makers need to identify all relevant sources of debiasing involved in trading and analyze their cumulative impact on people's bias to get an estimate of the true extent to which loss aversion may affect private ordering. To demonstrate the reinforcement of debiasing in a multiple institution setup I presented subjects with both strategic markets and agency in my next experiment.

These two institutions interfere on different cognitive levels with the causal mechanism driving the endowment effect. Strategic markets reduce regret over selling because they lessen owners' fixations on their entitlements; agents, by contrast, decrease regret over selling by allowing the principals to share responsibility for the transactions. Tested separately, neither institution completely drove out the bias in the descriptive data (of course, the bias did not reach a conventional significance in the strategic market treatments); their joint impact however may eliminate the bias entirely.

B. Study Design

In the *Agency & Markets* experiment, I assigned each subject an agent. The subject could instruct the agent what price to demand, and the agent was incentivized to follow this instruction, as he was s paid €2 by the experimenter if he followed this instruction, and nothing otherwise. However, the agent was not bound to execute the instruction, and his decision as to whether to trade or keep the ticket was final. As basic structure for the experiment, I used the *Strategy* treatment with the modified BDM mechanism. As such, if subjects sold their tickets, they were paid the prices their agent demanded, as long as this demand did not exceed the random price.

C. Hypothesis and Results

As hypothesized, presenting the subjects with two institutions reinforced the debiasing impact, and drives out the endowment effect completely. With a WTA of €4.03 and a WTP of €4.18, the price gap was even slightly negative -€0.15 (*p*-value 0.72 two-tailed Mann Whitney; cohens` *d* -0.06; see also Figure 1). The bias in regret costs has also disappeared. Regret costs were almost balanced in both the *Endowment* condition (3.46 over selling their ticket and 3.66 over keeping it) and the *No-Endowment* condition (3.58 over choosing the ticket and 3.80 over choosing the money). Consequently, comparing the *Endowment* and *No-Endowment* conditions yields no bias (-0.02[36]; *p*-value=0.95 two-tailed Mann-Whitney). The difference between loss-averse and rational subjects established in the *Baseline* treatment of the *Strategic Markets* study has also vanished: I find no evidence of a bias for either subject type. For both the loss averse (€-0.29; *p*-value 0.61 two-tailed Mann-Whitney) and the rational subjects (€-0.03; *p*-value=0.88 two-tailed Mann-Whitney), the gap between WTA and WTP prices is negative.

The study suggests that, in order to reliably estimate the EE's actual relevance for private ordering, policy interventions must consider the interplay of all institutions involved in trading. Professionals typically employ many institutions when they trade, the most common being the strategic markets and agency tested in this study.

V. Study 4: Social Context and Multiple Reference Points

A. Theory

1. Herding and the Transparency of Markets

So far I have shown that market institutions are powerful sources of debiasing, driving out loss aversion. But markets may also have an indirect effect on debiasing. Either in the physical world or online, markets allow people to observe others' choices. For example, on sites such as eBay, Edmunds, or Craigs-list, property owners can easily access offers or transactions by other entitlement holders, both of real and personal property. Creators of intellectual property

[36] The negative sign indicates that, if anything, regret pushes subjects slightly towards trading.

have access to trade journals, online forums, and advisors providing information on transactions carried out by owners of similar entitlements. Information both on existing and proposed contract terms and their adoption by others is often available through trade organizations (Bernstein, 2015). Thus, decision makers operating in today`s information-technology-based markets can often learn the choices made by others with great ease. They do not have to decide alone, as solitary figures, but they make their decisions within an often rich social context.

Generally, the social context may affect sellers` choices in both directions: it could debias sellers, or alternatively insulate their biases. But in turn it could also induce biases they might not have previously had. The central assumption of my next study is that a large number of market participants will be unbiased when they act within the market: the process of trading itself is a strong debiasing mechanism as are the institutions that sellers typically involve in their transactions. The transparency of modern (online) markets may work as a catalyst for this debiasing effect. Decision-makers can easily observe other market participants like professional sellers or agents who carry out their transactions. Once a large enough group of decision makers acts in a market without being biased, the social context may reinforce the debiasing effect induced by the formal market institutions and the trading process. Under these conditions, the market herd towards rationality.

2. Attentional Focus and Reference Points

Focusing on the choices of these other market participants could change a biased decision-maker's reference point. Instead of referring to the status quo, participants may take the market choice of others as their reference point. To understand how herding can shift reference point and bias, it is necessary to analyze how reference points drive loss aversion and anticipated regret. As seen above, if the loss could have been avoided by making an alternative choice, decision makers will anticipate experiencing regret over a trade should it turn out badly. More specifically, regret theory suggests that they compare the expected outcome of the choice in consideration with the perceived benefit of the alternative choice. When people assess the opportunity cost of alternative choice options, they tend to focus on the more salient choice, taking it as their reference point (Carmon and Ariely, 2000; Pachur and Scheibehenne, 2012). The more salient choice is typically to maintain the status quo, as the existing state of the world is immediately available, while the state of the world led to by the alternative choice is often hypothetical, existing only in the imagination (Bordalo et al., 2012). Focusing on the status quo owners more easily construct the benefits of maintaining their ownership (Kahneman and Tversky, 1982). Therefore, when they decide whether to keep or trade their endowment, they anticipate feeling stronger loss aversion and more regret over selling and departing from the status quo (Johnson et al., 2007; Bhatia and Golman, 2012). Thus, as long as the status quo is the owners` most salient reference point, the bias restrains their trading choices.

By contrast, if the dominant decision in the market is to sell and an owner takes this choice as his or her reference point, then the owner more easily constructs the gains from trade.[37] Now, the bias should shift: the seller gives greater weight to the foregone gains from trade and anticipates less regret over selling the good than over retaining his or her endowment. Therefore, in contrast to the debiasing institutions I have analyzed so far, herding does not remove the bias. Rather, herding strips the bias of its costs by shifting it such that it pushes sellers to trade. As a result, the interplay of formal market institutions and herding behavior can reinforce institutions' debiasing impact on private ordering even further.

B. Study Design

I tested the indirect debiasing effect of herding with a series of experiments in the laboratory and online. The basic lottery design remains the same as in the previous studies: I endowed each participant with a ticket. The lottery contained two tickets, one marked "heads" and the other "tails", so each was equally likely to win the same payoff. As before, subjects could exchange their ticket for the second ticket in the lottery plus a bonus of 25 €-cent. I measured the trading choices and the regret subjects anticipated over their decisions. As in the earlier studies, a significant majority of subjects exhibited an Endowment Effect; 70.3% of the lab-subjects and 44.4% of the participants in the online treatment kept their tickets. As in the agency and voting experiment, fewer subjects traded in the laboratory than traded online, most likely because the subjects in the lab were endowed with physical lottery tickets.

In the *Herding* treatment, subjects were correctly informed that the majority of participants in a prior study decided to trade. The lottery design makes it very salient that the participants in the earlier study could not have had better information as to whether trading or keeping the good would turn out to be the better choice. Since the lottery outcome is random, subjects could not expect to learn anything from the participants' decisions in earlier sessions; put differently, their decisions carried no informational value. Control questions confirmed that the subjects indeed understood this central element of the study design. When asked whether they had learned anything about the potential outcome of their own decision from the other participants' choices, almost all subjects answered correctly that they could not. The control questions also confirmed the subjects' understanding that trading would always lead to the

[37] I will show later that sellers have a deliberate influence on what reference point they take.

better expected payoff. Most of the data with which the subjects were presented with was taken from the agency experiment, in which most subjects had chosen to trade (see above Arlen and Tontrup, 2015 a). The subjects received accurate information as to how many participants had traded and how many had kept their ticket, as well as to the fact that the data was collected in a prior experiment in the same laboratory, with subjects recruited from the same subject pool of which they were part. The experiment is structured to eliminate alternative motivations for herding, such as asymmetric information, expertise, peer pressure, and social approval (see generally for motivations for herding Thaler and Sunstein, 2008). For example, as no one else could have better information than theirs, or could have any expertise in predicting the lottery`s random outcome, subjects had no reason to rely on the majority's choice for its informational value. To rule out peer pressure and demand effects, the subjects made their decisions anonymously and in the isolation of a separated booth (or online), ensuring that the subjects were aware that they could not be observed by others.

To provide direct evidence, as in earlier studies, I asked subjects to report their levels of anticipated regret, should their ticket win the lottery, after they sold it and in the *No-Endowment* condition, the level of regret felt over choosing the money, when the ticket they could have selected instead should turn out to be a winner A ten point Likert scale ranging from 1 (very little regret over a negative outcome) up to 10 (very strong regret) provided the measure.

C. Hypothesis and Results

As described above, the subjects in the *Baseline* treatment displayed a strong endowment effect that is consistent with them using the status quo as their reference point. The subjects stated that they anticipated greater senses of loss and regret over trading than over retaining the status quo and keeping their original ticket. This evidence supports the theory that loss aversion causes them to give the greater weight to the benefit of the lottery ticket they held than to the alternative ticket.

In *Herding*, subjects' reference points should switch; the majority choice should turn out to be the dominant reference point when subjects decide whether or not to trade, pushing the status quo aside. Subjects who take "trade" as their reference point choice should experience more regret over keeping their original ticket than over trading it. As the decision to trade produces both higher expected welfare and lower anticipated regret, these subjects should exchange their original tickets for the second tickets in the lottery, thereby overcoming their endowment effect. Not everyone should trade, however. Since subjects are presented with two potential reference points for their choices, the status quo may remain the dominant reference point for some people who are particularly loss averse. These subjects should still exhibit an endowment effect.

Supporting the debiasing effect, subjects were significantly more willing to trade than subjects in *Baseline*: 55.1% of the laboratory participants traded their tickets, as compared to only 29.7% in the *Baseline* condition (Fisher two-tailed p <0.01). Similarly, 72% of the online subjects exchanged their tickets, compared with 55.5% in *Baseline* (Fisher two-tailed p-value =0.02).

Table 2: Herding as a Bias-Insulating Strategy – Trading Choices

	Total N	Trade	Keep	Fisher test 2-tailed *(compared to Base)*
Laboratory Study				
Base	64	19 (29.7%)	45 (70.3%)	
Herding	49	27 (55.1%)	22 (44.9%)	p<0.01**
Online Study				
Base	90	50 (55.5%)	40 (44.4%)	
Herding	82	59 (72%)	23 (28%)	p=0.02*
BSM: Strategic Herding				
Multiple Reference Points	91	64 (70.3%)	27 (29.7%)	p=0.04*
Seek Information	64	50 (78.1%)	14 (21.9%)	p<0.01**

(all p-values report comparisons to *Baseline*)

Subjects also reported anticipating significantly more regret over trading and thereby losing a potentially winning ticket, than over keeping their tickets forging the change to win a prize with the alternative ticket (6.40 versus. 5.12; Mann-Whitney p-value <0.01; differential regret -1.28), evincing a bias towards exchanging their ticket. By contrast, subjects in *Baseline*, anticipated to experience more regret over trading and losing a potentially winning ticket, than over keeping their tickets even though they might gain the winning ticket by trading (6.5 versus 7.2; Mann-Whitney p-value =0.02; differential regret 0.7). In standard endowment effect experiments, the only reference point for the subjects` trading decisions is the status quo, making ownership and possession salient. The social context of real markets, by contrast, will often provide competing reference points. For some owners, the status quo will remain the dominant reference, but many others will instead focus on other market participants taking the other participants' behavior as their reference point.

The results show that social context and herding may thus substantially reinforce the impact of the debiasing effect caused by market institutions, even though they do not remove the bias.

VI. Study 5: Social Preferences Reduce Loss Aversion

A. Theory

My first paper has demonstrated that strategic markets can redirect sellers` focus and thereby lessen their loss aversion. The next study wants to show that social preferences triggered by the transaction can have a very similar effect on people`s attentional focus. The study subject is the Anticommons dilemma. In this scenario, multiple owners hold effective rights of exclusion in a scarce resource. Heller and Eisenberg (1998), for example, describe the development of a new drug that often requires patents owned by many different firms. Then, even if all other patent holders license to the investor, each of these firms alone can withhold the development of the drug by excluding the downstream inventor from using their patent. Patent owners can try to exploit their position by collecting most of the profit they assume will accrue to the downstream investor. However, if all patent holders follow that strategy, their cumulative demand may exceed the expected market revenue of the drug, with the project then failing in spite of its efficiency. The research on the Anticommons dilemma shows that not all owners behave selfishly. They condition their own cooperation on the cooperative behavior of others (Fischbacher et al., 2001). When they expect the other owners to act selfishly, they are likely to exaggerate their prices too. Yet, when they expect the others to be cooperative, they are likely to consider the negative externalities their choices can have on the group of anticommons owners. Their behavior is guided by social preferences (D`hont, 2011). The Anticommons dilemma is an ideal study object to show that this bundle of social motivations eases loss aversion as it directs the attentional focus of the subjects from the status quo and their entitlements towards the interaction with the buyer and the other Anticommons owners (D`hont et al., 2012).

B. Study Design

To test this theory, the same basic lottery design presented above is used: a choosing design is implemented in the *No-Endowment* condition and a BDM mechanism randomly selects the market price. Subjects could request prices for their lottery tickets ranging from €0.25 up to €10. [38] In the treatments implementing the Anticommons dilemma, the BDM mechanism is modified to provide participants with incentives for strategic pricing behavior. The interdependence of outcomes between the owners` characteristic for the dilemma, is created by forming groups of three owners and one buyer. In the *Summed Price* treatment, trade and payment were conditioned on whether the randomly drawn offer price was equal to or larger than the sum of the prices asked for by the three group members. The random offer was drawn from an urn with prices ranging from €0.75 to €30. The group members sold their tickets if the sum of the sales prices requested by all three group members for did not exceed the random offer. In exchange for their ticket, each group member received the price he or she had personally demanded. Otherwise, if the sum of the three prices exceeded the random offer, the deal failed, and all group members kept their tickets. The design assures that the participants' outcomes are interdependent, because any group member strategically pushing the price increases the likelihood that the transaction might fail, destroying both the three sellers' and the buyers' gains from trade.

The *Maximum Price* treatment aims to isolate social and other regarding preferences' influence on the endowment effect. Subjects are not provided with incentives for strategic behavior, but their outcomes remain interdependent nonetheless. The treatment conditioned the trade on the highest price indicated by the three group members. To determine whether the trade is carried out all three selling prices are compared with a randomly drawn single offer price ranging from €0.25 up to €10. If no one group member indicated a price exceeding this offer, the subjects sold their tickets, and all then received the same random offer price. However, if one of the group members asked for a price higher than the random price, the transaction failed, and all group members kept their tickets. Thus, each subject can still impose negative externalities on the other group members, spoiling the trade for those who are willing to accept lower offers for their tickets.

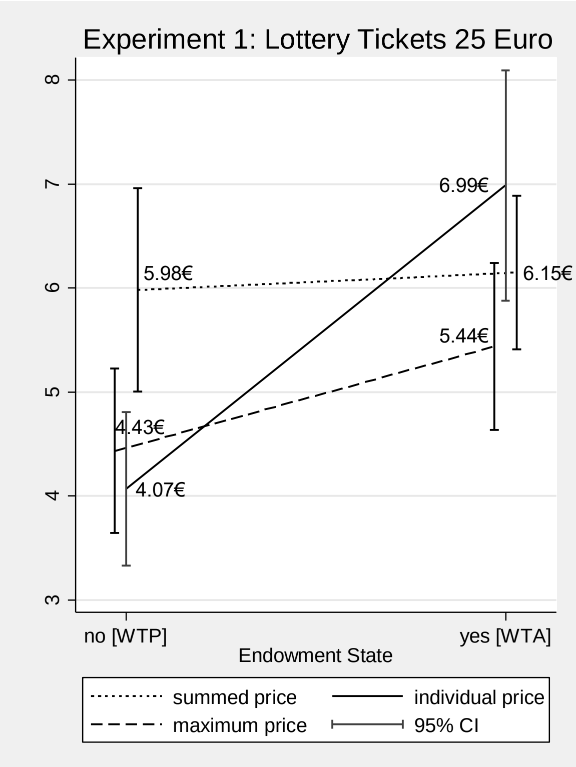
The *Individual Price* treatment generates a *Baseline*; the group members` payoffs are independent of each other and they are incentivized to reveal their true valuations for the tickets. The treatment is implemented with a standard BDM mechanism, so subjects received the random price, not the price they demanded. The offer price ranged from €0.25 to €10.

[38] To resemble a typical Anticommons situation with patents of unknown future value, the lottery contained 10 winning tickets worth €25 each, but the subjects did not know how blanks were in the urn. This design assured that participants had a rough estimate of the lottery tickets' value, but nevertheless a considerable uncertainty remained as to the actual and the winning probability of their ticket.

C. Hypothesis and Results

The Anticommons situation should trigger a bundle of selfish and social preferences, focusing the subjects both on the chance to push the prices more than the other group members and on their choices' negative externalities. Drawing the subjects` attention away from their entitlement and its valuation, the Anticommons should lessen subjects` loss aversion. The *Individual Price* treatment shows a significant gap of €2.92 between WTA and WTP prices (4.07 versus 6.99 Mann-Whitney *p*-value <0.01). In the *Maximum Price* treatment, the gap shrinks to a mere €1.01, a third of its original size (4.43 versus 5.44; Mann-Whitney *p*-value =0.08) and in the *Summed Price* treatment, finally, the bias disappears almost entirely: the WTA/WTP gap is clearly insignificant, measuring only €0.17 (5.98 versus 6.15; Mann-Whitney *p*-value 0.51).

Figure 2. Debiasing Effect of the Anticommons



A linear regression model yields an overall effect for endowment status of €1.41 (*p*-value <0.01). Two *Helmert* contrasts test for treatment effects. The incentive contrast captures strategic incentives' impact in the *Summed Price* treatment, comparing it with the mean of the *Maximum Price* and the *Individual Price* treatments.

Table 3. Regression Model of Ticket Prices

	Price in Euro (WTA / WTP) 25€ lottery
Endowment Status (yes=1 vs. no=0)	1.41** (3.85)
Incentive Effect (incentive=1 vs. no-incentive & individual=0)	0.83* (2.16)
Endowment*Incentive	-1.85 (-2.41
Interdependence Effect (no-incentive=1 vs. individual=0)	0.58 (1.30)
Endowment*Interdependence	1.91* (2.15)
Constant	5.51** (30.18)
Observations	97
Adjusted R^2	0.218

Note. Coefficients are unstandardized b's in Euro. *t* statistics are provided in parentheses, variables are centered, robust standard errors are used $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

By comparing the *Maximum Price* treatment, in which the subjects` outcomes are closely interdependent, with the *Individual Price* treatment, the interdependence contrast isolates the effect of social preferences and awareness of negative externalities. Interdependence had a highly significant influence on prices; consistent with the central hypothesis that social preferences affected subjects` attentional focus, it sharply reduced the endowment effect by €1.91 (*p*-value <0.01). As well, the incentive contrast turned out to be significant, with €1.85 (*p*-value <0.01) suggesting that, as predicted, the motivation bundle of strategic incentives and social preferences effectively eases loss aversion.

To further support the evidence, a second study included survey questions with Likert items querying the subjects as how strongly they cared about the other group members' outcomes. The Likert items allowed me to estimate the impact of subjects` other regarding preferences on their pricing decisions (Cronbachs α =0.72).

Regressing prices on endowment status, the participants' scores on the Likert item that captures their concern for others, and their interaction, demonstrated as predicted that other-regarding preferences reduced the endowment effect. Both the main effects and the interaction turned out to be significant. Thus, prices rose with endowment (b=2.30, t=5.66, *p*-value <0.01) and decreased with subjects` growing social concern about the other group members (b=-0.33, t=-4.13, *g* <0.01). The interaction effect reveals that the individuals concerned about the negative externalities their decisions might impose upon others demonstrated a significantly smaller endowment effect (b=-0.38, t= -2.39, *p-value* =0.02).

VII. Bias-Self-Management – Complementing Debiasing through Law

So far, I have shown that the trading task itself, the institutions and the social context that all constitute private ordering have a strong debiasing effect on sellers. However, there exists another source of debiasing that has been considered neither by the empirical nor by the legal literature: the biased decision-maker himself. When legal scholars assert that conquering loss aversion and the endowment effect requires external intervention into private ordering, the interventions they suggest often assume implicitly – or even explicitly – that entitlement holders have either no ability or no incentive to manage and overcome their own biases.

In the following, I want to present evidence that people are not only motivated to manage, but in fact can succeed in managing their biases. I developed a concept of bias-self-management that is new to the literature. The concept departs distinctively from the previous literature proposing strategies for self-debiasing (see for example Soll et al., 2016). That literature's goal is constructing and testing the effectiveness of optimal debiasing strategies, rather than identifying the Strategies that people actually use to make their choices. A typical study would present subjects with a cognitive protocol whose intended purpose is to reduce or remove the targeted bias. For example, in one study, corporate financial officers were asked to forecast yearly returns for the S&P 500. Their 80% intervals (e.g., "*I am 80% sure that the return will be between 2%-4%*") captured the correct values only 33% of the time, suggesting that the officers had been overconfident in their judgment. Soll et al. (2004) proposed to split the judgment into multiple percentiles and Jain et al. (2014) suggest forecasting a whole time series, considering the return after one month, after three months, and so on. Both methods are intended to cause subjects to develop a better sense of the true degree of uncertainty involved in the judgment, in order to reduce the subjects´ overconfidence.

Apparently, this type of study only aims to advice people how they *could* debias if they learned about the suggested protocols. It does not provide any evidence that a) people are aware of their own bias and b) are, in fact, employing the suggested debiasing strategy in real life without external intervention.[39] In contrast, my legal policy goal is to show that people actively use private ordering's institutions and social context to manage their biases, thus deliberately improving their decisions and payoffs.

As before, I use loss aversion and the endowment effect as my test case. It is far from self-evident that subjects can manage to reduce or even remove their loss aversion. If endowment status boosts people`s true valuations of their property beyond potential buyers' WTP, it obscures existing gains from trade. In exchange for their property, owners may be able to obtain a good that they would value more than their current property after coming to own it. However, because they possess only their own good and not the other, they might not realize that they would prefer the second over their own good if they had both of them in their possession. As such the endowment effect hides the gains from trade.

Regret theory can explain why owners may be both aware of their bias and motivated to engage in bias self-management. Owners experience regret costs over giving up their property. The regret costs are triggered by their loss aversion. They can remove this disutility by keeping their entitlement, or they can compensate the regret costs by asking for a higher sales price. However, whenever trading is beneficial absent regret costs, they have an incentive, to employ strategies to reduce the disutility. Thus, even when owners have no other reason to transact through market institutions, they may voluntarily rely on agency in order to self-debias. Private ordering may therefore be able to reach better allocations by inducing private debiasing.

Behavioral Law and Economics scholars typically separate three types of interventions into private ordering concerning judgment and decision-making errors: (1) insulating biases from affecting outcomes (with many examples Hanson and Kysar, 1999 a, b), (2) debiasing (Jolls and Sunstein, 2006) and, (3) nudging (Taylor and Sunstein, 2008). An insulating intervention seeks to prevent people from acting upon their bias, thereby aiming to stop the bias from becoming consequential. For example, when consumers tend to overestimate their expertise in handling a dangerous product, a policy forbidding use of the particular good prevents people from acting upon their bias, but it does not remove that bias. In contrast, a debiasing intervention seeks to enable individuals to make an unbiased choice, ideally without directing their decision towards a specific outcome. An example is using natural frequencies instead of conditional probabilities when informing patients of the risks and benefits of treatment; natural frequencies help patients and doctors to not fall prey to biases like base rate neglect, allowing them to make a better-informed decision

[39] Often the studies do not even demonstrate that people would be capable to employ the given strategy on their own.

about pursuing treatment (for a numerical illustration see the footnote and Gigerenzer et al., 2007)[40].

Finally, nudging aims to change the choice architecture in a legal context such that biased parties are pushed to make better decisions, whether measured by their personal welfare or by a social standard. An example of a nudge that aims to promote a public good is switching the default for organ donations. When the default treats a person subject to the law as a non-donor, thus requiring individuals to affirmatively indicate their intent to become a donor, not donating is their reference point. Therefore, the individuals focus on and can more easily construct the attributes of not being a donor, such as potential medical uncertainties around the determination of their death or keeping their body "intact" after dying. By contrast, when the legal default treats individuals as donors, people focus on the advantages of being a donor, potentially saving multiple lives with their donations (Johnson et al., 2003). Depending on how the default is set, the rate of those willing to donate increases dramatically, even if countries as similar as Germany (12%) and Austria (99%) are compared.[41]

In the following studies I want to present evidence for my main argument that, to improve their decision-making, private parties employ the same three strategies the Behavioral Law and Economics literature suggests lawmakers should engage: (1) they self-debias; (2) they insulate their biases; and (3) they nudge their future-selves. I will present one study for each of the three strategies of bias-self-management. The first study analyzes a case of self-debiasing.

VIII. Study 6: Self-Debiasing - Agency and Voting

A. Theory

Above, I presented evidence that principals can debias by employing the institutions of agency and voting in their trading. Both institutions assume some

[40] An example is reported by Casscells et al. (1978): In their study, medical professionals from Harvard Medical School were given a fairly simple diagnostic problem presented in terms of conditional probabilities: "If a test to detect a disease whose prevalence is 0.1% has a false positive rate of 5%, what is the chance that a person found to have a positive result actually has the disease…?" A vast majority of the participating doctors neglected the base rate – that is the low prevalence of the disease in the overall population - their most common answer being 95%. Only 18% of the professionals inferred the correct value. By contrast, a presentation of the same problem in the format of natural frequencies preserves the base rate information and makes the correct inference obvious: 1 in 1000 persons has the disease. Of these 1000 persons, 50 get a positive test, even though they are not ill. Now the answer is obvious: the chance that the positively tested patient has the disease is a mere 1 in 50. Apparently, treatment decisions will be hugely affected when doctors and patients assume the probability to be 95% instead.

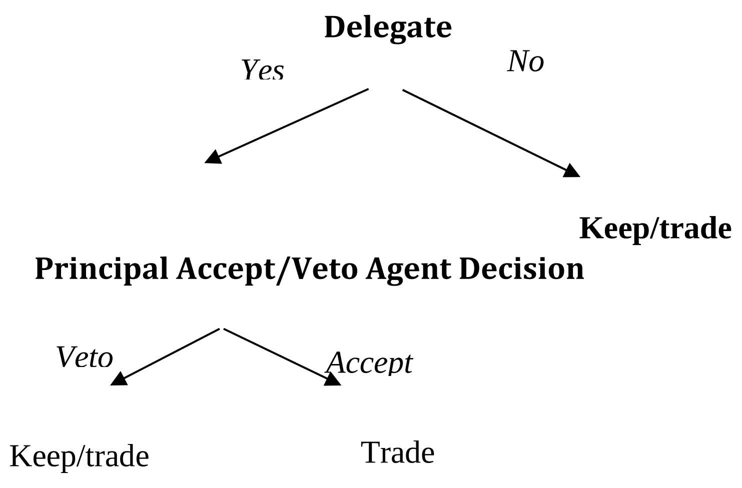
[41] Another reason for the large impact of the default may that that people want to avoid thinking about their own death. Therefore, they may accept either default to avoid an active decision (Johnson 2006).

responsibility for the trade, thereby allowing the owner to enter a beneficial transaction at lower or no regret costs. The regret property owners anticipate to experience over trading suggests that they can be aware that their endowment creates a bias that may keep them from selling. In order to overcome this bias, they may deliberately involve agents and voting procedures to sell their property at lower or no regret costs.

B. Study Design

To test this theory, I offered subjects the option of either deciding on their own or involving a costly institution. In the first treatment, *Optional Agent,* each subject could incur a cost to delegate the trading decision to an agent. In the second treatment, participants could delegate to a majority vote in exchange for a fee. The *Baseline* treatment (design presented above) serves as benchmark to analyze whether subjects` self-debiasing is effective. I conducted the experiment online and in the laboratory. The basic setting is the same as before. Each subject was endowed with a lottery ticket marked either "heads" or "tails". Subjects won €4 if the ticket they held at the end of the session matched the outcome of the lottery; if not, they received nothing. Trading their ticket against the second ticket that had the alternative symbol earned the subjects a bonus of €0.25.

Figure 3. Decision Tree for Principals



The structure of the new experiment follows the *Mandatory* treatment. One subject acts in the role of a principal and another in the role of an agent. This new experiment differs from *Mandatory* in that the principal can voluntarily involve the agent in the trade (the agent is not mandatory). Accordingly, the experiment is referred to as *Optional Agent*. In the first stage, subjects could either choose between trading or keeping the ticket on their own, or they could alternatively decide to delegate the decision to an agent. Subjects who delegated could still accept or veto the decision of their agents. If they vetoed, they retracted full decision authority and could decide themselves whether they wanted to trade or not (see Figure 3). To ensure that unbiased subjects should not delegate, a cost on delegation was imposed. The laboratory subjects were informed that a decision to delegate would increase the experiment's duration by 10 minutes, which prolonged the study considerably from a total of 15 to a total of 25 minutes. Online participants were instructed that delegation would cost them 20% of their gains from trade. Additionally, I asked participants to indicate their WTP for involving an agent.

As in the *Mandatory* study, subjects were instructed that the agent would receive his €2 payment only if the agent traded the principal's ticket for the alternate ticket. Since principals could with large certainty predict that their agents would trade, this strong incentive ruled out curiosity on the principal's side as a reason to delegate and ambiguity aversion as a reason not to delegate. Control questions confirmed that almost all principals indeed expected their agents to exchange the tickets. Additionally, I informed principals that they would learn their agent's choice in any case – that is, independently of whether the principal decided to delegate or not. To eliminate any confounding effect of other-regarding preferences, the subjects were instructed that their agent would receive his or her payment of €2 even should the subject not delegate or veto the agent's initial decision.

A within-subject design tests whether subjects in fact deliberately delegate the trading decision to their agent in order to self-debias. Participants were instructed that they would complete two separate experiments, one of which would afterwards be randomly selected to determine their payoff (strategy method). Subjects first completed the *Optional Agent* treatment as described above. Once finished, they received new instructions presenting them with the *Baseline* treatment.

A switching choice pattern is supposed to reveal participants who delegate deliberately in order to debias: they should trade in *Optional Agent*, but keep their tickets in *Baseline*, in which they have to decide on their own. This test is conservative, as subjects who trade in *Baseline* may nevertheless benefit from delegating to their agent*:* they may delegate to trade at lower regret costs.

C. Hypotheses and Results

I described the general tendency of individuals to divide the responsibility for an outcome among all actors in a causal chain, attributing the main responsibility to the affirmative action closest to the final outcome (Spellman, 1997). Therefore, even though the principal deliberately delegates to the agent with the firm expectation that his or her agent will trade, the principal in *Optional Agent* should still assign primary responsibility to the agent carrying out the trade. Should the principal subsequently decide not to veto the agent's trade, this constitutes an inaction, to which less responsibility is typically attributed (the so-called omission bias; see Baron and Ritov, 1994).

Table 4: Bias-Self-Management – Self-Debasing

	N	Keep	Trade	Fisher Exact 2-tailed
Baseline (Lab)	64	45 (70.3%)	19 (29.7%)	
Optional Agent – Delegation YES (Lab)	43/ 82 (52.4%)	6 (13.9%)	37 (86.1%)	p <0.01
Optional Agent – Delegation NO (Lab)	39/ 82 (47.6%)	17 (43.5%)	22 (56.5%)	p =1
Baseline (Online)	90	40 (44.4%)	50 (55.5%)	
Optional Agent (Online)	38/ 82 (46.3%)	23 (28%)	59 (72%)	p =0.02
Debiasing by Delegation Base-line keep and Optional Agent Trade (within-design) (Online)	21/ 82 (25.6%)	21 (53.3%)	17 (44.7%)	p =0.87
Optional Voting with Veto (Online)	41/ 90 (45.5%)	13 (14.8%)	75 (85.2%)	p <0.01
Optional Voting without Veto– (Online)	91	19 (20.9%)	72 (79.1%)	p<0.01
Debiasing by Delegation Base-line keep and Optional Voting Trade (within-design) (Online)	16 /90 (17.7%)	16 (43.3%)	21 (56.7%)	

(all p-values report comparisons to *Baseline* and *Default*)

The results confirm the hypothesis that subjects delegate in order to self-debias: almost half of the subjects decided to delegate the trading choice to their agent, and of these subjects almost 90% did, in fact, trade. The evidence generated by the within-subject design suggests that 44.7% of the subjects used their agents to debias, keeping their ticket in the *Baseline* treatment but trading in *Optional Agent*. In addition, participants who delegated expected that involving the agent would reduce the responsibility (2.19 versus 0.97; Mann-Whitney p-value =0.05) and regret (1.22 versus 0.82; Mann-Whitney p-value = 0.43) felt over trading significantly more than non-delegators expected their feelings of responsibility and regret would be reduced by involving the agent. The reason is that delegators attribute significantly more responsibility for the trade to the agent (6.32, Wilcoxon *p*-value <0.01) than to themselves (4.9).

D. Study Design – Optional Voting

As it allows them to reduce regret by sharing responsibility, people should also be willing to use a voting procedure for debiasing. To test this claim, I first asked the participants to imagine that, in exchange for a small cost, they could delegate the trading choice to their session's participants, who would decide by majority vote. I also queried participants as to whether they would accept or veto the trade of their tickets, assuming that the majority of the participants would opt to trade. To prevent subjects from delegating because of thinking they might be able to learn something from the majority vote (recall that learning is impossible because the lottery is random), participants were instructed to assume that, even should they not decide to delegate and make the trading decision on their own, they would learn about the vote's result before making their trading decisions.

E. Hypothesis and Results

Self-debiasing implies that subjects should deliberately involve the voting process allowing them to trade more often and at lower costs. Indeed, 45.5% of the participants preferred to delegate to the majority vote with veto instead of deciding on their own. Only 7.3% of the subjects who indicated they would delegate expected that they would have vetoed a majority decision for the trade. In total, 75.6% of the participants in the *Optional Voting* treatment indicated they would trade—which is significantly more than the 55.6% who exchanged their tickets in the *Baseline* treatment. The results suggest that the voluntary access to market institutions such as agency and voting procedures significantly increased trading, having a debiasing effect similar to the impact measured for mandatory institutions.

IX. Study 7: Insulating Biases – Shifting Reference Points

A. Theory

Debiasing is not an end in and of itself; individuals` goal is to make better choices, taking their own biases into account. Analogous to public policies that aim at depriving biases of effect, individuals often decide not to remove but to insulate their bias, such that the bias does not prevent them from making an otherwise preferable choice. I termed this strategy "self-insulating". The next study is an example of this strategy. The experiment shows that, when anticipated regret burdens an owner`s sale, the owner strategically accesses market information, hoping that the other participants have traded. In that case, the other market participants' choices provide the owner with a potentially superior reference point. The new reference point shifts their focus of attention from the status quo to the sale's benefits, which allows them to trade without regret costs. By switching their reference points, owners do not remove their biases, but they shift them such that it pushes them towards entering the beneficial sale: with the crowd's decision to sell as their reference point, losing the gains from trade causes them more regret costs than giving up their entitlement.

The assumption that decision makers are not bound to treat the status quo as their reference point is central to the study. The reference point is assumed to be an endogenous element of the decision-making process, such that people can, to some degree, deliberately choose their reference points. The central assumption is that individuals, by focusing on others' choices, can switch their own reference points. Of course, choosing an alternative reference point does not necessarily make the decision-maker better off; this is only the case if the new reference point pushes him or her to a superior outcome. Markets can provide such a superior reference point, because, as the first study has suggested, the trading task largely debiases market participants by focusing them on the gains from trade. Additionally, many markets are dominated by professional traders and businesses, who tend to make decisions through agency relationships and voting procedures. Thus, these markets are less likely to be affected by loss aversion, as shown by the second study. If individuals take the choice to trade as their reference point, the evidence suggests they can more easily construct the benefits of the gains from trade. Accordingly, these individuals should anticipate more regret over retaining the status quo and keeping their entitlement than over selling it.

The expectation is that people to herd strategically. When biased towards a suboptimal choice, individuals will seek information on others' decisions, trying to improve their expected welfare with this new reference point. By contrast, when their reference point leads them to an optimal choice, they have no interest in accessing the same information from others. However, markets will often provide more than just one or two reference points. They are populated by different types of decision-makers, some more likely to be biased than others. Some are professional dealers who trade regularly; others operate within organizations and decide through institutions. Some transact infrequently and decide alone. If people have the ability to select their reference point strategically, they should focus on the particular group who, in their perception, would lead them to the optimal outcome. Typically, the literature conceptualizes herding as following the dominant behavior occurring in the overall market. By contrast the ability to deliberately take particular sub-groups as their reference –point would allow individuals to use herding as an effective behavioral strategy to improve their outcomes.

B. Study Design

To test the theory that people can self-insulate their loss aversion by deliberately shifting their bias, I implemented an online experiment with two main treatments: *Seek Information* and *Multiple Reference Points*. *Seek Information* uses the same basic design as the *Herding* study presented above, except for that subjects were not automatically informed about the majority choice made by subjects in the prior experiment. Instead, participants were offered choice to either obtain the information or decide without it. To ensure that subjects sought the information only to improve their decision and payoff, subjects had to incur a transaction cost if they wished to gain access. In this case, they had to wait one minute until the data was displayed; by contrast, if they did not want access, they directly proceeded to the trading decision. Because they needed to press a button within three seconds after the minute was over, participants had to stay focused; otherwise, the experiment was terminated. This design is ecologically valid, as people face similar transaction costs when researching others' market choices in reality. The waiting time, is calibrated in accordance to the amount a subject would earn in an average student job (~10€). With the one-minute delay, the opportunity cost is kept below the €0.25 bonus the study offered subjects for trading.

The control questions confirmed that subjects understood that the other participants whose choices the subjects were informed of could not have had any superior knowledge as to whether trading or keeping the ticket would lead to a better outcome in the lottery. As well, the control questions confirmed that subjects understood that their choices would remain completely anonymous, ruling out motivation by peer pressure.

The second treatment of the experiment, *Multiple Reference Points,* presented the subjects with the trading decisions of two distinct groups of subjects who had made opposing decisions as to whether or not to trade. Subjects learned that the first group of participants had decided on their own whether or not to trade, and that 70% of these participants kept their tickets, while 30% sold them. The subjects were informed that participants in the second group could either accept or reject the decision of an agent; in this group, the majority of the participants, 77%, confirmed the agent`s decision to trade and exchanged their tickets, while 33% kept them. The instructions pointed out that the agent could not have any better information about the lottery's outcome than that possessed by participants in the principal's role.

In this setting, subjects could focus on the majority choice of either one of the two groups, taking this choice as their reference point for making their own decisions. If subjects are only able to aggregate the information across the groups, rather than being able to selectively refer to one of the two groups, they would face an almost equal split of trading and keeping. This would leave them with no salient reference point choice besides the status quo. In this case, they would be expected to make similar trading choices as in *Baseline*. They may also refer to one of the groups by chance. In this case they should be less likely to trade than the subjects in *Herding* Only if subjects are able to focus specifically on the group whose majority had decided to trade should the subjects be capable of switching their reference point from “keep” to “trade,” just as effectively as the subjects in the *Herding* experiment who are presented with only one group in which by a large majority most participants had decided to trade.

In both treatments, two main variables were measured: subjects’ choices to seek information and their actual trading decisions. In addition, I asked subjects to report the regret they expected to experience over a potential loss following their trading decision.

C. Hypothesis and Results for Seeking Information

Rational actors cannot benefit from seeking the trading information about the other subjects` choices; because they can sell their ticket without facing regret costs over the trade. By contrast, a biased actor can. If anticipated regret would otherwise deter him from a beneficial trade, this information provides him with the largest benefit allowing him to sell the ticket instead of keeping it. Yet, even those participants who would have traded experiencing lower levels of regret costs, could also benefit from the information: switching their reference point may enable them to trade without any regret costs at all. Whenever regret costs exceed the opportunity cost of the one-minute waiting time, both types of subjects should obtain the information.

Not all subjects are expected to seek information, however. For example, not only rational types whose initial reference point is “trade”, but also participants who only have a small bias may expect to not sufficiently benefit from shifts in their reference points. On average, in comparison with *Baseline,* subjects should both be more likely to trade and to experience a lower level of regret over selling.

The results support the theory: even though access is costly, half of the subjects (32; N=64) sought the information (Fisher Exact test p-value < 0.01). The vast majority (90.1%) of these subjects also traded their tickets. As a result, subjects were significantly more likely to trade than in *Baseline* (78% versus. 55%; Fisher Exact test p-value <0.01). To further support the central claim that subjects seek the information in order to trade, I asked participants in a post-experimental questionnaire whether they would have traded without having this information; 46.8% of those subjects who decided to obtain the information and then traded indicated that they would have kept their ticket if they had not acquired the information.

Table 5: Bias-Self-Management: Bias-Insulating

BSM: Strategic Herding	Total N	Trade	Keep	Fisher test *(compared to Base)*
Base	90	50 (55.5%)	40 (44.4%)	
Herding	82	59 (72%)	23 (28%)	p=0.02*
Multiple Reference Points	91	64 (70.3%)	27 (29.7%)	p=0.04*
Seek Information	64	50 (78.1%)	14 (21.9%)	p<0.01**

(all p-values report comparisons to *Baseline*)

The regret item measured also supports the theory, which predicts, that the more subjects expect the information on others' prior trades will reduce their anticipated regret over trading, the more likely should they be to obtain that information. Indeed, the difference [regret over trading without Info] – [regret over trading with Info] is significantly larger for the subjects who sought information than for those who decided not to seek access (2.26 vs1.09; Mann-Whitney p-value =0.08). Additionally, subjects who had sought the information that the majority traded reported that they expected to experience significantly less regret over trading than if they had traded without this information (5.49 versus 7.52; Mann-Whitney p-value <0.01). The results also show a strong treatment effect in comparison to *Baseline*, in which subjects were not provided with an alternative reference point. As predicted, if they assumed that they learned the majority traded, subjects anticipated experiencing significantly less regret over trading than in *Baseline* (5.49 versus 7.2; Mann-Whitney p-value <0.01).

D. Hypothesis and Results – Multiple Reference Points Treatment

For rational actors, the multiple reference point treatment should present no change in comparison to *Base*; as their choices are not reference dependent, they should always trade. Loss averse actors, by contrast, should trade only when they can select the group acting and selling through an agent as their reference point. If, as expected, subjects can rationally pick out their reference points, they should not only be more likely to trade than in *Baseline,* in which the status quo was the only reference point provided. Subjects should also trade equally often as in the *Herding* treatments, as subjects should be able to focus their attention on the (same) optimal reference point in both treatments. In turn, if subjects would not be able to make a deliberate choice, they should be less likely to trade, as two salient reference points - the status quo and the participant group that kept their ticket deciding without assistance – would push the subjects to hold on to their ticket, making it less likely that subjects end up with the optimal reference point by chance. In *Herding* by contrast only the status quo suggests to keep the entitlement.

First, the results that *MRP* subjects were indeed more likely to trade than subjects in *Baseline*: 70.3% of the *MRP* participants traded, while only 55.5% of the participants in *Base* did so (Fisher Exact test *p*-value =0.04). Supporting the claim that subjects can rationally select their optimal reference point, subjects in *MRP* were as equally likely to trade as the subjects in *Herding* (70.3% versus 72%; odds ratio =0.92; confidence interval 95% lower bound: 0.56 compared to confidence interval with perfect odds ratio 1 = 0.53; difference <10%). These results suggest that subjects can pick out the optimal reference point just as effectively as in *Herding,* rendering improbable the alternative hypothesis that subjects` attention focuses randomly on one or the other group, taking either "keep" or "trade" as their reference point by chance.

The data on regret are also in line with predictions. Subjects in *MRP* expected to experience less regret over trading than over keeping (4.9 versus 6.23, Mann-Whitney *p*-value <0.01), suggesting that the participants, by focusing on the group trading with an agent, managed to take a reference point of "trade" Additionally, the predicted treatment effects are mirrored in the regret data: subjects anticipated significantly less regret over trading than in *Base* (4.9 versus 7.2; Mann-Whitney *p*-value <0.01), but not more regret than in *Herding* (5.2), suggesting they were able to deliberately select the optimal reference point.

X. Study 8: Self-Nudging – Employing Loss Framed Contracts as a Commitment Device

A. Theory

So far, I have presented evidence for two separate types of bias-self-management: self-debiasing, which aims to remove the bias; and bias-insulating, which tries to prevent the bias from causing harm without removing it. Now, I will focus on the third type of bias-self-management, the analogue to nudging (see Thaler and Sunstein, 2008). The idea behind self-nudging is that, in order to push their future self towards a superior outcome, people will purposefully exploit their own bias. As an example, I will analyze loss-framed contracts, which parties can employ as a commitment device to improve their future selves` productivity.

Contracting parties have always used a variety of instruments to influence each other's performances. Often, they rely on plain incentives and provide bonuses or stipulate penalties. More recently, however, they have tried to exploit their workers' and partners' biases to extract more effort from them (Madrian, 2014). Hossain and List (2012), for example, present evidence supporting the effectiveness of such loss- framed contracts.

However, loss-framing can also depress productivity if thresholds are not well-adjusted (Brooks, Stremitzer and Tontrup, 2017). The more distant perceived losses are from people`s reference points, the lower will be the value individuals place on them (Kahneman and Tversky, 1980). Thus, when contract drafters push the threshold too high, the loss frame will reduce, rather than spur, work effort. A low threshold, on the other hand, dampens performance, because workers conform to it by lowering their productivity (Brooks, Stremitzer and Tontrup, 2017). An attempt to exploit loss aversion might thus backfire. Self-nudging, by contrast, builds on party autonomy. For workers who are sophisticated about their biases, loss-framed contracts can be beneficial, as preferences about work are often inconsistent over time; moreover, when engaged in a work task, individuals frequently prefer a lower-effort choice than both before and after performing that task (Augenblick et al., 2015, Cadena et al., 2011, Kaur et al., 2010, Sadoff et al., 2015). Pervious work has shown that individuals can be aware of this time inconsistency, and thus try to nudge their future self into working harder by entering performance contracts (Giné et al., 2012, Hsiaw, 2013, 2015, Kaur et al., 2015, Royer et al., 2015, Schwartz et al., 2014, Imas et al., 2017 and Quidt, 2014). Self-nudging suggests that workers can provide their future self with additional performance incentives by employing their own bias in choosing a loss-framed contract. If this claim proves correct, then contract drafters can improve their own outcomes as well as those of their workers by offering a choice of loss- and gain-framed contracts, stipulating different thresholds for workers to use as effective commitment devices.

B. Study Design

The experimental design testing this claim offered subjects the opportunity to enter into a contract obliging them to complete a real effort task. Participants could also reject closing the contract. The task presented participants with tables containing digits between 1 and 9, which subjects had to count. When they completed a table successfully, they could decide whether to stop or to continue with the task.

The treatments varied the thresholds, ranging from a *Low Bar* contract specifying a threshold of 5 tables, a *Stretching* contract with a threshold of 15 tables, to an *Extreme Effort* contract with 50 tables. As a benchmark, I implemented a *Baseline* treatment offering a gain- framed *Linear* contract that did not express a threshold. Across all treatments, subjects earned €1 for each table they finished, regardless of the threshold specified. The loss frame endowed the participants with an ex ante payment. For example, in the *Low Bar* treatment, this was €5 and €50 in the *Extreme Effort* treatment.

Self-Commitment is the main treatment. It offered participants a choice between the three threshold contracts – *Low Bar*, *Stretching* and *Extreme Effort* – as well as the *Linear* contract. If they wished to enter a loss-framed contract, subjects had to pay a fee. In addition, I elicited how participants expected to perform under each of the contracts to analyze whether, if they chose a loss framed contract, they did so to improve their productivity. To support the findings with direct evidence, a loss-aversion measure (Fehr and Götte, 2007) was employed.

C. Hypothesis and Results

Subjects need to have two characteristics to benefit from a loss-framed contract. First, they have to be aware that they are present-biased. The time-inconsistent preference is their motivation for committing their future selves to performance levels they want to achieve but expect their future selves will fall short off; hence, the time-inconsistency presents the motivation for self-nudging. Loss-framed contracts lead subjects to a lower net-outcome, because gain-loss utility is negative below the threshold reducing the net-outcome of the same performance under the loss-framed contract compared to a gain-framed contract. However, for subjects with a present bias, negative gain-loss utility creates an extra incentive to comply with the contract – losses loom larger than gains – so overall, they expect to earn more as their future self will try harder to meet the threshold, attempting to prevent a loss. On the other hand, subjects with time-consistent preferences, lacking a demand for a commitment device, should principally be indifferent between the four contracts they are offered. However, as they would have to pay a fee for entering one of the loss-framed contracts, they should prefer the *Linear* gain-framed contract.

Second, subjects need to be loss-averse to be able to use the loss frame as a commitment device. Strictly rational subjects only respond to plain incentives; they are not sensitive to framing. As the experiment holds incentives constant across all contract types, unbiased subjects should be indifferent as to the choice between the four contracts subjects are offered. However, the loss-framed contracts cost an extra fee to enter. Therefore, subjects not sensitive to loss aversion should also prefer a *Linear* contract. Thus, better off choosing a loss-framed contract are only the subjects who are loss-averse *and* have a demand for self-commitment.[42] Note, however, subjects who cannot benefit from

[42] There is also a methodological reason why subjects that the measure of Fehr and Götte (2007) has classified as rational may benefit from a loss-framed contract. They may be loss-averse to a minor

the contract`s loss-frame may also use the threshold contract as a commitment device, since the threshold specifies a performance norm. When they fall short of this norm, subjects experience social disutility (Brooks, Stremitzer, Tontrup, 2017). Thus, by choosing a threshold contract, subjects can impose costs of social disutility on their future self, should it fall short of the selected threshold.

The data show that 80.3% of the subjects chose a loss-framed contract, suggesting a broad willingness-to-pay for self-nudging (Fisher test *p*-value <0.01).[43] The expectation measure reveals that 75.9% of the subjects (Fisher Exact test *p-value* <0.01) believe that they will be more productive under a loss-framed contract than under the *Linear* contract. Under the loss-framed contract anticipated by the subjects to lead them to their best performances, the subjects expect their future selves to reach a mean productivity of 22.3 tables. In comparison they expect their future self will complete only 11.8 tables performing under the gain-framed *Linear* contract (Wilcoxon *p*-value <0.01).

Loss aversion is a continuum, subjects can be more strongly, and they can be weaker loss averse. The loss aversion measure I used, allows me to exploit this variance. Subjects who are relatively stronger loss aversion, should possess a more efficient tool for self-nudging than the subjects, relatively less averse to losses: the nudge increases the costs of quitting for their future selves comparatively stronger. Accordingly, if subjects indeed consider their degree of loss aversion in their performance expectations, the stronger loss averse subjects should expect to benefit comparatively more from a loss-framed contract than those subjects the Fehr and Götte measure classified as less (or not) loss averse and the stronger loss averse subjects should also perform better. Even more, if subjects are consider the strength of their loss aversion in their contract choice, the stronger loss averse should – assuming they have the same demand for self-commitment to counter their present bias.

The results show, in total, 35 subjects choose the *Low Bar* contract (23.2%), 60 subjects pick a *Stretching* contract (39.7%) and 33 choose the even more costly *Extreme Effort* contract (21.9%). 34 subjects avoid the fee picking a *Standard* contract (22.5%). For non-parametric testing I classify subjects with regard to their loss-aversion into two types, individuals who are more averse to losses (i.e. subjects who rejected either both or one lottery) and individuals less or not loss-averse (subjects who accepted both lotteries). The more strongly loss-averse subjects were indeed (1) significantly more likely to

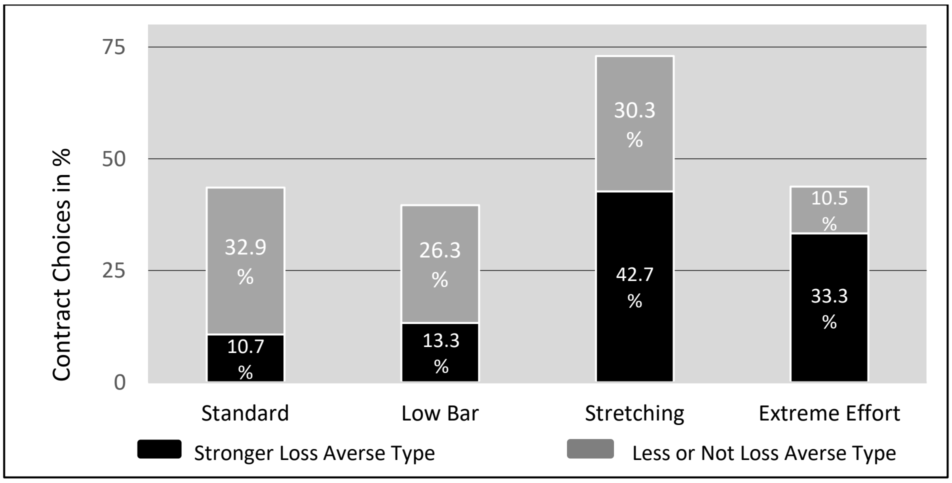
degree that their loss aversion measure is not sufficiently sensitive to discover. Their loss-aversion measure offers subjects the opportunity to participate in two lotteries with positive outcomes in expectations, but which if they turn out negative lead to a loss. As the measure allows only for a relatively broad classification, a subject that participates in both lotteries may nevertheless be loss-averse to some minor degree and benefit from choosing a loss-framed contract.

[43] 19.7% enter a *Low Bar* contract, 37.5% pick a *Stretching* contract and 23.2% the most expensive the *Extreme Effort* contract.

select a loss-framed contract than the less or not loss-averse group of subjects (89.3%; 67/75 subjects versus 67.1%; 25/76; Fisher 2x2 p-value <0.01) and they also chose significantly deeper loss-framed contracts (Fisher test 2x4 *p*-value <0.01). When estimating the marginal effects for each contract category in an ordered logistic regression, I find that with an increase in loss aversion by one unit, the subjects get (1) less likely to choose a *Standard* contract (dy/dx-0.110; *p*-value <0.01), (2) and get less likely to choose a *Low Bar* contract (dy/dx -0.060; *p*-value <0.01), while they get (3) more likely to choose a *Stretching* contract (dy/dx 0.055; *p*-value =0.02) and more likely to choose an *Extreme Effort* contract (dy/dx 0.115; *p*-value <0.01).

The results fit my theory: the *Standard* contract does not provide a self-nudging option, while the *Low Bar* contract with its low threshold is not a useful tool for self-nudging and improving one's productivity. By contrast, for both the *Stretching* and the *Extreme Effort* contract this is different and these contracts I see subject choose more often with increasing loss aversion; i.e. the stronger their nudge, the harder can and do subjects push their future-self. The results underline, that subjects appear to take the expected strength of their nudge into account when choosing a contract.

Table 6: Contract Choices by Contract and Loss Aversion Type



The results also support the second hypothesis: the stronger loss-averse subjects expected to improve their performance under their chosen loss-framed contracts, comparatively more than the less or not loss averse subjects.

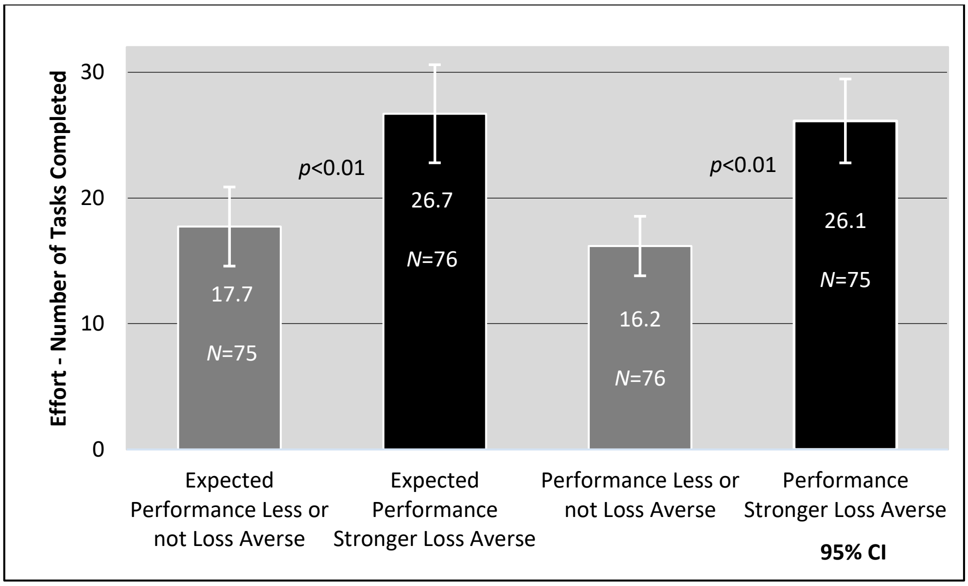
I find that the more loss-averse subjects expect to reach a significantly higher productivity of 26.7 tasks (*N*=75) under the contract they chose, compared to the less or not loss-averse subjects who expect to solve only 17.7 tasks

(*N*=76; Mann-Whitney *p*-value <0.01). The results are confirmed in an OLS regression that includes dummy variables for the chosen contract types, contract acceptance, and variables for age and gender.

The data also support my last hypothesis and show that the more loss-averse participants perform significantly better (26.1 tasks; *N*=75) compared to the less or not loss averse subjects (16.2 tasks; *N*=76; Mann-Whitney p-value <0.01). The result holds in the OLS regression.

The self-nudging strategy is also very productive in absolute terms: subjects' productivity in the *Self-Nudging* treatment exceeds the performance of participants in each of the single contract treatments.

Figure 5. Self-Nudging: Expected and Actual Contributions by Loss Aversion Type



The productivity in *Self-Nudging* (21.1 tasks; *N*=151) is significantly higher compared to performance under the most productive single contract treatment—i.e., the *Stretching* contract (14.3 tasks; *N*=54; Mann-Whitney *p*-value <0.01; with a control for loss aversion reg beta=4.092 *p*-value <0.01). The same holds obviously also for a comparison to the less productive loss-framed contracts *Low Bar* (6.2 tasks; *N*=50; Mann-Whitney p-value <0.01; reg beta=4.246 *p*-value <0.01) and *Extreme Effort* (8.4 tasks; *N*=39; Mann-Whitney *p*-value <0.01; reg beta=1.922 *p*-value <0.01). Finally, the productivity is also significantly higher compared to the gain-framed Standard contract (10.4 tasks; *N*=50; Mann-Whitney *p*-value <0.01; reg beta=4.921 *p*-value <0.01).

In sum, I find consistent evidence for my theory that participants possess the sophistication to anticipate their own time-inconsistent preferences and are prepared to use their own loss-aversion as a nudge for improving their performance:

(1) my data show that the more strongly loss-averse subjects choose contracts with higher thresholds and deeper loss frames, suggesting that subjects anticipate the strength of their loss aversion and consider it when they decide how deeply loss-framed the contract they choose should be.

(2) The more strongly loss-averse subjects also expect to perform significantly better, which again suggests that they consider in their expectations the degree of their loss aversion and thus the power of their self-nudge.

(3) The more strongly loss-averse subjects raise their actual productivity to a significantly higher level compared to less or not loss-averse subjects, showing that the more strongly loss-averse can indeed utilize their loss aversion more effectively.

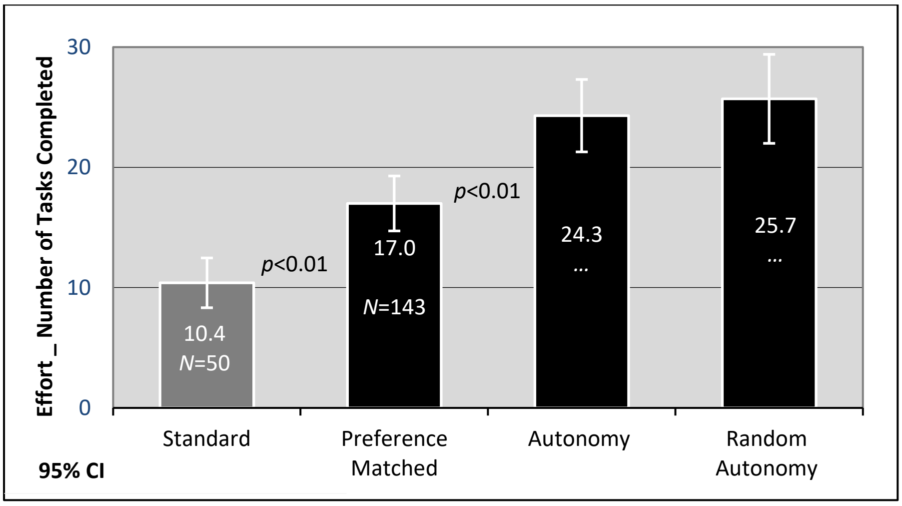
In a second experiment I want to demonstrate that the autonomy that self-nudging entails adds substantially to its efficacy. I aim to distinguish two performance-enhancing effects caused by this autonomy of self-management. First, autonomy enhances productivity by allowing workers to select or adjust tasks according to their personal preference—I will refer to this effect as "preference-matching." Second, the experience of autonomy in exercising a task may also foster intrinsic motivation. This effect I call the "Autonomy Premium." To my knowledge this study is the first to establish and separate an Autonomy Premium from effects caused by preference matching. Earlier studies restrict subjects both in their ability to choose according to own preferences and in their experience of choice autonomy (for example Dickinson et al. 2008, Moller et al. 2006 and Falk et al. 2006a). The design of these studies therefore does not allow for identifying what has caused the positive effect on productivity: is it that subjects can select the task best fitting their preferences? Or does the experience of self-determination raise their effort?

To isolate the Autonomy Premium, I use a novel identification strategy: In the Autonomy treatment subjects can choose among the same three loss-framed contracts as in the first study. The contracts establish thresholds that require subjects to complete 5, 15 or 50 tasks in order to fulfill the contract. Payment is piece rate and subjects receive bonuses for exceeding and penalties for falling short of the thresholds. In the Preference Matching treatment subjects are presented with the same three loss-framed contracts and are incentivized to reveal which of the contracts they prefer. Then subjects have to pick one of the three contracts blindly in a random assignment. The results of the treatments that present subjects only one particular contract without giving them an alternative option, allows to compare three conditions: (a) when subjects have no choice autonomy and are offered a contract that does not fit their preferences, (b) when subjects have no choice autonomy but are offered the contract they would have chosen themselves and (c) when subjects have choice autonomy and accordingly are able to themselves choose the contract they prefer.

Three hundred subjects participated in the *Preference Matching* treatment, of which 275 accepted a contract. The results support my hypothesis, as Figure 3 illustrates: subjects who accepted the contract and whose preferences were matched, reach a mean productivity of 17.0 tasks (*N*=144), while participants who were randomly assigned a loss-framed contract, they would not have chosen, complete only a mean 11.2 tasks (*N*=131; Mann-Whitney p-value <0.01). Results hold when we account for the 25 subjects who rejected the agreement (16.0 versus 10.0 tasks; Mann-Whitney *p*-value <0.01). The subjects whose preference were matched performed also significantly better than the N=50 subjects in the Standard treatment (10.4 tasks Mann-Whitney *p*-value <0.01) and the average of all *N*=143 subjects who were presented with a single loss framed contract (average of *Low-Bar*, *Stretching* and *Extreme Effort* 9.9 tasks, Mann-Whitney *p*-value <0.01).

To isolate the *Autonomy* Premium I compare participants in *Autonomy* with the subjects in *Preference Matching* who are assigned their favored contract. Self-determination utility should improve subjects' effort provision under all three loss-framed contracts ($H_{2.1}$).

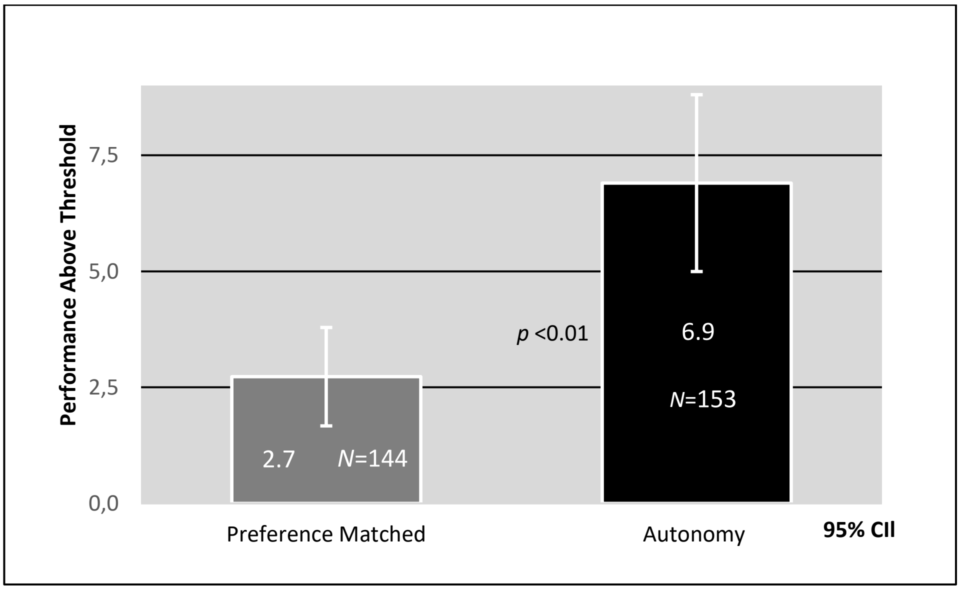
Figure 6: Autonomy Premium



Indeed, as illustrated in Figure 6, mean performance of subjects in Autonomy (24.4 tasks *N*=153) is higher than subjects' productivity in the *Preference Matching* treatment across all contracts (*N*=144; 17.0 tasks; Mann-Whitney *p*-value <0.01). The difference holds when I include the 27 subjects (*N*=25 in *Preference Matching*; *N*=2 in *Autonomy*) who rejected the contract (24.1 tasks vs. 16.0; Mann-Whitney *p*-value <0.01).

In support of $H_{2.2}$ I find significantly more subjects in the *Autonomy* treatment exceed the contract threshold (69.3%; 105 of 153), than in *Preference Matching* (45.8%; 66 of 144; Fisher test *p*-value <0.01). As expected, subjects in *Autonomy* raise their performance and solve with an average of 6.9 tasks versus 2.7 tasks (i.e. 1056 vs. 325) significantly more and three times the number of tasks above the threshold than the subjects in *Preference Matching* with their favored contract assigned (Mann-Whitney *p*-value <0.01). The significant performance raise above the threshold in the *Autonomy* treatment, supports an independent effect of the Autonomy Premium that cannot be explained by alternative behavioral mechanisms such as regret costs, cognitive dissonance or guilt aversion, as they all focus on the threshold but cannot push productivity beyond the threshold.

Figure 7: Performance Above Thresholds by Treatment



Isolating an Autonomy Premium has important implications for work contracts. While some firms can collect and analyze data that allow them to track employees and learn how to best match their production functions, an Autonomy Premium would elevate performance only when workers are in fact granted choice autonomy and experience self-determination utility. So even if companies adjust performance thresholds optimally, preserving or granting work autonomy could further improve work effort.

Indeed, in this experimental setting, the performance gain due to self-nudging and the Autonomy Premium was substantial. Achieving the same work effort with monetary incentives alone required doubling workers' payment.

I conclude that self-nudging not only improves productivity by enlisting subjects' loss aversion to commit their future-self to a targeted performance and by enabling individuals to select tasks fitting their personal ambitions and capacities. In addition, the experience of self-determination utility also fosters intrinsic motivation when performing a task. Our data show that this extra motivation increases participation and acceptance rates significantly beyond the rates reported for the commitment contracts offered by Gine and Ayres or even the soft-enrollment in retirement savings plans suggested by Benartzi and Thaler.

Table 5: Summary of Effects on Self-Nudging

Contract Choice, Expectation & Performance by Loss Aversion Type

			(p-values are two-tailed Mann-Whitney	
Self-Nudging	*N*	*p-value*	Stronger Loss Averse	Less or not Loss Averse
Choice of Loss or Gain Frame	N=151	$p < 0.01$ ($Z = 10.9$)	*26.13*	*16.18*
Performance Expectations	N=151	$p < 0.01$ ($Z = 3.72$)	*27.24*	*17.46*
Actual Performance	N=151	$p < 0.01$ ($Z = 4.56$)	*26.13*	*16.18*

*Only Subjects Who Entered a Contract

D. Implications for a Legal Concept of Bias-Self-Management

BSM can have significant advantages over direct policy interventions. BSM imposes costs only on those parties who are in fact biased, therefore choosing to overcome or to insulate their bias. For self-nudging, the bias serves as a costless commitment device. In contrast, an external policy intervention imposes costs on all affected parties, because the policy cannot identify those who are biased and address them separately from parties who are not (see Arlen and Tontrup, 2015a; Korobkin and Ulen, 2000; Rizzo and Whitman, 2009). For example, withdrawing a consumer product from the market, with the aim of protecting those who do not have sufficient expertise and diligence to handle it, also prevents people from using the product who have the required expertise and who would benefit from its use.

Nudging intends not to override, but to help people act upon their true preferences, as implied by the concept of soft paternalism (see Thaler and Sunstein 2008). As an external intervention, it nevertheless faces a similar information problem. Suppose, the law-maker considers a nudge, assuming that people make suboptimal choices in a particular area because they are biased. As the bias alters the behavior that the intervention is intended to remedy, the law-maker cannot derive people's true preferences from their actions beforehand. In turn, once the nudge has been implemented, neither will the actions of those successfully impacted reveal their true preferences, because the nudge has affected their behavior thereafter. For any approach of soft-paternalism, preference identification is intricate, causing a severe information problem. With individuals themselves recognizing their preferences and managing their biases accordingly, BSM avoids this problem.

BSM has further advantages. It relies on granting people choice and autonomy. Choice and autonomy foster intrinsic motivation (see Tontrup and Sprigman, 2022), while regulation that cuts off choice, intending to push people in a particular direction, can trigger opposition and reactance (see Arad and Rubinstein, 2015).

The studies I presented here show that, by providing decision-makers with institutions and social infrastructure, policy-makers can support and encourage BSM allowing decision-makers to manage their own biases and improve their outcomes. The examples I elicited original evidence for are, firstly, legally structured agency relationships, which enable decision-makers to overcome their loss aversion by sharing decision-making responsibility, secondly, enabling people to deliberately select superior reference points in markets, pushing decision-makers towards beneficial choices for example by regulating transparent trading platforms to support information flow.

Finally, I have shown that people can use loss-framed contracts for self-nudging. By employing their own loss aversion as a commitment device, people can nudge their future selves towards better performances and outcomes. Retirement savings plans that are supposed to raise the savings rate of present biased workers are an example of an area where loss-framed contracts can be used effectively to enable self-nudging. Workers could be offered loss-framed retirement savings plans, giving them an *advance* matching of their future savings goal; the advance is theirs to own, however, it can only be used for retirement. If workers fail to meet their savings goals, they would then lose these advance employer contributions. Because they get the matching contributions in advance rather than in the (distant) future, this plan design overcomes workers' present bias. Additionally, the workers` loss aversion pushes them to meet their savings goals. The current policy – offering present-biased workers an ex-post-match of the money they have actually saved – has proven ineffective to raise the actual savings rate of workers (see Chetty et al., 2013; Madrian and Shea, 2001). The reason is obvious: As the intervention promises rewards only for the future, the present biased workers who the regulation intends to address do only weakly respond to these incentives. Unbiased workers may not respond to the plan in an optimal way either: if they would have saved an optimal amount without the regulation, the matching contributions may now push towards saving more than optimal. This is a bizarre case of regulatory failure.

As a strategy of individual decision-making, BSM will often improve outcomes without any direct state intervention and the welfare costs of private bias-self-management will often be small. Of course, relying on agents and voting procedures or investigating other market participants' behavior can be costly. However, the associated costs mainly belong to the transaction costs that apply to businesses and organizations in general. In many cases, to trade successfully, sellers need to learn about the market's current prices and trends, or use agents to carry out transactions for them; also, the corporate structure of an organization might require collective decision-making. None of these are exclusive debiasing costs, they all fall under the category of general transaction costs necessary to maintain business. BSM imposes marginal costs only when institutions are used primarily to debias, for example principals would otherwise handle the transaction themselves, but they involve an agent in order to debias. But even in these cases, BSM would likely be efficient under the given circumstances, as BSM only imposes costs when the entitlement holder expects that the costs of employing an institution or investigating the market will be exceeded by the benefits of debiasing. Therefore, BSM would likely only be used by those expecting to derive a net-benefit from insulating, debiasing or self-nudging.

BSM underlines a central theme of the work that I have developed here: the importance of independent empirical legal research focused on law and policy interests. Many studies in psychology and economics try to construct effective decision-making heuristics and debiasing strategies, testing their performances relative to rational benchmarks (for an overview see Bazerman, 2017). For example, Roese et al. (2012) suggest an intervention to suppress the hindsight bias; they ask subjects to assume an alternative outcome and to think of a chain of events that could have led to this outcome. But Roese et al. neither analyze whether people are likely to be aware of their hindsight bias, nor whether they are already using a debiasing strategy without the researchers' intervention. Another example is Herzog et al. (2009), who propose a technique to increase the accuracy of predictions under uncertainty. After an initial best guess of a quantity (e.g., an adjustable mortgage rate in 5 years), they asked subjects to assume that their first estimate was wrong, to guess again, and to average their estimations. The two estimations are more likely to consider alternative assumptions and chains of events, cancelling out errors, when the estimations are averaged. However, the authors do not present any evidence that real decision-makers employ this strategy. BSM, by contrast, is tailored to be policy-relevant, because it identifies and analyzes the effectiveness of the strategies that people in fact use to manage their biases. Legal policy is concerned with the difference between actual and optimal private behavior; an intervention only becomes sensible if the gap between the two leaves large enough room for improvement. People using BSM strategies, even if not optimal, may suggest that external intervention is not needed or may not lead to an improvement sufficient enough to justify the regulatory costs. By enabling people to use BSM the law-maker may be able to improve policy outcomes without invasive interventions, causing fewer costs and side-effects.

Of course, BSM also has limits. People are not aware of many of the cognitive and motivational biases that may affect their decision-making and public welfare. One example for how difficult the recognition of a bias can be is the hindsight bias: people are not often aware of the bias, as it effectively changes their memory (Hoffrage et al., 2000). Indeed, studies have shown that the bias routinely afflicts judgments about a defendant's past conduct in cases of negligence, product liability, and medical malpractice (see Roese et al., 2012; Rachlinski, 1998). I propose BSM as a complementary instrument in the behavioral toolbox of regulators and contract drafters. While BSM can have significant advantages, it is not a general replacement for debiasing through law or nudging (see Jolls et al., 2006).

On the other hand, BSM should not be limited to loss and regret aversion. Present bias, lack of self-control and risk aversion are other obvious candidates of motivational biases that decision makers could deliberately overcome by delegating choices or imposing self-commitments. Buying insurance is one example of a self-debiasing strategy overcoming risk aversion. Self-imposed deadlines exemplify a strategy of self-nudging that aims to control present-bias. In Germany, people can effectively bar themselves from all public lotteries and casinos. As they cannot revoke this self-commitment, it serves addicted players as a self-insulating strategy. The concept of BSM may also extend into other domains of decision-making with less obvious policy implications. Fairness preferences could be such an extension. Fischbacher et al. (2011) show that subjects in a dictator game, instead of directly allocating a low outcome to the dummy player, prefer to delegate the decision to an agent. Fischbacher et al.`s design motivates principals to delegate as a means to avoid punishment for selecting the unfair distribution. However, principals may also delegate, aiming to receive the better side of the unfair distribution at lower costs of their own inequality aversion. By delegation principals would suppress their social preferences, instead of a bias, to reach better outcomes. Similarly, in an ultimatum game with multiple receivers and one proposer, proposers might reduce their costs of inequality aversion by focusing on making equal, although unfair, offers to all receivers. Receivers might try to focus on the other receivers' equal outcomes, rather than on the unfair allocation between themselves and the proposer in order to accept the offer at lower inequality aversion costs. The latter examples not only demonstrate BSM's potential range, but its status as a private strategy of decision-making, one that need not always be in line with the public interest. Suppose for example an employer who pays less than minimum wage equally to all of his workers.

XI. A Model Case for Empirical Legal Research

In all the studies presented here I have identified and analyzed mechanisms that reduce or even eliminate loss aversion. The results demonstrate the ecological and adaptive nature of decision-making. The magnitude of debiasing and bias-insulating the results reveal suggest that loss aversion and the Endowment Effect are unlikely to threaten most transactions.

Equally important are the studies` methodological implications. It appears that the legal literature was well-justified in relying upon the overwhelming evidence for loss aversion and reference dependence collected by Economics and Psychology. As such, to prevent a supposedly prevalent bias from hampering private exchange, scholars have primarily focused their work on suggesting, testing and criticizing possible legal interventions (see for example Kelman, 2003, and Langevoort, 1998).

However, findings in judgment and decision-making will often not be ready-mades from which the law would be directly able to derive policy recommendations. Behavioral Law and Economics and empirical legal studies have rightly criticized many legal scholars for following an armchair approach regarding empirical evidence; but there is also a temptation to (mis-)use empirical evidence in an armchair fashion. Such a style of research imports behavioral findings and treats them as ready-mades to be used in models and arguments presuming that people are consistently and in and of themselves loss-averse, present-biased, risk-averse, ambiguity-adverse, and so on. But these are hardly fixed attitudes.

Decision-making is ecological and adaptive. The studies I presented here show, for example how strongly loss aversion varies with the task that people perform: while the valuation task implemented and tested by most Psychology and Economics studies is robustly biased towards the status quo, loss-aversion barely impacted the trading task in which sellers aimed to sell their property for profit. The same held for the institutional and social context in which people operate and make their decisions. Institutions allowing owners to share their individual responsibility for trading largely dissolved loss aversion and so did social concerns and negative externalities which appeared to redirect the subjects` attention.

BSM demonstrates the adaptability of decision making most saliently, as people use the market's social and institutional context to purposefully debias or insulate their bias. Even more so, they take advantage of their own biases to nudge themselves towards better outcomes. All this evidence shows that it is misleading to think of biases as stable entities.

The law's policy interest differs distinctively from Psychology and Economics' dominant interest in theory-building and -testing. My first study on strategic markets demonstrates this difference. Most economic and psychological work focuses subjects` attention on the values and attributes of their entitlements. This experimental design is ideal for proving that valuation depends on reference points so as to support Prospect theory, because it is parsimonious (prevents interactions and confounds) and triggers a strong bias. However, as it elevates the level of bias, it can mislead legal policy – and as I have shown has done so in the past. To determine policy implications and conclusions a realistic strategic market design is vital.

The evidence on debiasing strategies also demonstrates the difference between the law`s policy interest and the interest of other disciplines. The business literature for example tries to discover and test optimal debiasing strategies, aiming to advise people as to how to improve their decision-making. What decision strategies people actually use is not relevant for this purpose. By contrast, my concept of BSM is tailored to the law`s policy focus: my goal with BSM is to analyze the strategies that people actually employ to debias, insulate their biases or use for nudging themselves to reach better outcomes. The evidence across the many studies I conduced demonstrates that people are capable of productively managing their own biases, and that BSM can therefore be an effective complement to regulation.

The same holds for analyzing the reinforcing interplay of separate debiasing mechanisms. Since it may depend on the interplay whether or not the remaining magnitude of a bias is policy relevant, this analysis holds interest for the law. By contrast the Psychology and Business literature shows little interest in producing this evidence, because of a strong publication bias: proposing a new debiasing strategy is much more promising for publication.

I conclude that the analysis of loss aversion and the Endowment Effect can serve as an informative role model for the specificity of empirical legal research. Empirical evidence is most important for designing effective legal regulation, but it can also lead the law astray.

Chapter 2 - Debiasing through Markets

Abstract

Most Endowment Effect studies incentivize individuals to reveal their true valuation when selling their entitlement. This experimental design can mislead the legal debate because it focuses sellers` attention on their endowment and thereby elevates the size of their bias. In my study, I place subjects in a market with incentives for strategic trading behavior. As in real markets, participants can increase their profits by demanding sales prices beyond their true valuations. I assume that trading in a strategic market environment debiases sellers because it shifts their attention from their entitlements to the gains from trade and to estimating the WTP of potential buyers. As predicted the trading task that allowed subjects to make profit reduced the Endowment Effect drastically, to a highly insignificant one-fifth of its original size when I endowed subjects with lottery tickets (cohen`s *d* -0.14). I support the debiasing effect with direct evidence, showing that both the impact of loss aversion and regret on the trade vanish under these conditions. To test both effect channels that can drive the endowment effect, anticipated regret as well as attachment and sentiment, I conducted a separate study with university mugs. In this second study the endowment effect was eliminated completely.

Finally, I show that involving more than one market institution into trading can reinforce the debiasing effect. To test this assumption, I provide the subjects with agents who carry out the trades following each subject's instructions. With the two institutions, strategic markets and agency, debiasing the sellers, the gap between WTA and WTP prices disappeared completely - also for lottery tickets.

My findings should apply to the large majority of market transactions. Most businesses and private sales trade for profit. I conclude that loss aversion and the Endowment Effect may have far less impact on private ordering than typically suggested in the legal literature.

I. Introduction

Hundreds of law review articles have argued that the Endowment Effect (henceforth EE) requires the law to intervene in private ordering. In free markets, people should ideally exchange goods until they are owned by those who value them the most, absent transaction costs (Coase, 1960). But transaction costs are not the only impediment to beneficial exchange. When their decisions are biased, the owners themselves can constitute an impediment. The EE's size alone suggests that it may pose a severe behavioral threat to the efficiency of trade, as prices demanded in the state of ownership can exceed buyers` willingness to pay by a factor of 3 or 4. This bias is robust and replicates reliably using the standard experimental protocols. The implications for private ordering are far-reaching. Unlike in a Coasian world, the EE suggests that initial allocation matters. Owners keep their entitlements, even when other market participants would value the property considerably higher (see, for example, Thaler, 1980; Knetsch and Sinden, 1984; Knetsch, 1989; Tversky and Kahneman, 1991; Kahneman, Knetsch and Thaler, 1991). Voluntary transactions cannot realize or even discover these gains from trade, since they exist only when both parties are in the same state of possession. Thus, unless it can be overcome by owners themselves, the EE seems to necessitate external legal intervention.

However, in recent years, evidence about the effects of the EE, as well as policy recommendations based on that evidence, have come to be viewed more skeptically. First, Plott and Zeiler criticized the results on methodological grounds. They argue that the gap between WTA and WTP prices is often an artefact of the experimental methods used. For example, Plott and Zeiler argue that goods had often not been allocated randomly in studies on EE, consequently showing that subjects value a good more when it has been selected for them. However, while their analysis improved the experimental protocol of EE studies, the bias has since been reproduced in attending to their critique - for example, by Isoni et al (2011), Heffitz and List (2013), Arlen and Tontrup (2015a, b), and others.

Arlen and Tontrup (2015a), by contrast, do not question that ownership can bias valuation. They argue that the legal literature on EE tends to derive policy recommendations from experiments that do not account for the institutions that people typically use or act within when trading. As they may find out after the sale that keeping their entitlement would have been the better decision, sellers anticipate experiencing regret. The expected costs of regret can prevent them from trading. In business contexts, however, owners seldom trade alone. They employ institutions like an agent, or a board of peers votes to reach the decision to trade. These institutions allow decision-makers to share responsibility, muting the regret they expect to experience over a wrong decision. Entitlement holders even purposely involve costly institutions in order to self-debias and trade at lower regret costs.[44]

In this study, I analyze the debiasing capacity of strategic markets. Glöckner et al. (2015) presented first evidence that the EE decreases when

[44] Another strategy of bias-self-management does not remove the EE. Instead, people shift their bias such that it does not prevent them from making their preferred choice (see Arlen and Tontrup, 2015 b).

owners in an Anticommons dilemma raise sales prices to exploit buyers and other sellers. Here, I depart from the narrow context of the social dilemma, which mingles strategic incentives with sellers` social concerns.[45] My goal is to generalize the debiasing effect of strategic markets, showing that they debias the majority of market transactions, as the task of trading debiases traders in and of itself. The sale incentivizes sellers to focus their attention on the gains from trade and on the WTP of their transaction partners, as opposed to the properties of their entitlements. Focusing on the profits from trading and strategies to earn this profit eases sellers` loss aversion. I define trading as strategic when sellers try to max out the WTP of potential buyers, pushing the prices beyond their actual costs. Strategic pricing is characteristic for many, if not most, business transactions, as most markets permit traders to increase their profit by setting prices strategically. Competition is almost never perfect. The reasons for this include the frequent lack of full transparency as to a good's quality and price, the unavailability of nearby substitutes for certain goods, and the possibility of market failures like hold-ups occurring. Both owners and buyers can exploit strategic market situations and exaggerate or downplay their true valuations. This makes the trade a strategic interaction with the same goal on both sides: to increase the own share of the gains from trade. I test my theory by placing the experimental subjects in an actual market with incentives for strategic trading behavior. My results suggest a strong reduction of the EE for a realistic market scenario.

EE experiments in the fields of Psychology and Economics are typically designed to exclude strategic behavior, on the assumption that it will confound experimental results. Where strategic effects are not excluded, their impact on the EE is not systematically measured, which would require both eliciting subjects` true valuations in a standard BDM design *and* placing these subjects in a strategic market to compare their EE, all else being equal. The two experimental paradigms most often used in Economics and Psychology are a random price mechanism and a random allocation design, both of which are detailed in the subsequent section of the paper concerning theory. Both paradigms incentivize subjects to focus their attention on the valuations of their entitlements. The experiments are designed to analyze the cognitive process underpinning the reference dependence of preferences. To cleanly demonstrate that valuation is state-dependent and influenced by loss aversion, these paradigms strip down the decision-making context to the point that ownership alone can affect the subjects` valuations of the goods. Besides valuing and pricing their entitlements, the participants have no other cognitive task. Accordingly, the only

[45] The Anticommons dilemma also refers to a different strategic incentive than I do: the incentive to outsmart the other anticommons owners` by trying to increase ones` own share of the overall sales price, relative to theirs.

pieces of information they need to process are the positive and negative attributes of their entitlements.

Legal analyses of EE, by contrast, ultimately aim to determine policy applications. They are concerned with the effectiveness of private exchange, which brings strategic behavior into focus. Namely, when people trade in real markets, they often set prices strategically to increase their profit. The personal valuation elicited by most EE studies often only amounts to the sellers` well-hidden reservation price. Determining one's own true valuation constitutes just one element of the more cognitively complex task of trading. For example, sellers will study market prices to form beliefs about the WTP of potential buyers, as well as considering the quality of the goods other sellers may offer. Often, gains from trade reach beyond concrete transactions, since the traders must take the potential for future deals with the same partners into account. Moreover, fulfilling profit expectations may be crucial to a personal promotion or his own business considerations. I assume that the complexity of this trading task shifts the sellers` attention from the valuable properties of their entitlements to the social and economic context of the transactions. This change of attentional focus reduces loss aversion and the bias in their valuation of the property. The results of this study show that strategic markets form a powerful debiasing institution. Therefore, I conclude that the EE may affect private ordering much less than typically reported in the legal literature.

I base my experimental predictions on cognitive theories of attention. The original explanation for the EE is Prospect Theory`s account of loss aversion (Kahneman and Tversky, 1979). According to Prospect Theory, the owner experiences selling a good as a loss, while the buyer perceives adding the same good to his endowment as a gain. The loss looms larger than the gain biasing, preferences, and sales prices. Therefore, one values the same good more in the role of a seller than as a buyer, indicated by the large gap yawning between WTA and WTP prices (Thaler, 1980). Prospect Theory, however, black-boxes the cognitive process that causes losses and gains to be experienced differently. As a result, it cannot explain why social factors tend to strongly affect the size of the EE without changing endowment status. For instance, personal attachment (Strahilevitz and Lowenstein,1998) and self-association with the good up for trade (Maddux et al., 2010) elevate an owner`s EE. Given the large impact they have on the EE, both attachment and self-association must affect the cognitive process that causes the bias. The same holds true for the strategic markets I analyze here, as I expect them to reduce the EE without altering the owners` endowment status. To analyze the influence on the EE produced by profit-oriented behavior in markets, a better understanding of the specific cognitive process driving loss aversion is needed.

Recent work has tried to provide this understanding[46]. Carmon and Ariely (2000), for example, propose that the EE is caused by a cognitive focus on the foregone: sellers focus on the good they would give up, while buyers focus on the money they would have to spend on the trade. Ashby et al. (2012) use eye-tracking to show that loss aversion focuses sellers on the positive, value-increasing aspects of their entitlement. Negative aspects, by contrast, receive little attention. Buyers, in turn, focus on the opportunity costs they would have to bear if they obtain the good. Johnson et al. (2012) show that endowment status also biases the retrieval of attributes from memory. Consequently, when sellers and buyers weigh the positive and negative aspects of a transaction, their opposing endowment status leads to the characteristic gap between WTA and WTP prices.

Attention-based theories (Bhatia, S. and Golman, R. 2019; Yechiam E., & Hochman G. 2013). predict that social factors can increase or reduce the EE by affecting the seller`s attentional focus. Personal memories, for example, increase the EE because they focus owners on their entitlement. Strategic market incentives do the opposite, drawing the sellers` focus from their entitlement to the profit they may earn upon accomplishing a lucrative deal. Based on cognitive theories of attention, I therefore expect strategic markets to effectively debias owners, loosening their loss aversion-driven fixation on their entitlement and switching their attentional focus to gains from trade.

A central difference to the literature is that this theory is not based on experience and learning. A longer debate in the literature discusses, whether real markets may teach participants to trade effectively. Professional traders may then not be subject to the EE. John List (2003) conducted an exchange experiments in the field. On real sports card markets he invited professional traders and inexperienced lay participants and endowed both groups with sports cards. Then he offered both groups of subjects to exchange their sports cards against a second sports card-card in his possession. His study demonstrated that only the most experienced traders did not reveal an exchange asymmetry. List suggests that experienced subjects may have been more certain of their preferences (or the goods' values) and therefore traded more often than the lay subjects.

Earlier studies have tested the impact of experience in laboratory settings. They analyzed the size of the price-gap in repeated auctions (for example Shogren et al 2001 and Plott 1996, while Cox et al 2003 and Loomes et al. 2003 find evidence for a shrinking yet persistent gap). In the repeated rounds participants appeared to better adjust their pricing decisions to the offers they were able to observe in the market. The price gaps narrowed down

[46] See also the overviews by Morewedge, et al., 2015 and Brown, 2005.

after as many as repetitions. They drew the conclusions that anomalies result from errors, and became less frequent as market experience accumulated. With incentives and practice, it seems subjects arrive at 'considered choices' that reflect stable underlying preferences (Plott 1996).

However, whether this shrinking of the pice gap indeed is evidence for a process of learning from experience leading to more rational pricing is questioned: Knetsch et al. (2003) suggest the behavior might also reflect a conformity bias with subjects simply mimicking the other market side.

Engelmann and Hollard (2010) are also skeptical about market experience diminishing the trade asymmetries. They propose trade uncertainty as the source of subjects` hesitance to trade. They show that subjects might quickly learn to overcome this uncertainty when forced to trade in their artificial experimental setting, yet they do not assume that real markets are a good teacher: they suppose that subjects avoid deals, they are uncomfortable with and therefore typically do not learn to overcome trade uncertainty in real markets.

Note that the debiasing effect I propose here does not depend on the experience of the traders, how often they have repeated a task or how familiar they are with the traded goods or their own preferences. The shift of attentional focus is an automatic cognitive process, it is not a learning process, it does not require time or repetition in contrast to the theories and evidence suggested by Plott, Shogren and List. Indeed, my experiment presents subjects with a one-shot task, they have never seen before. The attentional focus is shifted by the differently structured task subjects have to solve when they trade rather than evaluate an object. My theory suggests that the market does not teach subjects to change their trading behavior, but it shifts their attention and with it their valuation by the task it presents traders with.

To test my theory, I endow subjects with tickets of a real public lottery, the Eurojackpot.[47] I inform the participants about the probabilities that the ticket might win a prize, allowing them to determine the ticket`s expected payoff. The 2x4 design varies the endowment status of the subjects, comparing a pair of *Endowment* and *No-Endowment* conditions for each of the treatments. In the *Baseline* treatment, I use a BDM mechanism, which incentivizes participants to reveal their true valuation for the entitlement. In the second treatment, I modify the BDM mechanism, allowing subjects to benefit from strategic trading behavior. In the third treatment, subjects trade with a real transaction

[47] The official website of the lottery: https://www.lottoland.com/en/eurojackpot/results-winning-numbers

partner, who may also act strategically. For the fourth and final treatment, participants act through an agent in a strategic market.

For the *Endowment* conditions, I present subjects with a price list. This list asks, for each price, whether the subject would prefer to sell or keep their lottery ticket. I compare the subjects` WTA with the randomly drawn price or when they are matched with a real interaction partner, I compare their WTA with the price their assigned partner has offered. If their WTA does not exceed either price, the subjects sell their ticket and receive either the random price or their partner`s offer in exchange. Otherwise, they keep their ticket. For the *No-Endowment* conditions, I use a choosing design (Johnson et al. 2007). Subjects have to choose, for each price in the list, whether they prefer receiving the ticket or the money. If they have chosen in the price list, that for the random price or the price offered by their partner they prefer the ticket, then they receive the ticket. Otherwise, they are paid the money. As expected, a strong EE is replicated in the *Baseline* treatment. Subjects demand an average €5.79 for selling their lottery ticket, while their choosing price under the *No-Endowment* condition is only €3.76.

For the *Strategy* treatment, I modify the random price mechanism: participants are not paid the random BDM price, but the amount they demand. As is typical in the case of real markets, they can benefit from setting a strategic price.[48] The higher the amount they ask for, the more money they can earn. In turn, however, strategic pricing creates the risk that the deal fails at a price they would have preferred to have sold for. The results support an attention-based theory, demonstrating that the strategic market decreases the EE sharply. Under the *Endowment* condition, subjects demand a mean WTA of €5.71, while under the *No-Endowment* condition I find a mean choosing price of €5.11. The comparison yields only a small and insignificant bias (Mann-Whitney p-value 0.33; cohen`s d -0.24).

In the *Interaction* treatment, I move my design closer to the conditions of a real market. I replace the random price mechanism with a transaction partner who can likewise act strategically, such that subjects can try to outsmart each other. Under the *Endowment* condition, subjects sell their tickets for the prices offered by their partners, should their WTAs not exceed their offers; otherwise, they keep their tickets. Under the *No-Endowment* condition, subjects receive either the ticket or the money, depending on whether they had chosen the ticket or the money in exchange for the price their partner accepts for selling the ticket. The interaction with a partner complicates the experimental trading task. When they form beliefs about their partners' WTPs for the tickets,

[48] Assume subjects demand €8 for the ticket while their true WTA is €5. In this case they are paid €8 if the BDM price is not smaller than the €8 they asked for; see with many examples the design section.

subjects must consider that their partners may strategically lowball their offers. I assume that these interactions draw their attention to strategic considerations. And indeed, the WTA/WTP gap (*Endowment* condition: €4.04; *No-Endowment:* €4.14) shrinks further, to less than one-fifth of the magnitude measured in the *Baseline* treatment. The remaining effect size is marginal (Mann Whitney *p*-value 0.45; cohen`s *d* -014).

To support the debiasing effect with additional direct evidence, I employ two psychometric tests: a regret and an incentivized loss aversion measure. For the *Baseline* treatment the measures confirm that the subjects are biased. Owners experience significantly more anticipated regret over selling their tickets than participants in the *No Endowment* condition who have chosen money over the ticket. The loss aversion measure reveals this bias as well: the subjects the Fehr and Götte measure (2007) identifies as loss averse have a significantly stronger EE than the subjects categorized as rational. In line with the pricing decisions, the strategic market drives out the biases in both regret and loss aversion data. The difference between the regret subjects anticipate over selling their ticket in the *Endowment* condition, in comparison to under the *No-Endowment* condition, emerges as insignificant in the *Strategy* treatment and entirely disappears in the *Interaction* treatment.[49] The same holds for the loss aversion data: neither in the *Strategy* nor in the *Interaction* treatment do loss averse (or rational) subjects reveal an EE. In sum, I conclude that strategic markets are a powerful debiasing institution.

In a further treatment, I broaden the scope of my analysis to include other market institutions. To estimate the EE's true impact on private ordering, policy makers should not only identify all relevant debiasing sources involved in trading. They also need to analyze these sources' cumulative impact on the level of bias. When owners trade, they will often use more than one market institution capable of debiasing them. To test their cumulative impact on the EE, I present subjects with two such institutions: strategic markets and agency. I assume that these two institutions interfere with the causal process that drives the EE on different cognitive levels, which I expect to reinforce their debiasing effects. Strategic markets reduce regret over selling by lessening the owners' fixations on their entitlements. Agents, by contrast, decrease regret over selling by allowing the principal to share the responsibility for a transaction (Arlen and Tontrup, 2015 a). Tested alone, neither institution eliminated the EE completely in the descriptive results – even though, of course, the bias in the strategic markets did no longer reached significant size. In the *Agency&Markets* treatment I assign an agent to each subject. The subject can instruct the agent as to what price to demand. The agent is incentivized to follow this instruction - he is paid €2 by the experimenter if he does, and nothing

[49] To support my theory, I estimate a linear model that includes all data (see the result section).

otherwise – but it does not bind him, and the final trading decision is his to make. As implemented in the *Strategy* treatment, subjects sell their tickets and are paid the price the agent demands, provided that the amount of this demand is lower than that of the random price. As assumed, the two institutions seem to reinforce the debiasing effect, eliminating the WTA/WTP price gap completely (-€0.15; Mann-Whitney *p*-value=0.81; cohen`s *d* -0.06).

In my final experiment I show that the results also hold for the second effect channel that the literature has identified to drive the EE: attachment and sentiment. I therefore repeat the *Interaction* and *True Valuation* treatments, using the classic University mugs instead of lottery tickets. As expected, the results show a strong EE in the *True Valuation* treatment with subjects asking for a WTA of €5.32 in the *Endowment* condition versus a WTP price of €2.66 in the *No-Endowment* condition. Thus, endowment status doubled the price of the University mug providing significant evidence for the EE (two-sided Mann-Whitney *p*-value <0.01). The *Interaction* treatment should reduce the bias by focusing the subjects on gains from trade and the potential WTP of the second player. Indeed, confirming my hypothesis trading in the strategic market completely eliminated the EE. The results reveal a WTA price of €4.08 in the *Endowment* condition and €4.10 in the *No-Endowment* condition (two-sided Mann-Whitney *p*-value 0.98).

This study holds important implications for both legal policy and legal research on the EE. The law is concerned with private exchange and gains from trade in markets whose pricing is typically strategic, and in which owners must divide their attention between multiple economic and social cues, as well as the attributes of their entitlement. To decide whether a legal intervention is reasonable, it is crucial to determine the actual size of the EE in a typical business environment, where people trade for profit. Most economic and psychological studies, however, do not address this question. Instead, they focus their subjects` attention on the values and attributes of their entitlements, thereby elevating the subjects' biases. Because it is parsimonious and triggers a strong bias, this experimental design is ideal for isolating and analyzing phenomena produced by state-dependent preferences. By contrast, to determine policy implications and conclusions for legal research, a realistic strategic market design is vital. As such, the strategic market reduced the EE to less than one-fifth of the size I measured in the *Baseline* treatment. The results also demonstrate that policymakers should be advised to consider all relevant debiasing institutions before deciding upon an intervention. The cumulative impact of the two market institutions considered led to the complete elimination of the EE. This leads me the ultimate conclusion that, analyzed in an institutional market context, the EE poses far less of a threat for private ordering than suggested both by most legal literature and prior experimental evidence in Psychology and Economics.

This article proceeds in the following order. Section II presents the attentional focus theory the experiment is based upon and the related literature. In section III I detail the experimental design, and section IV reports my results. Closing the article, Section V discusses the validity of the findings and their implications for legal research and policy

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II. Theory and Related Literature

I assume that the EE is caused by a reference-dependent focus of attention, which leads to a biased weighting of entitlements' positive and negative attributes during the trading process (Carmon and Ariel, 2000; Nayakankuppam and Mishra, 2005; Johnson et al., 2007; Ashby et al., 2012). When owners decide whether or not to sell a good, they concentrate on the value-increasing aspects of their entitlements, giving them more weight in decision-making. Conversely, buyers focus on the transaction's opportunity costs: the money they could otherwise use for alternative causes. This process biases decision makers towards their status quo.

I assume that this mechanism is independently which effect channel is in fact driving the EE. Generally, the literature distinguishes between attachment, which pertains mostly to physical goods with qualities like beauty, elegance or usability. Because their endowment status focuses the owners' attention on these attributes of usability and beauty, they correspondingly develop a higher valuation of their goods and the literature speaks of that they have become "attached" to their entitlement. The second effect channel is anticipated regret about trading choice when the future value of the good is uncertain; it refers to entitlements like stocks and patents (Lemley and Shapiro, 2005). Neither the goods' properties nor their market prices need to be the cause of this uncertainty. The uncertainty can also result from the owner's vague preferences about the value of the good. In this case, when owners are not sure of their goods' values, they can feel regret over a sale, anticipating that they later realize they would have preferred keeping their entitlements instead.[50]

I assume that the mechanism behind these two effect channels is the same. It is the attentional focus on the entitlement, that enhances the weight that is given to the positive attributes of the entitlement. This leads in one case to a higher attachment to the good and in the other to an enhanced regret over

[50] See also Weaver and Fredrick (2012). They find that people reject selling for a price that falls short of the assumed market price, even when it exceeds their true WTA. Sellers anticipate that they will feel regret over selling for their true WTA, should an option later occur to sell at market price.

losing an entitlement that might turn out to be very valuable. The trading task redirect this focus from the entitlement to the gains from trade and the WTP of the buyer and thereby reduces the bias; in one case the attachment shrinks, because the subject`s focus is less on the positive attributes of the good and in the other case, anticipated regret is smaller, because the good`s value is viewed less favorably. For my main study I chose lottery tickets and thereby anticipated regret to be the dominant effect channel, as lottery tickets tend to produce very strong EE`s (Bar-Hillel et al., 1996; van Dijk and Zeelenberg, 2005). However, I analyzed the second effect channel of attachment and sentiment by conducting an otherwise identical separate study using the classic University mugs instead of lottery tickets. The results are the same; in fact the EE for the mugs disappears entirely, when subjects trade for profit rather than are asked to state their valuation for the cup. I report the complete mug experiment in the appendix.

In the following I will focus on the main experiment and therefore will base the predictions on regret theory (Kahneman and Tversky, 1982). My central assumption is that the biased attention increases the cognitive availability of the good`s positive attributes, at once increasing the values owners ascribe to their entitlements (Kahneman et al., 1986; Johnson, et al., 2007). The more valuable the goods' positive attributes appear to their owners, the more regret they will anticipate over selling their property (see Bell, 1982; Loomes and Sugden, 1982; Connolly and Butler, 2006; Zeelenberg and Pieters, 2004). For example, when patent holders consider selling their entitlements, they focus on the patent's economic potential, which increases their feelings of regret over selling. As ownership focuses the entitlement holders on their goods' benefits, gains from trade receive comparatively less attention, and accordingly less weight in the decision-making process (Bhatia, S. and Golman, R. 2019, Yechiam E., and Hochman G. 2013 and Ashby et al., 2012 direct eye tracking evidence). Therefore, patent holders anticipate more regret over selling than over keeping their entitlements. The asymmetry of regret costs biases them against trading (Loomes and Sugden, 1982; Sugden, 1985; Landman, 1987; Baron and Ritov, 1994; Connolly and Butler, 2006; Nicolle et al., 2011). As applied to my study, subjects should experience more regret over trading their lottery tickets than over keeping them. As will be shown in the section explaining my hypothesis, I expect subjects in seller roles to seek compensation for their regret costs by asking for higher sales prices.

I assume that, when the subjects trade, both the structure of the cognitive task and the information they must process influence the focus of their attention and the size of their biases. Business contexts will often contain strategic elements that the parties can exploit for increased profit. For example, owners can influence the speed and likelihood of closing a deal by asking for higher or lower prices. Strategic interactions with buyers lead sellers to form beliefs about what incentives and WTP those buyers might have. This moves sellers' attention to focus on the buyers, along with all their personal motivations for the trade, their resources, as well as their traits like risk attitudes. Business relationships are typically characterized by multiple layers of strategic incentives, which sellers must then balance in their decisions Thus, counter to standard EE experiments, which incentivize sellers to reveal their actual valuations for their goods, the actual trading process forces decision makers to consider a higher degree of information, beyond the attributes they value in their entitlements. I assume that this cognitive process loosens owners` attentional focus on their entitlements, reducing the biased values they ascribe to their property and the regret they anticipate over its sale.[51]

I test my theory by systematically increasing the strategic character of the market in which I place the subjects. The more demanding the strategic market context, the larger is the portion of the owners' attention` that may be absorbed and drawn from their entitlements. In the *Strategy* treatment, the incentive structure remains simple compared to a trading task in a real market, as the random price's probability distribution informs the subjects precisely about the gains and risks of strategic pricing. Subjects can earn more by taking higher risks. The second treatment moves the scenario closer to the conditions present in a real market. By matching subjects with trading partners who have the same strategic incentives, an element of strategic exchange is added. Subjects can earn more by second-guessing the WTP and strategic behaviors of their partners. My theory predicts the more salient the possible profits are that depend on their decisions, the more likely subjects are to switch from loss into gain frames.

The attention-based theory allows me to understand the EE as a continuous bias, one that institutions can reduce or eliminate without changing endowment status. In many psychological and economic EE studies, the bias is strong because the owners' focus fixates on their entitlements. When the trading task`s social and economic context catches and disperses owners' attention, their biases are weakened. Once the owners predominately focus on gains

[51] A strategic market may also switch the task's framing. If sellers trade to make a profit, I expect their attention to focus on the benefits they may gain from trading. The more pronounced this focus, the more likely, they may be in a gain frame, not a loss frame. For example, patent holders may not focus on the values their patents have for them, but on the profits, they can make by selling them. A switch in framing explains nicely why professional traders, who own goods only for their exchange value, do not display any EE (see Kahneman et al., 1990). Accordingly, if the participants` attention switches from their entitlements to the possible gains from trade, they might not view their lottery tickets as potential winners. Instead, they perceive them as likely blanks that would earn them more money when sold. In this scenario, anticipated regret over losing the ticket and with it the EE would diminish.

from trade, as typical for businesses that trade for profit, their bias completely vanishes.

III. Experimental Design

A. Real Lottery

I endowed the subjects with tickets of the lottery Eurojackpot. The tickets can be purchased online. The lottery is conducted every week and distributes millions of Euros amongst its winners. The draw is broadcast live on the internet, with the winning numbers also publicized on the lottery's website. Players can tip 5 and two extra numbers on their tickets. In advance of the experiment, I purchased online tickets for the targeted number of observations, informing the subjects that I used software to randomly select the numbers tipped on the tickets. I then drew the tickets from an urn and numbered them following the order of the draw. I sent the first ticket drawn to the first subject to start the experiment; the second participant received the ticket drawn second, and so forth. Before the subjects began completing the study, they were prompted to create anonymous e-mail accounts (see method section for details), to which I sent the lottery tickets. To confirm that they read the e-mail messages they received, they had to indicate the first three numbers tipped on their respective ticket. The instructions clarified that the participants would only be paid if they submitted the correct numbers. As some subjects may have participated in the Eurojackpot lottery before the experiment, either with success or with disappointment, I did not reveal the lottery's name before the subjects had fully completed the study.

Under all treatments I sent subjects receipts from Eurojackpot, which confirmed the numbers tipped on the tickets. Therefore, subjects were aware that they would learn after the fact whether their tickets had won in the lottery or not. Importantly, the receipts alone did not establish ownership. I informed the participants that Eurojackpot sends separate authentication codes to all lottery participants. Only those persons who rightfully have these codes own the tickets and are authorized to claim prizes.

B. Endowment Status

In the *Endowment* conditions of all treatments I send subjects the tickets and authentication codes which establish their ownership, entitling them to collect prizes should their tickets win. I instruct each participant that, should they sell their ticket in the course of the experiment, they give up ownership of the ticket and their right to claim a prize. In the *No-Endowment* condition by contrast, subjects are provided only with the ticket receipt and not with the authentication code. Thus, while the subjects also learn the tipped numbers, they have no ownership rights to them. I use a choosing design (Johnson et al., 2007), which prompts subjects to choose for each price listed between the lottery ticket and a payment. They receive whichever option they have chosen – the ticket or money - either at the randomly drawn price or at the price offered by their partner. In the treatments, in which they are given incentives for strategic trading behavior, subjects are paid the lowest amount for which they chose money over tickets.

C. Price Mechanisms

My experimental protocol allows me to measure how the effect size of the EE changes in a market scenario with incentives for strategic trading behavior. My *Baseline* treatment implements a standard random price mechanism (hereafter BDM, see Becker, et al. 1964) widely used in both economic and psychological experiments to replicate the EE. [52] The random price mechanism incentivizes subjects to reveal their actual valuations of their entitlements. I measure the effect of strategic markets on the magnitudes of their biases against the benchmark of the BDM.

In the *Strategy* treatment, I modify the price mechanism to provide subjects with incentives for strategic behavior. Instead of the random price, subjects are paid the amounts they demand. In the *Interaction* treatment they are paid the offers of their partners should they sell their entitlements. This allows the subjects to increase their profits by pushing prices beyond their true valuations. As long as their demands do not exceed the random price or their partner`s offer, they receive any price asked. However, by lifting their WTA, subjects risk deals failing, even though they were offered a price they would have preferred to sell for.

D. Treatments

1. Baseline: True Valuation Treatment

In the *Baseline* treatment, I elicit subjects' true valuations for the lottery tickets. The treatment does not provide strategic incentives. In the *Endowment* condition, the BDM mechanism compares the WTA subjects indicate with the random price drawn. Subjects can demand any price ranging from 25 Cents to €10 in exchange for their lottery ticket. The random prices are drawn within the same range. I present the subjects with a complete list of all 40 prices then asking them whether they want to sell or keep the ticket for each price separately (0.25; 0.50; 0.75 … 9.75; €10.00). The lowest price that they indicate they

[52] A second experimental design type used in many EE studies is a random allocation design (see for example Knetsch, Kahneman and Taylor, 1990). This design randomly assigns subjects either the role of an owner - who receives the good - or the role of a buyer who is given a monetary endowment. Assuming the valuation for the good is randomly distributed across owners and buyers, half of the owners should sell, while the other half should keep the good. The fewer people trade, the stronger is the evidence for the EE. The design avoids strategic incentives: As sellers and buyers trade for the market clearing price, they do not benefit from setting a strategic price. Sellers should act as price takers in this setting.

would prefer to sell for reveals their WTA. If their demand does not exceed the random price, they sell their tickets and are paid the random price in return. By contrast, if they ask for amounts beyond the random price, they can keep their tickets and are entitled to collect any prize the tickets may win in the lottery.

In the *No-Endowment* condition I employ a particular method of choosing prices (see Johnson et al., 2007). Subjects are presented with the same price list as in the *Endowment* condition; for each price, they must choose between the ticket and the money. The lowest price for which each subject would prefer to obtain the money reveals their WTP or choosing price.

The BDM mechanism incentivizes the subjects to reveal their true valuations for the tickets. Because they receive either the ticket or the random price, they have no influence on the size of the actual sale or the choosing price. An explanatory example can be found in the footnotes.[53] Thus, exaggerating the price holds no benefit for the subjects, provided their demands do not exceed the random prices. If the subject asks for an amount that turns out to exceed the random price, exaggerating the price even becomes disadvantageous. A comparison between the *Endowment* and *No-Endowment* conditions reveals the EE and serves as the study`s *Baseline*.

2. Strategy Treatment

I modify the BDM mechanism to provide subjects with incentives for strategic behavior. While the original mechanism allocates the random price to the subjects, even if they demand prices lower than the random price, participants obtain the prices they ask for in the modified version. In an example scenario, the subject demands €5, consistent with his actual valuation, as compensation for the sale of his ticket, and €8 is then drawn as the random price. The subject then sells the ticket and receives the €5 he asked for in exchange. If the subject demands €7, exaggerating his actual valuation, then the subject can increase his earnings by €2. Thus, my modified BDM mechanism allows the participants to benefit from pushing the price beyond their actual valuations. In turn, however, the design attaches a risk to this strategic behavior. For example, consider that the subject demands €9. Now he must keep the ticket, even though he would have preferred to sell it for the random price.

[53] For example, assume a participant demands €5 for selling his ticket, while a random price of €8 is drawn (note in all examples I assume a random price of €8). Since the random price is larger than the €5 the subject is willing to accept, the subject collects the random price in exchange for the ticket. Setting a price beyond his or her true valuation of the ticket can only make a subject worse off. For example, assume the subject demands €6 instead of his or her true WTA of €5. Then, the subject does not benefit, but would still obtain the same random price. However, if the subject pushes the price even higher and demands €9, then his or her WTA exceeds the random price and the subject has to keep the ticket, even though he or she would have preferred to trade for the €8 of the random price.

In the *No-Endowment* condition, subjects obtain either their choosing price or the ticket. For example, the subject states a price of €7, while the lowest price for which he actually prefers money over the ticket is €5. By overstating his valuation, the subject obtains €2 more than his actual valuation, providing the incentive for strategic behavior. However, just as in the *Endowment* condition, if the subject, in exaggerating, pushes his choosing price above what turns out to be the random price, then he is allocated the lottery ticket. The gap between *Endowment* and *No-Endowment* conditions allows me to estimate whether the strategic market has reduced the EE relative to the effect size measured in the *Baseline* treatment.

3. Interaction Treatment

In order to move the study design closer to a real market, in which both counterparts can act strategically, I assign subjects real partners for their transactions.[54]. The two roles - subjects and partners - are randomly assigned. As for each treatment, I implement a pair of *Endowment* and *No-Endowment* conditions as a means to compare the magnitude of the WTA/WTP gap with the other treatments.

In the *Endowment* condition, a subject sells his ticket, should the lowest price he is willing to sell for not exceed the amount his partner offers. The subject obtains this price in exchange for the ticket, and the experimenter collects any difference between the offer and the subject`s WTA. If he asks for a price higher than his partner's offer, the deal fails, and the participant keeps his ticket.

In the *No-Endowment* condition, subjects state for each price whether they would prefer to obtain the money or a ticket. If subjects stated they would prefer the ticket over the price offered by their partner, the subjects receive a ticket. In turn, if the choosing price the subjects state (the price at which they prefer to receive the money over the ticket), does not exceed the price offered by their partner, then the subjects receive the choosing price they stated.[55] The higher the subjects set their choosing prices, the larger are the profits they earn giving the subjects an incentive for strategic behavior. However, if a subject strategically pushes the price higher than what his partner turns out to offer, he is then allocated a ticket, even though he would have preferred to have obtained the monetary price.

[54] The treatment increases the cognitive complexity of subjects` decision-making task. In the *Strategy* treatment subjects can calculate risk and benefits of setting a strategic price, as they know the distribution of prices in the BDM mechanism. In the *Interaction* treatment, by contrast, the subjects have to anticipate the strategic behavior of their counterpart if they want to trade successfully.

[55] Assume a subject indicates his true choosing price of €5. If the partner offers €8 for the ticket, then the subject obtains his choosing price of €5. However, if the subject sets a strategic price of €7, then he would obtain €7 instead of €5, because his partner`s WTP of €8 is still higher. Yet, if the strategic price is €9, then the deal fails.

The experimental design does not change the partner`s role in either the *Endowment* or *No-Endowment* conditions. In both, the partner makes an offer to buy the ticket. This design keeps the manipulation consistent across the two endowment state conditions. By being matched with real counterparts, the subjects are forced to anticipate their partners' (potentially strategic) behavior. Would their individual counterparts be owners in one condition and buyers in the other, subjects may expect their counterparts to offer widely different prices based on their endowment status. For instance, subjects may anticipate EEs in their partners they may consider in their own pricing decisions (see van Boven et al., 2003). By giving the partner a €10 endowment in both *Endowment* and *No-Endowment* conditions and asking him to indicate his WTP for the ticket, I avoid this potential confound. Just like the subjects, the partner has incentives encouraging strategic behavior. Namely, he can offer any price within his budget to buy the ticket and keep the amount that remains unspent. The choosing design allows for an intuitive implementation of the transaction in the *No-Endowment* condition. If the partner states that he values the ticket at least as highly as the subject, the partner gets the ticket and the subject receives his choosing price. Otherwise, if the partner offers a price lower than the subject`s choosing price, then the subject receives the ticket and the partner keeps all his endowment. The instructions informed the subjects of their partners` trading incentives.

4. Agency&Markets Treatment

In order to analyze whether multiple institutions can reinforce each other's debiasing effects, I assign the subjects an agent and place them in the otherwise unchanged *Strategy* treatment. Agency is an institution that people often use for trading and that has been shown to decrease their EE (Arlen and Tontrup, 2015a). By sharing the responsibility for the trade with an agent, owners can reduce regret over the sale of their entitlements. In the *Strategy* treatment, I randomly match each subject with a second participant who acts as their agent. The agent sets the price; his decision is final, leaving the subject with no veto. However, subjects instruct the agent by selecting their preferred sale or choosing price. The agent is paid €2 from the experimenter if he complies with the instruction and nothing if he does not comply, incentivizing the agent to follow the subject's instructions. The agent has no monetary interest in the transaction itself and receives no other compensation for the study.

In the *Endowment* condition, subjects sell their tickets if their agents demand prices equal to or lower than the random price. In the *No-Endowment* condition, a subject is paid the money if the agent has chosen money over the ticket for the random price; otherwise the subject receives a ticket. Since the subjects receive the prices their agents demand and not the random price, both conditions *Endowment* and *No-Endowment* provide incentives for the subjects to let their agents set strategic prices.

In addition, I implement a control group, in which subject and agent are placed in the *Baseline* treatment without incentives for strategic trading behavior. The treatment allows us to isolate the debiasing effect of the agent alone and compare it to the cumulative effect of agent and strategic markets. In the control treatment, the subject receives the random price, neither the price the agent demands nor the ticket.

E. Second Market

My theory suggests that strategic markets may loosen the sellers' focus on their entitlements' positive attributes, instead drawing their attention to gains from trade and estimating the buyers' potential WTP. I assume that this redirection of focus reduces their regret over trading and their loss aversion. To provide further support for this theory, I seek to identify the subjects who respond to the strategic market's incentives, pushing the prices beyond their true valuations. I expect these subjects to reveal smaller biases. In order to identify the particular subjects acting strategically, a second, hypothetical market, is implemented, which presents the *Strategy* and *Interaction* treatments' subjects with the *True Valuation Baseline* treatment. In turn, subjects who first completed the *Baseline* treatment are presented with the *Strategy* treatment in the second market.

Whenever the price a subject sets in the strategic market exceeds the price he sets in the true valuation market, the subject has made a strategic decision. As such, comparing the prices set in the two markets reveals evidence of strategic trading. This identification method allows us to compare the EE of subjects who by acting strategically revealed that they devoted attention to the market incentives with those subjects who did not.

Since all subjects were already familiar with the price mechanism, they only needed to be informed about how the second market's rules differ from those of the treatment they had completed in the first market. To illustrate the meaning of the changes, I provided them with scenarios and control questions, which either tested why they could benefit from asking for more than their true WTA in a strategic market or why they should reveal their actual preferences in a market presenting them with the *True Valuation* treatment.

IV. Additional Measures

A. Regret

My experimental predictions for the main study refer to regret theory (but see the study with university mugs in the appendix). I assume that the strategic markets reduce the subjects` biased focus on their entitlements, thereby reducing the degree of regret they anticipate experiencing over a potential loss. To support my theory with direct evidence, I elicit the levels of regret the subjects expect to experience, over a negative outcome following both a decision to trade and a decision to keep their tickets. The difference between the two I refer to as differential regret.[56] A comparison between differential regret costs in the *Endowment* and the *No-Endowment* conditions reveals the EE. For example, in the *Baseline* treatment, I expect differential regret costs to be higher in the *Endowment* than in the *No-Endowment* condition. For measuring regret I use Likert items with 10 ordered response levels, ranging from 1=no regret at all to 10=very strong regret.

B. Loss Aversion

To provide direct evidence that strategic markets reduce loss aversions' impact on trading behavior, I use an incentivized measure (Götte et al. 2004) that gives subjects the opportunity to participate in two lotteries. The first lottery offers subjects a 0.5 chance of either winning €8 or losing €5. The second lottery offers the same payoffs but is repeated six times. Subjects are paid the average outcome of all six draws, considerably lowering the chance that participation leads to an overall loss. An unbiased participant should accept both lotteries, because the expected outcome of both is positive. I classify the choices participants make into two categories: (1) the "loss averse" type, who rejects one or both lotteries; and (2) the "rational" type, who participates in both lotteries. In the *True Valuation* treatment, I expect loss averse subjects to ask for higher sales prices than rational types. By contrast, the strategic markets should reduce the loss averse subjects' fixations on their tickets, with the difference between the types` EEs expected to diminish correspondingly.

V. Methods

Participants are current and former students of the University of Münster. The subjects were recruited through the University`s main mail server. The server reaches the enrolled students of all departments (over 42,000 in total) and many graduates of the university. About 30% of the participants in this study have graduated and work as professionals. Subjects signed up for the experiment by clicking on a link in their invitation. The invitation contained an individual key, which they had to enter to begin with the study. Once used, the key

[56] In the *Endowment* conditions, I query participants to indicate how much regret they anticipate over selling their lottery tickets, considering that they might lose the prize their ticket may ultimately win. n. And secondly, I assess how much regret the participants anticipate over keeping their tickets, considering that they might lose the tickets' sales price without the tickets ever winning a prize. In the *No-Endowment* conditions, I query the subjects both as to how much regret they anticipate over collecting the money, considering that they might lose a prize the ticket might win, and as to how much regret they anticipate over receiving the tickets, considering that they may lose the sales price without the ticket ever winning a prize.

became invalid. To keep the dropout rate as low as possible, the subjects were only paid if they finished the experiment; only 8% of the subjects dropped out.

I elicited 100 observations for each treatment that is 50 subjects for the *Endowment* condition and 50 subjects of the *No-Endowment* condition of each treatment. I closed the sessions once the planned number of 100 observations in each treatment was reached. Subjects who had started but could not finish completing the experiment received a show up fee. Thus, a total of 100 subjects participated in each of the treatments in the *True Valuation*, the *Strategy*, the *Interaction* and the *Agency&Markets* treatment. Participation was anonymous. I asked the subjects to create an anonymous and cost-free e-mail account; to open the account, the subjects did not have to provide any personal information.[57] The account was used to send the subjects their lottery tickets and authentication codes. The payment was also anonymous. I instructed subjects to make up a 5-digit number. After the experiment, they could pick up their earnings in the student government offices, after indicating their personal code.[58] The subjects have different academic backgrounds, coming from both the social and natural sciences and an equal number of men and women was invited to participate in the experiment. Demographic variables like gender and age did not significantly affect the regression results.

To ensure that the subjects fully understood the operation of the price mechanism, they had to complete a set of control questions, reported in the appendix to this study. The participants could only proceed with the experiment if they answered all questions correctly. The questions presented the subjects with various scenarios, for each of which they were asked to calculate the payoffs.[59] Participants did not demonstrate any difficulty understanding the instructions and the price mechanism.

VI. Hypothesis

A. Comparing WTA and WTP prices

I expect ownership to focus the subjects` attention on the positive attributes of their lottery tickets (Ashby et al., 2012). The biased attention should lead subjects to give more weight to the potential prize the lottery ticket may win than to the money they would earn from its sale. In the *No-Endowment* choosing condition, subjects` focus should neither be biased towards the ticket nor the

[57] The provider of the anonymous e-mail account is: https://securemail.hidemyass.com/.
[58] Subjects could opt out of the anonymous payment and choose to be paid via bank transfer or PayPal.
[59] . The scenarios tested whether subjects understood both that they were best off when they revealed their actual WTA in the Baseline treatment and that they could increase their profit in the strategic market treatments at the risk of losing a beneficial deal.

money, because they own neither of the two. I therefore expect to observe a gap between WTA and WTP prices that reveals an EE.

Hypothesis$_{1a}$*: Ownership Causes WTA/ WTP Price Gap*

I theorize that strategic markets draw sellers` attention away from their entitlements and towards gains from trade and to estimating buyers' potential WTP. Loosening their fixation on their entitlements should reduce the biased weight given by sellers to their lottery tickets. The profit subjects can earn might even switch the framing of the trade. Instead of experiencing the sale of the tickets as the loss of an entitlement, the subjects may be pushed into a gain frame by the potential profits they could earn asking for prices beyond their actual valuations of the tickets. I predict that the strategic market has a significant debiasing effect compared with the *True Valuation Baseline*.

Hypothesis$_{2a}$: *Strategy Treatment Reduces EE*

This debiasing effect should also appear in the second strategic market treatment, which presents subjects with real trading partners. The counterparts increase the complexity of the subjects` cognitive task, because they must keep in mind that their partner has the same incentives to act strategically. To earn a profit, subjects have to anticipate their partners` decisions and set their prices accordingly. this strategic interaction –typical for real markets – should draw the subjects' attention away from their entitlements, refocusing it on the motivations and the potential WTP`s of their counterparts, as well as gains from trade possibly to be earned. Results should reveal a strong debiasing of the sellers compared to the *True Valuation* treatment.

Hypothesis$_{3a}$: *Strategic Interaction Reduces EE*

The two institutions – strategic markets and agency – should reinforce each other's debiasing effects. Strategic markets I expect to loosen the subjects` focus on their endowment, lowering their valuations of the tickets and their regret over giving them up. By contrast, agency should reduce regret over the ticket's sale, because it allows the owner to share the responsibility for the trade with an agent (Arlen and Tontrup, 2015 a). As such, the two institutions interfere on two different cognitive levels with the causal process driving the EE. Therefore they should debias subjects more effectively, potentially driving out the EE completely.

Hypothesis$_{4a}$: *Multiple Institutions can Reinforce Debiasing*

B. Regret Costs and Loss Aversion Data

Because sellers focus on their entitlements, subjects should experience more regret over selling their tickets than over forgoing the sales price. To the difference between the two values I refer as differential regret costs. Positive differential regret costs suggest that a subject is biased against trading. Differential regret costs should be large in the *Endowment* condition because of the EE. In contrast, they should be small in the *No-Endowment* condition, as subjects should not be biased towards either option, the money or the ticket. Comparing the two conditions, should yield a significant EE in *Baseline*. My loss aversion measure allows me to categorize the subjects as either loss-averse or rational types. The loss-averse types should focus their attention more strongly on their entitlements. As a result, loss-averse types should have a stronger EE than rational subjects.

Hypothesis$_{\mathbf{1b}}$: *EE in Differential Regret Costs and Loss Aversion Data*

Loosening the subjects' focus on their entitlements I expected to reduce the regret they anticipate over giving them up. Comparing the *Strategy* and the *True Valuation* treatments, the strategic market should have a significant debiasing effect. Secondly, if, as I assume, loss-averse types are more likely to be focused on their entitlement, they should also be more likely to benefit from the change of focus the strategic markets induce. Therefore, I expect the price gap between loss-averse and rational types to decrease in a comparison between the *Strategy* and *Baseline* treatments.

Hypothesis$_{\mathbf{2b}}$: *Strategic Markets Reduce Bias in Regret and Loss Aversion Data*

While subjects deliberate as to the strategic elements of interacting with their counterpart, subjects should focus less on their entitlements' positive attributes'. Strategic interaction should thereby decrease differential regret costs in comparison to the *True Valuation* treatment. As loss-averse subjects should benefit from the debiasing effect more, I expect the gap between loss-averse and rational subjects to be smaller in the *Interaction* than the *True Valuation* treatment.

Hypothesis$_{\mathbf{3b}}$: *Strategic Interaction Reduces Bias in Regret and Loss Aversion Data*

Strategic markets and agency should impact and reduce regret costs over trading with separate mechanisms: by changing the owners` attentional focus and by allowing subjects to share responsibility. The more loss-averse subjects should experience higher level of regret costs, thus leading them to ask for higher sales prices than the rational types. Reducing regret effectively should remove the price gap between loss-averse and rational subjects.

Hypothesis$_{\mathbf{4b}}$: *Multiple Institutions Reduce Bias more effectively*

C. Second Market: The Effect of Trading for Profit

I cannot directly observe whether strategic markets shift subjects` attention from their entitlements' attributes to the gains from trade. However, if participants set a strategic price, they must have devoted some attention to strategic reasoning, rather than having been focused on their entitlement alone. I expect this change of focus to reduce their bias. To test my prediction, I compare subjects who set a strategic price with subjects who ask for the same price, both in the true valuation and the strategic market. Strategic sellers should reveal a smaller EE in comparison to the *Endowment* and *No-Endowment* conditions (H_5).

Hypothesis$_5$: *Strategically Acting Subjects have a Smaller Bias.*

VII. Results

I will first report the WTA and WTP prices across all treatments to analyze the EE and the effect strategic markets have on the bias. In the second part, I present the results for differential regret costs and the loss aversion data; the result section will be concluded by the second market data, in order to analyze whether subjects who act strategically have a smaller bias.

A. Pricing Decisions

Comparing the *Endowment* and *No-Endowment* conditions of the *True Valuation Baseline* treatment shows that ownership causes a strong EE (H_{1a}). I find the characteristic gap between the mean WTA price that subjects demand for selling their ticket (€5.79) and the mean choosing price (€3.76). The price gap yields €2.03 and is significant at *p*-value <0.01 (two-tailed Mann Whitney); the cohen`s *d* of -0.82 is suggests a bias of a large effect size (see Table 1[60]).

Comparing the two conditions of the *Strategy* treatment shows that, in support of H_{2a}, the strategic market reduces the WTA/ WTP price gap considerably. In the *Endowment* condition, the sellers demand a mean price of €5.71 for their tickets; the mean choosing price in the *No-Endowment* condition is only marginally lower at €5.11. The gap between WTA/ WTP prices having thus shrunk to €0.60, the price gap becomes insignificant and the effect size becomes smaller (*p*-value 0.33 two-tailed Mann Whitney; cohen`s *d* -0.24).

[60] As it is practice in Experimental Economics, I will report non-parametric test results for the treatment comparisons and then regression results. However, the data do also fulfill the requirements for parametric testing; the results are not affected by this choice of method.

Keep in mind that subjects in both conditions strategically increase their pricing in response to the market incentives (see below for the second market data).[61]

Table 1. Non-Parametric Treatment Comparisons

Treatments	Price € (WTA / WTP)		Differential Regret Costs		Loss Aversion		
					Loss Averse	Rational	*p*-value
Endow True Value (N=50)	5.79	*p*<.01	2.13	*p*<.01	*6.62*	*3.47*	*p*<.01
No-Endow True Value (N=50)	3.76		0.28		4.04	3.47	*p*=.23
Endow Strategy (N=50)	5.71	*p*=.33	0.89	*p*=.84	*5.93*	*5.41*	*p=.53*
No-Endow Strategy (N=50)	5.11		0.49		5.25	4.82	*p*=.47
Endow Interaction (N=50)	4.54	*p*=.45	-0.24	*p*=.76	*4.86*	*4.39*	*p=.78*
No-Endow Interaction (N=50)	4.14		-0.03		3.87	4.56	*p*=.23
Endow Agency&Markets (N=50)	4.03	*p*=.72	-0.20	*p*=.95	*4.1*	*3.97*	*p=.70*
No-Endow Agency&Markets (N=50)	4.18		-0.22		4.39	4.0	*p*=.37

**p-values refer to two tailed Mann-Whitney tests.*

The *Interaction* treatment has an even stronger debiasing effect. The mean WTA is €4.54, and the choosing price in the *No-Endowment* condition differs only insignificantly from this value, with a mean WTP of €4.14. In line with H_{3a}, the treatment comparison reveals that the strategic interaction has shrunk the gap between *WTA* and *WTP* prices even further, to a difference of only €0.40 (*p*-value 0.45 two-tailed Mann Whitney) and a cohen`s *d* of -0.14, which suggests less than a small effect size.[62] In the *Interaction* treatment, the bias has not only shrunk to only one-fifth of its original size in *Baseline*. The WTA is also significantly lower than in the *Endowment* condition of the *Baseline treatment*.

Finally, I report the results of the *Agency&Markets* treatment, which presents subjects with two debiasing institutions. Supporting H_{4a}, the EE vanishes in this treatment completely: With a WTA of €4.03 and a WTP of €4.18 the price gap is even slightly reversed with a negative -€0.15 (*p*-value 0.72 two-tailed Mann Whitney; cohen`s *d* -0.06).

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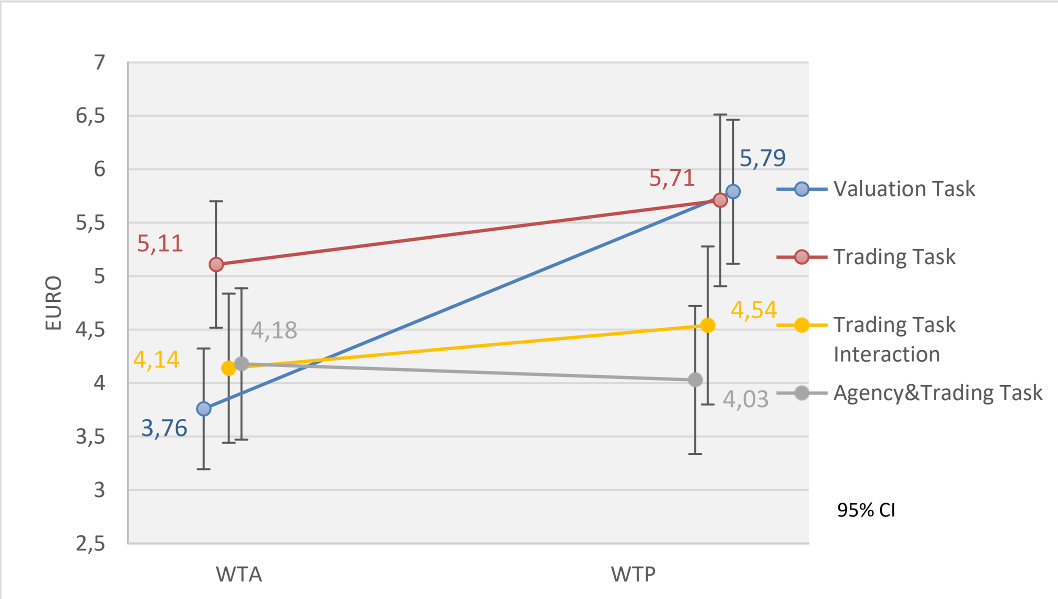
[61] At first, the reduction of the WTA/WTP gap in the *Strategy* treatment may not seem to be caused by debiasing. Subjects increasing their WTP in the *No-Endowment* condition appear to be closing the gap. However, the participants lift both the prices both in the *Endowment* and the *No-Endowment* condition when presented with the incentives for strategic trading (see the second market data section IV.3b). Because debiasing in turn reduces the price, the WTA does not rise. In the *No-Endowment* condition, by contrast, strategic behavior drives the subjects` WTP up, because the strategic market has no debiasing effect. Thus, the small gap between the WTA/ WTP prices reveals the powerful debiasing effect of the strategic market.

[62] Note also that the WTA prices in the strategic markets differ from the WTA price in the true valuation *Baseline* market. Subjects` in the strategic market have strategically lifted the price; to reach an agreement in a negotiation they may reduce the price. In the *True Valuation* treatment by contrast the WTA reflects the subjects` biased but not exaggerated valuation of the ticket. In a negotiation the owner therefore is unlikely to accept a lower price. The different nature of the two WTA prices further supports that the debiasing effect of the strategic markets creates a larger range for agreement facilitating trades.

Figure 1. Debiasing Effect of WTA and WTP Prices



Debiasing Effects. In order to show that the strategic market has a significant debiasing effect, I estimate a linear regression model, which allows us to compare the size of the EE across the different treatments. I construct dummy variables for *EndowmentStatus* and the three treatments: *Strategy, Interaction and Agency&Markets*. *EndowmentStatus* measures the difference between prices in the *Endowment* and the *No-Endowment* conditions, and thus estimates the size of the EE across the treatments. To test my main hypotheses, I construct three interaction terms. *Strategy*EndowmentStatus* measures whether the strategic market of the *Strategy* treatment reduces the size of the EE. Accordingly, the second term, *Interaction*EndowmentStatus* measures whether the strategic trading with a counterpart decreases the EE. Thirdly, the term *Agency&Markets*EndowmentStatus* estimates whether the two institutions – agency and markets – have a cumulative debiasing effect. Demographic variables are included in the model to control for age and gender. For each of the *Treatment*EndowmentStatus* interaction terms the *N* is 50 subjects.

First, the analysis confirms that ownership induces a strong bias (H_{1a}). The result shows a significant effect of *EndowmentStatus* on sales prices with β =2.030 *p*-value <0.01. My main hypothesis – that strategic markets debias subjects (H_{2a} H_{3a} and H_{4a}) – also finds strong support the *Strategy* treatment (*Strategy*EndowmentStatus* β =-1.425 and *p*-value 0.04) substantially reduces the biasing impact *EndowmentStatus* has on pricing decisions. The debiasing effect of the *Interaction* treatment is even stronger: the treatment reduces the EE with β =-1.625 and *p*-value 0.02 for *Interaction*EndowmentStatus*.

Table 2. Regressions for Prices, Regret Costs and Loss Aversion

	Column I Pricing Decisions	Column II. Differential Regret Costs		Column III Loss Aversion		Column IV Strategic Behavior
Endowment Status	2.030*** (0.486)	1.600*** (0.556)	Endowment Status	0.360 (0.369)	Endowment Status	3.357*** (0.601)
Strategy	1.345*** (0.486)	0.120 (0.556)	Loss-Aversion Type	0.761 (0.500)	Strategic Behavior	0.878 (0.413)
Interaction	0.375 (0.486)	-0.440 (0.556)	Strategy	1.393 (0.554)	Strategic Markets	-0.305 (0.368)
Agency & Markets	-0.420 (0.486)	-0.680 (0.556)	Interaction	0.420 (0.541)	Strategic Markets *Endowment	-1.817*** (0.588)
Strategy* Endowment	-1.425** (0.688)	-1.300* (0.786)	Agency & Markets	0.252 (0.510)	Strategic Behavior * Endowment	-1.964*** (0.518)
Interaction Endowment*	-1.625 (0.688)	-1.940** (0.786)	Endowment Status *Loss-Aversion Type	2.219*** (0.721)	Constant	3.558 (0.420)
Agency& Markets* Endowment	-0.2180 (0.688)	-1.580** (0.786)	Endowment Status *Loss-Aversion Type *Strategy	0.127 (0.290)	R^2	0.12
Constant	3.765 (0.344)	0.460 (0.393)	EndowmentStatus *Loss-Aversion Type *Interaction*	-2.169** (0.887)		
R^2	0.09	0.08	Endowment Status *Loss-Aversion Type *Agency&Markets	-2.861*** (0.942)		
			Agency&Markets *Loss Aversion Type	0.093 (0.852)		
			Interaction *Loss-Aversion Type	-0.193 (0.829)		
			Strategy *Loss Aversion Type	-0.182 (0.834)		
			Constant	3.559		
			R^2	0.12		

*, **, *** indicate significance at 10%, 5% and the 1% level.
Standard errors are reported in parenthesis.

Confirming the non-parametric results, I find the most effective debiasing for the *Agency&Markets* treatment to be β =-2.180 and *p*-value <0.01. As seen above the two institutions completely drive out the bias. The coefficient also suggests that the two institutions together produce the strongest debiasing effect of the three strategic market treatments (note the study with University mugs that also yielded a full debiasing results as present in the appendix). However, as the debiasing effect of the *Strategy* treatment alone is already strong, it does not leave a large enough window to show significant evidence. The regression results are reported in Column I. of Table 2.

B. Other Measures: Regret and Loss Aversion Data

1. Regret Costs

In the *Endowment* conditions, I elicit differential regret costs by subtracting the level of regret they anticipate over keeping their ticket (losing the sales price) from the level of regret subjects anticipate over selling their ticket (losing a potential lottery prize). Similarly, in the *No-Endowment* conditions, I elicit differential regret costs by subtracting the regret subjects anticipate over choosing the money (forging a potential lottery prize) from the regret the subjects expect to feel over choosing the ticket (forging the money for a ticket that may be a blank).

In the *True Valuation Baseline* treatment, differential regret costs should bias subjects against trading (H_{1b}). Indeed, in the *Endowment* condition, subjects anticipate differential regret costs of 2.13 pushing against the trade (6.71 over selling and 4.58 over keeping the ticket). In the *No-Endowment* condition, by contrast, differential regret costs approximate zero with 0.28 (5.00 over choosing the money and 4.72 over choosing the ticket). Comparing the regret costs across the two conditions shows strong support for an EE: the costs are significantly larger in the *Endowment* than in the *No-Endowment* condition (+1.85; two-tailed Mann-Whitney p-value <0.01). The strategic market in the *Strategy* treatment should reduce this bias (H_{2b}). In the *Endowment* condition of the *Strategy* treatment, subjects anticipate differential costs of 0.89 (4.60 over selling their ticket and 3.71 over keeping it). In the *No-Endowment* condition, the differential regret costs are 0.49 (4.19 over choosing the money and 3.70 over choosing the ticket). Comparing the *Endowment* with *No-Endowment* condition shows a much weaker and insignificant EE in regret costs with 0.40 (p-value 0.64 two-tailed Mann-Whitney).

Regret costs in the *Interaction* treatment provide an even stronger support for the theory that strategic markets decrease the EE. In fact, subjects anticipate experiencing more regret costs over forgoing the sales price (4.06) than over selling their tickets (3.82), leading to negative differential regret costs of -0.24 in the *Endowment* condition. In the *No-Endowment* condition, regret costs are balanced with -0.03 (4.18 over choosing the ticket and 4.21 over choosing the money). Comparing the regret costs between the *Endowment* and *No-Endowment* conditions shows that the EE has completely disappeared in line with H_{3b} (-0.21[63]; p-value=0.76 two-tailed Mann-Whitney).

Finally, the *Agency&Markets* treatment also completely wipes out the bias. With a -0.20 differential, regret costs are almost balanced in the *Endowment* condition (3.46 over selling their ticket and 3.66 over keeping it). In the

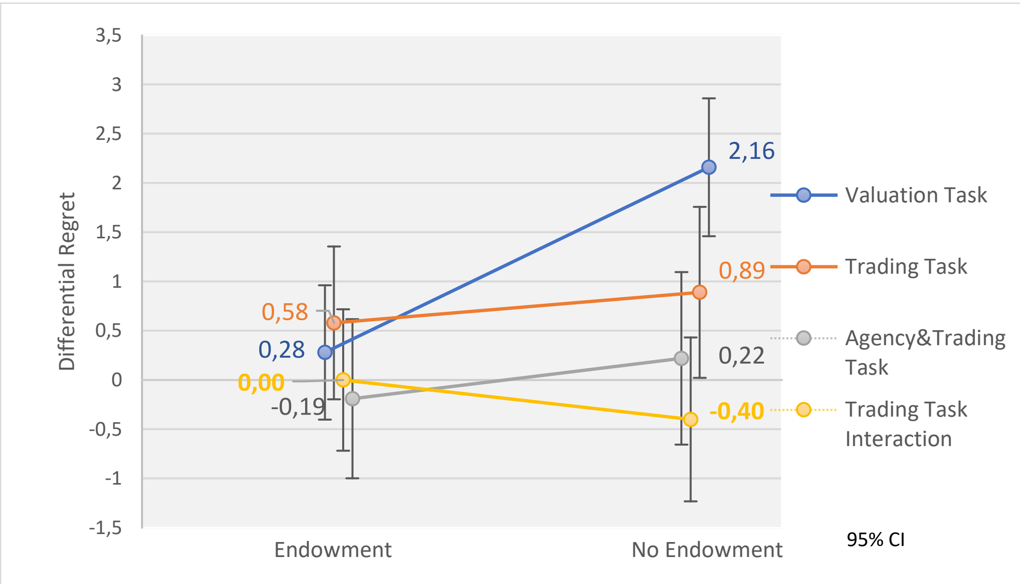
[63] The negative sign indicates that, if anything, regret pushes subjects slightly towards trading.

No-Endowment condition, the data show regret costs of 0.22 (3.58 over choosing the ticket and 3.80 over choosing the money). In line with H_{4b}, the EE has vanished: comparing the *Endowment* and *No-Endowment* conditions yields practically no bias (-0.02[64]; *p*-value=0.95 two-tailed Mann-Whitney).

Debiasing Effects. Once more, I estimate a linear model to show that the strategic markets significantly debias differential regret costs. I construct dummy variables for *Endowment Status* and the three treatments. The interaction terms *Strategy*EndowmentStatus*, *Interaction*EndowmentStatus* and *Agency&Markets *EndowmentStatus* measure the debiasing effect the treatments have on the WTA/ WTP price gap.

First, the results show evidence for an EE (H1b): EndowmentStatus biases regret costs significantly (β =1.600 p-value <0.01). Second, I find strong support for my central claim that strategic markets have a substantial debiasing effect. All three strategic market treatments – the Strategy treatment (β =-1.300 and p-value 0.09), the Interaction treatment (β =-2.039 and p-value 0.01) and the Agency&Markets treatment (β =-1.580 and p-value 0.04) – significantly decrease the bias in respect to regret costs. The complete set of results are reported below in table 2, Column II.

Figure 2: Debiasing Effect on Differential Regret Costs

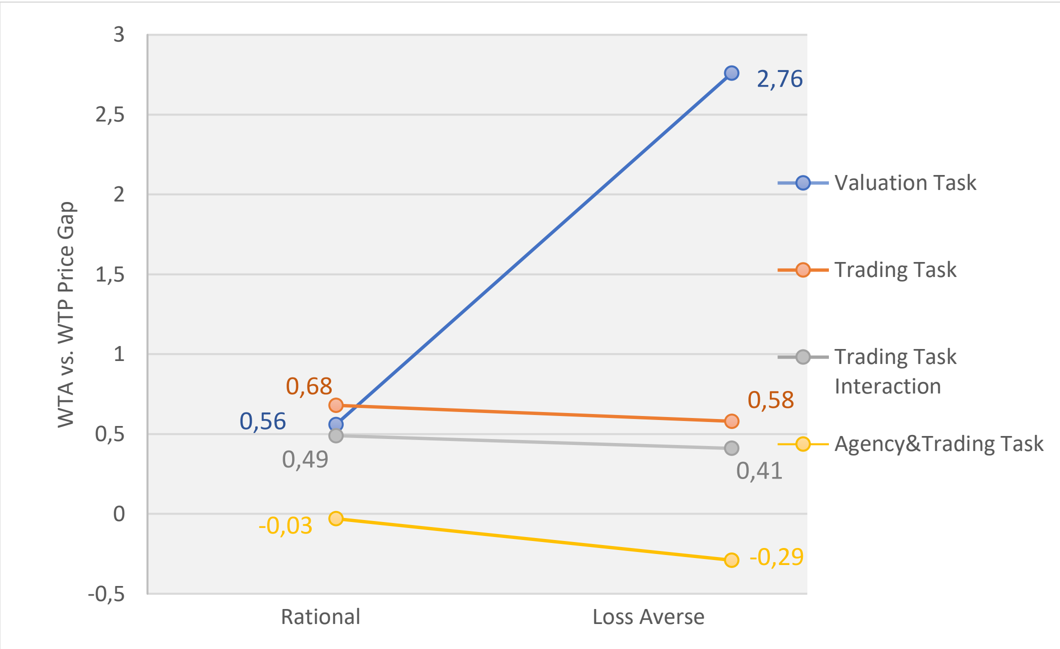


[64] The negative sign indicates that, if anything, regret pushes subjects slightly towards trading.

2. Loss Aversion Data

I distinguish between rational subjects who enter both lotteries and loss averse subjects who enter either only one or no lottery.[65] As hypothesized (H_{1b}) in the *Baseline* treatment, the loss-averse participants (*N*=61) reveal a strong bias of €2.58 (*p*-value <0.01 two-tailed Mann-Whitney), while the rational types (*N*=39) reveal a much smaller gap between WTA/WTP prices at €0.56 (*p*-value 0.38 two-tailed Mann-Whitney).[66] Supporting predictions H_{2b}, H_{3b} and H_{4b}, all three strategic market treatments reduce the price gap and leave both loss averse and rational types without a significant EE. In the *Strategy* treatment I find a gap of €0.68 for loss-averse ((*N*=62; *p*-value 0.61 two-tailed Mann-Whitney) and a gap of €0.59 for rational types ((*N*=38; *p*-value 0.58 two-tailed Mann-Whitney). In the *Interaction* treatment, I measure €0.41 (*p*-value 0.49 two-tailed Mann-Whitney) for loss-averse (*N*=58) and €0.49 for rational subjects (*N*=42; *p*-value 0.70 two-tailed Mann-Whitney).

Figure 3. Debiasing Effect by Subject Type (Loss Averse vs. Rational)



Debiasing Effects. To measure the strategic markets\` effects on both rational and loss-averse subjects, I estimate a linear model, including dummy variables

[65] Distinguishing between stronger and weaker loss-averse subjects does not change the results.
[66] Results contain a few subjects with inconsistent choices, who enter the first but not the second lottery, even though the likelihood that they will experience a loss is considerably smaller in the second lottery. The results I report here include these subjects, because the subjects\` decisions nevertheless suggest that they are loss-averse. The results do not change when the inconsistent observations are removed from the sample.

for *EndowmentStatus* and *LossAversionType*, as well as one for each of the three strategic market treatments. The term *EndowmentStatus*LossAversionType* reveals whether loss-averse subjects have a stronger bias than rational types. The two-way interaction terms *EndowmentStatus*LossAversionType* *treatment measure the debiasing effect the three market treatments have on the EE. As expected, *EndowmentStatus*LossAversionType* suggests that the WTA/ WTP gap for loss-averse subjects is significantly larger than for rational subjects, whose bias is much smaller (β =2.219 *p*-value <0.01).

Each of the market treatments significantly reduces this gap between the two subject types: for *EndowmentStatus*LossAversionType*Interaction* the results show β =-2.169 *p*-value 0.01); the two-way interaction with the *Strategy* treatment yields β =-1.905 *p*-value 0.02; and, finally, for the interaction with the *Agency&Markets* treatment, I measure the strongest debiasing effect of β =-2.861 *p*-value <0.01.

C. Second Market: Strategic Sellers have a Smaller Endowment Effect

In order to provide further evidence that focusing on the incentives of the strategic market reduces subjects` EE, I compare subjects` pricing decisions in the first and second markets. Subjects who ask for a higher price in the first strategic market than in the second true valuation market reveal that they do not focus their attention on their entitlement alone, but also on the profit they can make in the trading process. The focus theory suggests that these strategically-acting subjects should have a comparatively smaller bias (H_5). In the *True Valuation* treatment, strategic subjects may ask for a higher price in the second strategic market. Both types – strategic and non-strategic subjects – should be equally biased.

To test my prediction, I elicit the gap between WTA/ WTP prices of participants who set a strategic price, then comparing them with the subjects who did not try to earn an extra profit in the strategic market. I pool the *Strategy* and the *Interaction* treatments. The results support my prediction. Strategic types do indeed not reveal an ownership bias; they ask for almost exactly the same price in the *Endowment* than the No-*Endowment* condition (€ 0.02; Mann Whitney *p*-value 0.77).[67] The subjects who did not make a strategic decision, by contrast, reveal a larger gap of €0.98 between WTA/ WTP prices (Mann Whitney *p*-value 0.14).[68]

[67] Results for the *Strategy* treatment (strategic: 0.01; non-strategic:1.4) and the *Interaction* (strategic: 0.06; non-strategic:0.72) treatment show the same pattern.

[68] The subjects who do not set a strategic price may also focus some attention on the incentives in their decision- making process, even though they do not decide in the end to set a strategic price. For

As expected in the *True Valuation Baseline* treatment, in the absence of a debiasing effect, the gap for both types is similar: for strategic types I find €2.06 (Mann Whitney *p*-value <0.01) and €1.82 for non-strategic types (Mann Whitney *p*-value <0.01).

I estimate a linear model to test whether the strategic types are, as hypothesized, significantly less biased by their endowment status than the non-strategic types. A dummy variable captures the subject type. The dummy variables of *EndowmentStatus* and *StrategicMarket* serve to distinguish whether or not subjects were endowed and placed into a strategic market. I construct the following interaction terms. First, the EE should decrease when subjects are placed in a strategic market, as measured by *Strategic Market*Endowment Status*. Second, the central claim that the size of the EE measured will depend on whether the subjects act strategically or not – is captured by *Subject Type*Endowment Status.*

Supporting H_5, the interaction is significant (β =-1.945 and *p*-value <0.01), suggesting that subjects who made a strategic pricing decision have a significantly smaller bias. The finding fits an attention-based explanation for the debiasing effect of strategic markets: when subjects act strategically, they focus less attention on their entitlement and therefore have a smaller or no bias at all. Interestingly, the main effect of *SubjectType* on prices is insignificant (β =0.305 and *p*-value 0.41), suggesting that strategic behavior is not generally associated with overall higher prices. My theory explains this finding: the smaller EE of the strategic subjects counterbalances the price increase caused by their strategic trading behavior. The full regression results are reported in Column IV. of Table 2.

VIII. Discussion and Policy Implications

A. Discussion

In their seminal series of papers, *Plott* and *Zeiler* (2005, 2007, 2011) have identified confounds and misconceptions in the experimental design of many EE studies. I attended to their critique designing this study. For example, *Plott* and *Zeiler* have argued that endowing subjects with goods selected by the experimenter deliberately for them may signal to the participants that the good they receive is of particular value. This perception may then cause endowed participants to ask for a higher sales price. This confound is avoided by allocating the tickets randomly (see design section); the instructions inform the subjects about the protocol that I use for the random assignment. The instructions also

my purposes, the opposite claim is sufficient: the subjects who set a strategic price did focus some attention on the market incentives.

clarify that a software program has sampled the seven numbers tipped on the lottery tickets.

Plott and *Zeiler* (2005, 2007 and 2011) also suggested that random allocation experiments often impose higher transaction costs on trading than keeping the entitlement. For example, in some studies, experimenters seated all participants in a classroom, with subjects who wanted to trade needing to raise their hands. Additionally, if subjects who want to keep their entitlements can remain inactive, they may feel less responsible for the outcome, in comparison to the subjects who have to act in order to trade. Action triggers more responsibility than omission (Baron et all., 1994). The more responsible the subjects feel, the more regret they may anticipate over potential negative outcomes to their decisions. In other words, the gap between WTA and WTP prices may increase because of an omission bias, and not because of loss aversion. In my study participants had to perform the same action, whether they wanted to trade or keep their entitlement. In both conditions, the subjects are presented with pricelists and must in the *Endowment* condition state for each price whether they want to sell or keep the tickets and in the *No-Endowment* condition, they must state whether they prefer the money or the ticket.

This study`s experimental design also prevents the subjects from influencing each other's choices: When participants seated in the same room raise their hands in order to trade, this may affect the choices of other subjects, who might be induced to follow the example (see Arlen and Tontrup, 2015b). In the online study, participants could not observe the behavior of other subjects, remaining anonymous during the entire experiment. Finally, *Plott* and *Zeiler* suggest that subjects may not understand that the random price mechanism incentivizes them to reveal their true valuation. They show that the BDM mechanism can lead some subjects to make confused pricing decisions. To avoid this problem, as well as to teach the subjects how the mechanism operates, the subjects were presented with a set of training scenarios and control questions (all instructions are provided in the appendix). Subjects could only proceed with the experiment if they had completed the questions correctly. The participants – current university students and university graduates - did not show any difficulty understanding the experimental design and instructions. In particular, the subjects recognized in the *True Valuation* treatment that they would be paid the random price, not the amounts they demanded for selling their tickets. That they were required to perform payoff calculations made the fact that they could not increase their profit by asking for a higher price salient. In the *Strategy* treatment, by contrast, the subjects realized that they could benefit from strategic behavior by pushing the price beyond their true valuation. Their answers also showed that the subjects were aware, that by overstating their valuation, they could risk losing all gains from trading. The participants appeared to understand the treatments that employed the random price mechanism with equivalent ease to those involving interaction with a real counterpart.

The replication of the EE in the *Baseline* treatment succeeds a recent series of studies observing an EE while also attending to *Plott* and *Zeiler's* methodological critique (see Isoni et al. 2011; Arlen and Tontrup, 2015a, b; Marcin and Nicklisch, 2016 and others). This evinces that, even after *Plott* and *Zeiler`s* push for improving experimental protocols, the EE remains a robust phenomenon of human decision-making.

Potential experimenter demand effects are avoided by maintaining the participants` anonymity throughout the entirety of the study, including the payment process. To ensure full privacy, I prompt the subjects to create anonymous e-mail accounts and to invent personal codes allowing us to pay them without forcing them to reveal their identities. In order to prevent the participants from deducing the study's goal, I only informed the subjects about the particular treatment in which they participated.

Studies that use a random price mechanism often provide subjects in the *No-Endowment* condition with an initial monetary endowment, which can be used to buy the entitlement. This design leads to varying wealth levels in the two conditions, in particular because the budget needs to be large enough for higher WTP`s to not be truncated. Strictly speaking, this design varies two factors at once: endowment status and budget. The choosing design that implemented for eliciting the subjects` valuations does not require allocating budgets, permitting us to vary only the endowment status across treatments (see Johnson 2007).

My results also cover both effect channels of the EE that the literature has presented evidence for: anticipated regret as well as attachment and sentiment (see Korobkin, 2013).

B. Policy Implications

1. Legal Interventions and their Information Problems

The EE has attained such prominence in the legal literature for two reasons. First, the bias is immediately relevant for the efficiency of trading. Private ordering rests on the idea that, supported by an infrastructure of institutions, people will voluntarily trade their entitlements until they are owned by those who value them the most. The more the EE affects trading behavior, the greater the chance that more goods could end up in the hand of owners who, in comparison to other market participants, would value those goods considerably less.

The second reason why the EE appears to severely threaten private ordering derives from its effect size. A meta-analysis of 45 empirical studies by Horowitz and McConnell (2002) found a median gap between WTA and WTP prices with a ratio of 2.6. Following this analysis, people will assign a 2.6 times higher value to the same good once they own it. The size of the EE can even increase by a factor of 10 in relation to health and safety goods (see also the meta-analysis by Tunçel and Kammitt, 2014). If this data on the EE held for transactions in real markets, many buyers and owners would not be able to identify – let alone complete – an exchange that is beneficial, absent the bias.

Given the wealth of empirical evidence about the EE, it is not surprising that one can find many proposed interventions to address the bias in the legal literature: Korobkin (2013) counted more than 1,600 articles. Categorizing the suggested interventions, two general types can be distinguished. A first type aims to reduce the EE directly, by weakening people's feeling of entitlement for their property (Buccafusco and Sprigman 2011; Korobkin 2000; Rachlinski and Jourden 1998). In one example, the authors propose replacing property with liability rules to weaken owners' entitlements. The absence of EE property rules might allow private ordering to more efficiently allocate entitlements. However, in a world where entitlement holders are biased by ownership, limiting entitlement property rights could increase efficiency. Liability rules also enable involuntary appropriations, allowing those who value the goods more to gain entitlements for prices below the owners' biased valuations (Calabresi and Melamed, 1972).

A second category of interventions aims to attenuate the harm the EE inflicts without trying to remove the bias. For example, there are a number of scholars who want to adopt contractual default rules and resource allocations favoring groups who will supposedly value the property more than the current owners, but who will not enter a trade as long as the current owners` demand is biased (e.g., Jolls 2000; Jolls et al. 1998; Korobkin 1998; Sunstein 2002).

Both types of policy interventions impose costs and face a severe information problem. Ideally, policymakers would observe people's actual choices to identify the default contract terms most likely to maximize welfare (Coase 1960). However, if most market participants are biased, then policymakers cannot rely on the market; they have to adopt provisions they expect to be welfare-enhancing on theoretical grounds (see Sunstein, 1986, 2002). This is a very difficult task, as valuations are deeply subjective, affected by an endless number of personal motives, uncertainty and change.

In a related problem, policymakers cannot differentiate between those actors who are biased and those who are not, and cannot identify what transactions would occur if the bias was absent. This makes it difficult to determine how strongly the EE really affects pricing in a particular market. If the EE does not affect the majority of owners, then liability rules cannot substantially increase the number of transfers that would occur under a regime of property rules. Nevertheless, the intervention would impose costs on all owners, independently of whether they are, in fact, biased or not (see Arlen and Tontrup, 2015a). Whenever the presumption that the EE impacts most trading behavior in the targeted area is invalid, the intervention is likely to be inefficient.[69] My results suggest that a general presumption that the EE biases and impairs trading behavior may often be false and may produce more costs than benefits.

2. Strategic Markets Can Eliminate Endowment Effect

When lawyers recommend policy interventions that aim to attenuate the impact of the EE, their recommendations seem to be thoroughly justified by a large body of empirical evidence. This study suggests, however, that much of the evidence may ultimately mislead policy recommendations, as it does not speak to the main legal concern: whether or not the EE substantially impairs private exchange. The two standard experimental paradigms used in EE studies – the random allocation and the random price design – incentivize subjects to reveal the true valuations they accord to their entitlements (see the theory section). Both paradigms were developed to analyze and proof various phenomena of loss aversion and reference dependence. They present subjects with a single task: figuring out the personal value the good holds for them. In order to decide what price to request, the subjects need only to process and weigh the good's value-increasing and value-decreasing attributes; other information is not relevant and is not provided to the participants.

Trading in real markets has little in common with this simple valuation task. The goal of market exchange is typically to earn a profit, presenting sellers with a much more complex cognitive task. Sellers will study market prices and their trends; they will try to anticipate the buyers' actual WTP; they will consider their partners' business reputation and solvency; and they will take into account the potential for future deals, as well as many other relevant economic factors. After all, professional sellers will be more concerned with potential buyers than with their own valuations for the sales goods. The theory and evidence presented here suggest that the much more cognitively complex task of trading for profit loosens the owners` focus on their entitlements, thereby reducing or even eliminating their bias. The strategic complexity of the cognitive task required of participants increases across the three treatments, approximating actual trading behavior. The *True Valuation* treatment presents subjects with a standard design focusing subjects on their entitlements, as only the

[69] It requires field work to analyze whether a WTA/ WTP price gap exists in a particular market or not.

good`s attributes are relevant to their pricing decisions. In the *Strategy* treatment, by contrast, participants have to make a strategic trading decision: given the distribution of prices that may be randomly drawn, they have to find a balance between the potential profit gained from pushing prices beyond their true preferences, and the risks they may be willing to take that could spoil the deal. In the *Interaction* treatment, finally, seller and buyer interact with each other, requiring them to anticipate and respond to the (potentially) strategic behavior of their counterpart. The second market results demonstrate that subjects who respond to the incentives and ask for a strategic price beyond their true WTA do not reveal any EE at all. The finding stands in line with the body of literature reporting that professional traders, who trade for profit by definition, often do not reveal any bias (see List, 2003). *List* attributes this finding to the professional sellers` experience and better knowledge of market prices. On the contrary, the theory and evidence of this study suggest that traders may focus their attention fully on gains from trade because their personal valuations of the goods hold little relevance to the prices they demand.

My findings do not question the assumption that the EE is a robust phenomenon of human decision- making. The study is concerned with the policy and legal relevance of this bias. The data show that the size of the bias is heavily dependent on the cognitive task presented to the owners, or that they wish to solve. If they are presented with the task of valuing their entitlements, they focus their attention on their entitlements' positive attributes, with ownership strongly biasing their valuations, as shown in many previous studies. However, when sellers trade in a market context, in which pricing decisions are strategic and in which their goal is to earn a profit, the EE shrinks dramatically - leaving only insignificant evidence of a very small effect size. When trading in strategic markets involves also other market institutions, like agency, the bias seems to disappear entirely.

3. Policy Relevance of Theory and Evidence

As people characteristically trade for profit, my results should apply to many, if not most transactions. Strategic trading behavior is ever-present in markets with incomplete competition – for example, whenever goods are not standardized, and prices are not easily comparable or transparent. Assets with uncertain prospects are often priced with a large prognostic element, such as when patents are licensed or sold. The more difficult the future return is to estimate, the larger is the space for strategic pricing and bargaining. The same holds true for real property or used goods, whose value is often uncertain. The trading situation can also assign strategic market power. In a hold up scenario, for example owners may focus their attention primarily on how far they can push up the price without jeopardizing the trade. Hidden transaction costs can also complicate the direct comparison of prices, enabling market participants to strategically exploit a lack of transparency. For many products and services, bargaining over additional terms is typical.

In real markets the strategic complexity of the decision-making task will often be considerably higher compared with this study which supports the ecological validity of the findings. Traders in real markets face thick layers of strategic incentives that can reach far beyond the immediate trading context: reputation costs for future deals that prevent sellers from taking advantage of their partners; the long-term price trends of their entitlement, suggesting to either sell or keep their stocks and real property; outside offers that may occur at a later point; other deals they could use the sales price for; promotion; and many more. Trading encompasses the consideration of all these strategic elements, which reduces the sellers` focus on their entitlement, and as a result, reduces their bias.

My results cover both effect channels of the EE that have been analyzed in the literature: anticipated regret as well as attachment and sentiment. Therefore, the debiasing results apply to both, goods of uncertain value like stocks or patents as well as physical (consumer) goods that are valued for attributes like usability or beauty and that trigger sentiment like cars, real property or jewelry. While in the legal literature the two effect channels are often discussed separately my results are consistent with the shift of attentional focus being the underlying mechanisms behind both effect channels.

In order to estimate the actual relevance of the EE for private ordering, policy interventions should also consider the interplay of all institutions holding debiasing potential. Their debiasing effects may reinforce each other, should the institutions provide separate debiasing mechanisms that interfere with the process causing the bias. Professionals typically employ many institutions when they trade, the most common being the ones tested in this study: strategic markets and agency. My theory and evidence indicate that strategic markets reduce owners' attentional focus on their property, thereby debiasing their valuations of the goods; positive attributes. Agency, by contrast, reduces the regret owners anticipate over trading, as they can share the responsibility for the trade (Arlen and Tontrup, 2015a). Presented with both institutions in the same treatment, the subjects did not reveal an EE. Because trading will very often involve both institutions, my findings hold important implications for future policy-making.

IX. Conclusions

In this study, I have presented evidence that trading in strategic markets in off itself minimizes or even fully eliminates the EE. The evidence I present accounts for both effect channels that have been discussed in the literature to trigger the EE: anticipated regret as well as attachment and sentiment, as I conducted separate studies with goods of uncertain value (lottery tickets) and with physical goods (University mugs). The results have been the same showing in both cases a dramatic debiasing effect.

The results may surprise given the huge empirical evidence that both Psychology and Economics have collected demonstrating that the EE is a robust phenomenon of human decision making. This study does not cast doubt on the validity of these results. Instead, I argue that, while these studies are tailored to promote theory-building and -testing of Prospect theory and others, they disregard the strategic nature of the private exchanges with which the law is concerned. This leads to policy recommendations seeming to overestimate the relevance of the bias for private ordering. Typically, EE experiments incentivize participants to state their true valuations for the entitlements that they can buy or sell. This experimental design is well-suited to show that valuation varies with reference points, to proof loss aversion, and to underpin Prospect theory. However, as it elevates the bias in particular in comparison to a strategic market context, this design can mislead legal policy.

When people trade, their goal is typically to earn a profit from trading. Their behavior is often strategic, and they process a lot of information holding little connection to the relevant good's attributes and the valuation they personally accord to it; market prices and trends, their personal reputations in business and their communities, the option value of future deals, or strategic situations like hold-ups. Their true valuation of goods is only one cue among many relevant to their ultimate pricing decision.

Because ownership focuses peoples` attention on the positive attributes of their property and biases their valuation, losses loom larger than gains. My theory and evidence suggest that the strategic elements of trading draw the sellers` attention away from the entitlement to gains from trade. The strategic market eliminated more than 80% of the regret induced EE in the lottery ticket study, meanwhile in the follow up experiment (see appendix) with physical goods the EE disappeared completely, when subjects traded for profit. Supporting an attentional focus theory, the subjects who set strategic prices did not reveal a bias at all, in line with the observation that most professional traders are not affected by the bias (List, 2003).

My evidence suggests also that experience might play a smaller role in eliminating or reducing price gaps in real markets than discussed by some in the literature (Shogren 2003, Plott 1996, List 2003). The shift of attentional focus is an automatic cognitive process, not a learning process. This suggests that also markets in which lay persons or lay persons with professionals interact may not be subjects to strong endowment effects.

Finally, this study demonstrates the importance of taking all potential sources of debiasing into account. When people trade in markets, they typically use more than one institution that can help them debias. When I placed my subjects into strategic markets and provided them with agents for trading their lottery tickets, the EE disappeared entirely.

The legal literature has proposed many interventions to prevent the EE from affecting private exchange. These external interventions cannot distinguish whether a transaction does not occur due to the owner's bias or because the transaction is not beneficial, their central problem. Liability rules, for example, weaken all owners' senses of entitlement, whether they are biased or not. External interventions operate on the presumption that the EE substantially impairs trading in the target area, otherwise they are inefficient. My results suggest that this presumption may often lack validity. Interventions should be undertaken cautiously, based upon the existence of actual empirical evidence of relevant price gaps in a particular market. Otherwise, they may impose more costs than benefits.

X. Appendix – Testing Attachment and Sentiment

A. Theory

In addition, I conducted a follow up study using physical goods: University mugs. My attentional focus theory suggests that subjects debias because the trading process directs their attention away from their entitlement and towards potential profits. This debiasing process should equally impact both effect channels reported in the literature: anticipated regret and attachment. Thus, my results should likewise apply to goods of uncertain value that trigger regret over the trade and physical goods that induce attachment due to their sentimental or aesthetic value. Based on the attentional theory, I therefore expect to observe the same debiasing effect observed for lottery tickets in the case of university mugs.

B. Study Design and Methods

The treatment design of the follow-up study differs only from the studies reported in the main paper insofar as University mugs are used instead of lottery tickets. Two treatments are implemented, the *True Valuation* treatment and the *Interaction* treatment; the instructions given to participants were the same. Subjects could indicate prices ranging from €0.25, 0.50, 0.75 … up to €10 for the University mug. I recruited 60 subjects for each treatment: 30 subjects for the *Endowment* and 30 subjects for the *No-Endowment* condition. Every fifth subject was paid, with the subjects either receiving the price or the mug. In both treatments the subjects were presented with a short video featuring the mug, which allowed them to inspect the mug's appearance in high definition.

The subjects in the *Endowment* condition were sent a numbered receipt that enabled them to pick up a mug in the University shop. Participants were instructed that, should thy sell their mug in the course of the experiment, they would give up their right to claim the mug.

In addition, I presented the subjects with a survey aiming to measure the degree of attachment they felt for the mug. Following Strahilevitz and Loewenstein (1998), I asked the subjects to rank six different objects, including the mug, according to the objects' perceived desirability.

C. Hypothesis and Results

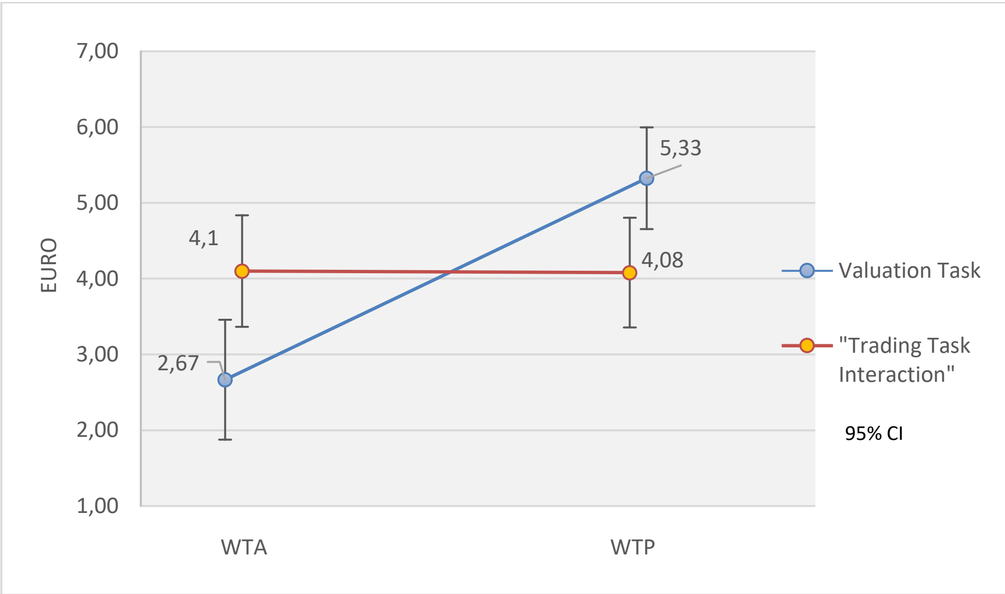
I hypothesized a replication of the EE in the *True Valuation* treatment: Indeed, the data show the expected price gap, with a WTA price of €5.32 in the *Endowment* condition versus a WTP price of €2.66 in the *No-Endowment* condition. Thus, in line with the results reported in the literature, endowment status more than doubled the price of the University mug in the *True Valuation* treatment, providing significant evidence for an endowment effect (Mann-Whitney p-value <0.01).

The *Interaction* treatment by contrast should reduce the bias by focusing the subjects on gains from trade and the potential WTP of the second player. The debiasing hypothesis was confirmed: trading in the strategic market completely eliminated the EE. I find a WTA price of €4.08 in the *Endowment* and €4.10 in the *No-Endowment* condition (Mann-Whitney p-value 0.98).

D. Debiasing Effect

To demonstrate that trading in the strategic market significantly debiased the subjects, I estimated a linear regression model comparing the EE's size in the two treatments. Dummy variables for *EndowmentStatus* and treatment are included in the regression. *EndowmentStatus* measures the difference between prices in the *Endowment* and the *No-Endowment* conditions, and thus estimates the EE's size in the treatments. To test my main hypotheses, I construct the interaction term *Treatment*EndowmentStatus,* which estimates whether the *Interaction* treatment reduces the size of the EE.

Figure 4. Debiasing Effect on Physical Goods



The results show *EndowmentStatus'* significant effect on sales prices, with β =2.658 *p*-value <0.01, confirming a strong bias in the *True Valuation* treatment. The main hypothesis – that trading in the strategic market debiases subjects – finds strong support in the results too, as the *Interaction* treatment significantly reduces (in fact eliminates) the bias (*Treatment***EndowmentStatus* β =-2.675 and *p*-value <0.01). I thus conclude, as hypothesized, that the debiasing mechanism impacts both effect channels of the EE and, therefore, reduces or eliminates the EE for both goods of uncertain future value like patens or stocks and physical consumer goods.

The survey results do not only confirm the debiasing evidence provided by the main experiment; they also show that the mugs trigger attachment, activating the EE`s second effect channel in line with Strahilevitz and Loewenstein`s work. In the *Endowment* condition of the *True Valuation* treatment, subjects ranked the University mug as being significantly more desirable (2.40) than did subjects in the *Endowment* condition of the *Interaction* treatment (3.07; Mann-Whitney *p*-value 0.04).

This difference did not occur in the *No-Endowment* condition, as I find 2.91 for the *True Valuation* and 3.23 for the *Interaction* treatment (Mann-Whitney *p*-value 0.48). Comparing the *Endowment* and *No-Endowment* conditions yields an EE in the *True Valuation* treatment (Mann-Whitney *p*-value 0.08), but no bias in the *Interaction* treatment (two-sided Mann-Whitney *p*-value 0.71).

Chapter 3 – Debiasing through Social Context

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Abstract

Anticommons are a special kind of mixed-motive dilemma in which negative effects for society are caused by the excessive use of exclusion rights. In two fully incentivized experiments on trading goods with risky prospects, I disentangle three psychological sources that have been suggested to contribute to increased pricing in anticommons dilemmas: the effects of strategic incentives for overpricing, endowment effects, and interdependence of outcomes. My results show that pricing of risky prospects in the anticommons is only marginally influenced by endowment status, whereas participants readily respond to incentives to overprice and to the interdependence of outcomes. Endowment effects are reduced both when strategic incentives to overprice are provided and when outcomes of subjects become interdependent. As a result, endowment effects for risky prospects are strongly reduced or even disappear completely in anticommons dilemmas. My results render support for an interaction model instead of an additive effect model in which both incentives and endowment effects would drive up pricing.

I. Introduction

Imagine a scenario in which three firms own biomedical patents and a pharmaceutical company suspects that a mechanism building on these three patents could potentially lead to the development of an effective drug against a particular form of cancer. The company would certainly want to license the patents from the owners. As all three patents are needed, a successful deal would require agreements with all three owners to license their patents to the company. Whether this transaction will occur depends on many factors, the valuation of the patents by the company, and by the owners being of crucial importance. Both valuations will reflect the immense uncertainty of the project; no specific probability is known whether the drug, which the company considers to develop, will be a success. Also, the validity and enforceability of patents is difficult to determine ex ante and creates significant uncertainty (Crouch, 2008; Lemley and Shapiro, 2005; Scherer, 2001; Scherer and Harhoff, 2000).

This scenario, in which multiple owners hold effective rights of exclusion in a scarce resource, is referred to as the dilemma of the anticommons in the literature (Heller, 1998; Heller and Eisenberg, 1998). Anticommons resemble a specific type of dilemma of mixed-motive decision making, which are generally characterized by conflicting self-interests and collective interests, that is, maximizing self-interest reduces overall outcomes for the group (for overviews, see Balliet, Parks and Joireman, 2009; Dawes, 1980; Komorita and Parks, 1995; Van Lange, Joireman, Parks and Van Dijk, 2013). Assuming that the pharmaceutical company in my example has a marginally higher willingness to pay (WTP) for the patents than the current owners' aggregated willingness to accept (WTA), it would be in the best interest of all parties if the three current owners truthfully revealed their WTA for selling their patents so that the transaction could be concluded and gains from trade realized and shared.

Nevertheless, as personal valuations of the patents are uncertain and not directly observable, each of the current owners could try to maximize personal profit by strategically asking for prices that are higher than his true valuation. Consequently, the WTA that the current owners actually demand could be higher than their true valuations of the patent they hold. This strategic bidding could lead to the worst outcome for all, namely that the sum of WTAs stated by the three owners is higher than the WTP of the pharmaceutical company, although the sum of their true valuations of the patents is not. In this case, the patents would not be licensed, with potentially tremendous effects on the welfare of society if such market failure occurs systematically (Heller, 1998). This scenario has led to an extensive debate in intellectual property and innovation research how strategic behavior of innovators in anticommons situations may impede the licensing of intellectual property rights, thereby drying out markets and decreasing social welfare (Heller and Eisenberg, 1998; Lemley and Shapiro, 2007; Murray and Stern, 2007; Scherer, 2002).

The game-theoretic structure of the anticommons dilemma is conceptually a reversed form of the prominent tragedy of the commons, and both are immensely important for core challenges of society (Hardin, 1968). In the commons dilemma, pool resources such as fishing grounds, common land, or water supply are overused if people cannot be effectively excluded from use. In the anticommons dilemma, by contrast, resources are underused as multiple individuals hold exclusion rights and use them strategically (e.g., by overcharging third parties who want to access the resources). Although economists have shown that both kinds of dilemmas are symmetrical from a game-theoretic perspective (Buchanan and Yoon, 2000), it is most likely that individuals perceive them as very different situations. Indeed, previous empirical work suggests that actual behavior in commons and anticommons dilemmas diverges considerably. Most importantly, it has been shown that the negative welfare effects of strategic behavior, which result from either overpricing or overuse, are more severe in anticommons than in commons dilemmas (Vanneste, Van Hiel, Parisi and Depoorter, 2006). Yet whereas the tragedy of the commons has received major interest, the empirical analysis of the different behavioral factors driving the anticommons dilemma is so far much less advanced.

I argue that pricing decisions in an anticommons dilemma are characterized by three basic features of the game that are likely to influence the valuations that individuals state. First, individuals are endowed with a good or right that they can sell; hence, pricing might be increased by an endowment effect (i.e., individuals ask for higher prices if they sell an object as compared with the amount of money they are willing to pay for buying the very same object, WTA > WTP; Kahneman, Knetsch and Thaler, 1990). Second, individuals have an incentive to state a price that is higher than their true valuation of the good in order to increase their share of the gains from a potential trade, which leads to an effect of strategic overpricing (Heller, 1998). Third, the outcomes of all group members are interdependent, that is, individual pricing decisions can impose negative externalities (i.e., negative monetary consequences) on others. This may occur, for example, if one owner spoils the deal for the others by demanding an excessively high price for his good (Dhont, Van Hiel and De Cremer, 2012). If people are motivated by other-regarding preferences (e.g., De Cremer and Van Lange, 2001; McClintock, 1972; Van Lange, 1999), they might want to avoid those externalities on others.

Explaining behavior in anticommons situations is complicated by the fact that it is unclear to what extent behavior is driven by one of these three psychological factors or a potential interaction of them. In two studies, I aim at disentangling the effects these factors have by varying the outcome structure in a joint lottery buying/selling task (i.e., participants buy/sell bundles of lottery tickets in anonymous groups of three). I independently manipulate endowment status (yes versus no) and use three different outcome structures. This setup allows us to disentangle the effects of the three influencing factors in a (non-fully crossed) factorial design: outcome interdependence; incentives for strategic overpricing, both varied within the factor outcome structure; and endowment effect. The actual anticommons situation is one cell in my design.

Specifically, I test an additive model assuming that endowment effects and strategic incentives to overprice drive up prices independently (i.e., endowment effects and strategic incentives to overprice are additive and interdependence has no further influence) against an interaction model that assumes, by contrast, that endowment effects are reduced by incentives and outcome interdependence (i.e., endowment effects and strategic incentives to overprice are not additive).

In the following, I first summarize previous findings on both the anticommons dilemma and the endowment effect. I then develop the competing additive and interaction models and derive hypotheses. Finally, I present and discuss the design and results of my experiments.

II. Previous Research

A. Anticommons Dilemma

Although behavior in the commons has been the subject of reams of studies in a wide range of disciplines such as psychology, economics, and political science (e.g., Dawes, McTavish and Shaklee, 1977; Gillet, Schram and Sonnemans, 2009; Hine, Gifford, Heath, Cooksey and Quain, 2009; Joireman, Posey, Truelove and Parks, 2009; Ostrom, 1990; Walker, Gardner, Herr and Ostrom, 2000), the empirical investigation of the anticommons dilemma has started only recently. As commons and anticommons are symmetrical from a rational decision-making perspective in providing the same incentives for strategic decision making (Buchanan and Yoon, 2000), one could expect the same behavior and findings in both situations. In their first empirical study of the anticommons, Depoorter and Vanneste (2006) showed that, in line with theoretical predictions (Schulz, Parisi and Depoorter, 2002), the welfare losses of foregone trades increase the more the property is fragmented among multiple owners and the less the good is substitutable for the buyer. Furthermore, the authors show that, when individuals decide on their WTA and set prices, they focus on the expected profit that the buyer of the rights bundle could make with the trade. Sellers ignore the objective value of their good and consider only how they think profit should be shared. Uncertainty about the buyers' profit seems to amplify this effect of overpricing, making individuals demand the maximum they expect they can skim from the purchaser's profit rather than asking for a fair proportion of expected gains.

This first paper led to a line of empirical studies providing converging evidence that human behavior in the commons and anticommons differs systematically. Vanneste et al. (2006) showed that participants' pricing increases in anticommons compared with commons dilemmas, boosting welfare losses. The authors conclude that it is inadequate to extrapolate findings from the commons to the anticommons dilemma and argue that behavioral effects such as the endowment effect might cause the observed differences. Specifically, people sell a piece of their property in the anticommons framework, which might trigger an endowment effect, whereas behavior in the commons typically does not involve trading at all.

Other studies find differences in how cooperative and non-cooperative behavior is perceived in commons as compared with anticommons, indicating that both constitute a different frame of reference for social judgments (Van Hiel, Vanneste and De Cremer, 2008). Participants maximize their self-interest in the anticommons framework more rigorously (Dhont et al., 2012), unless negative effects on others (i.e., negative externalities) are made salient. The authors explain the more selfish behavior in the anticommons dilemma by suggesting that the negative externalities on others are less salient in the anticommons than in the commons dilemma.

B. Endowment Effects

If I accept that human behavior differs between both dilemmas, the endowment effect appears to be a likely cause of this difference. As mentioned earlier, the endowment effect refers to the phenomenon that the minimum price at which a person agrees to sell a good (WTA) usually is two to three times the maximum price at which she or he agrees to buy it (WTP; Kahneman, Knetsch, and Thaler, 1991; Kahneman et al., 1990).

Endowment effects are reported for a variety of objects, ranging from simple consumer goods (such as mugs, pens, and chocolate bars; e.g., Brown, 2005; Johnson, Häubl, and Keinan, 2007; Nayakankuppam and Mishra, 2005) to objects with risky or uncertain outcomes, such as lottery tickets (e.g., Ashby, Dickert and Glöckner, 2012; Buccafusco and Sprigman, 2010; Cook and Wu, 2001; Eisenberger and Weber, 1995; Harless, 1989; Inder and O'Brien, 2003; Knetsch and Sinden, 1984; Peters, Slovic, and Gregory, 2003; van de Ven, Zeelenberg and van Dijk, 2005; Van Dijk and Van Knippenberg, 1996) and intellectual property rights (Buccafusco and Sprigman, 2010, 2011; Sprigman, Buccafusco and Burns, 2013). The effect is particularly relevant for patenting as it increases with involvement in the creation of the good that people are about to trade (cf. IKEA effect; Norton, Mochon and Ariely, 2012). Overall, the evidence indicates that the phenomenon is robust, even though there are considerable differences in effect sizes (for an overview, see Horowitz and McConnell, 2002).[70]

Although endowment effects in individual decision making can be considered a well-explored phenomenon, only very little evidence exists on the prevalence and the size of endowment effects in settings of interdependent or even strategic decision making. In an early vignette study, Carnevale (1995) used hypothetical goods to extend the endowment effect to group-owned property. He found that the endowment effect was slightly smaller compared

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[70] Plott and Zeiler (2005, 2007) recently cast doubt on the existence of the endowment effect in a series of papers that aim to debunk the effect as a methodological artifact. Our experimental design considers and avoids their criticism (e.g., I did not present the lottery ticket as a gift to participants, which might increase the valuation of the ticket in the endowment condition; participants decided in individual booths, such that they were not influenced by the decisions of others), and I still find an endowment effect in the control condition.

with individual decision making. By contrast, Galin (2013) analyzed the effect of group negotiations and showed that discussion within the group increased the size of the endowment effect. Similarly, Blumenthal (2012) demonstrated an increased endowment effect after group deliberation. Yet the anticommons situations that I investigate in this paper are very different from such joint group decisions in that sellers do not decide together. Rather than sitting in the same boat, their relationship is strategic, as they can earn more at the expense of the other sellers. The interdependence of outcomes is strategic. Thus, the existing literature does not allow for predictions of the endowment effect in anticommons dilemmas.

III. General Design and Hypotheses

To investigate the effects of the three influencing factors (endowment status, interdependence of outcomes, and incentive to overprice), I manipulate the factors endowment and outcome structure between subjects (Table 1). To manipulate endowment status, half of the participants receive a lottery ticket and can keep or sell it by indicating their sales price (i.e., WTA), whereas the other half is given the opportunity to choose between a lottery ticket and money by indicating the price at which they prefer the money over the ticket (i.e., WTP). From a rational decision-maker perspective, the stated prices for the lottery tickets should be the same in the endowment and the non-endowment condition because the rules for the transaction (summarized in the equations in Table 1) are equivalent.

The second factor (outcome structure) manipulates interdependence of outcomes and incentive to overprice. Specifically, I conditioned trade and payment on (1) the sum of prices from the three persons (summed price); (2) the maximum of the three individual prices (maximum price); or (3) only on the individual's own price (individual price). The first two outcome structures involve interdependent outcomes, whereas the last condition does not. The outcome structure of the summed price condition involved incentives for overpricing, whereas the others do not.

Interdependence of outcomes in the first two conditions is achieved by conditioning the deal on whether the randomly drawn offer price is equal to or larger than the sum (or, in the second condition, the maximum) of stated prices (WTA or WTP) in the group (see equations in Table 1). Outcomes are interdependent because a larger price increases the likelihood of the deal failing, thereby reducing the payoff of other persons in the group. No such effect exists for the individual price condition.

Table 1. General Design

			Endowment	
	Incentive for over-pricing	Interdepend. of out-comes	Yes	No
Summed price	Yes	Yes	Lottery ticket Offer ≥ sum (WTA) = deal ^{S}	Lottery or $ Offer ≥ sum (WTP) = deal^{S}
Maximum price	No	Yes	Lottery ticket Offer ≥ max (WTA) = deal^{O}	Lottery or $ Offer ≥ max (WTP) =deal^{O}
Individual price	No	No	*Lottery ticket Offer ≥ WTA = deal*^{O}	*Lottery or $ Offer ≥ WTP = deal*^{O}

Note: The standard anticommons condition is printed in bold; the standard comparison for investigating endowment effects is marked in italics. “deal” refers to selling the lottery ticket (endowment condition) or receiving the money (no endowment condition). “Lottery ticket offer” and “Lottery or $ offer” refer to randomly drawn offer prices.
^{O} If the deal is concluded, individuals receive their stated price.
^{S} If the deal is concluded, individuals receive the random offer price.

Incentives for overpricing are manipulated in that individuals in the case of a deal are paid either the offer price or the price they stated (see superscripts ^{O} and ^{S} in Table 1). If individuals receive their stated price, they can potentially increase their payoff by strategically indicating a price that goes beyond their valuation for the ticket (thereby reducing the payoff of the buyer and/or the earnings of the other dependent players). This incentive for overpricing is not prevalent in all other conditions in which individuals receive the offer price.

I implement the anticommons situation with multiple sellers in a summed price outcome scheme involving payment of stated prices and endowment with the good (Table 1, bold data). The summed and maximum price conditions allow us to isolate the impact of strategic incentives and the influence of the endowment effect on the anticommons situation. The comparison with the individual price condition allows us, in addition, to investigate the effect of interdependence.

For reasons explained in detail later, I consider two plausible models for the interplay of the three factors endowment, interdependence, and incentives for overpricing: an additive model and an interaction model. The additive model assumes that endowment effects and incentive effects independently increase prices; thus, their effects are additive. According to this additive model, endowment effects of equal size should be observed for all three outcome schemes and one would predict main effects of endowment (H_1endow) and incentive (H_2inc) but not an interaction between both factors (H_3end * inc).

Alternatively, I propose an interaction model assuming that the endowment effect is moderated by both incentives and interdependence. This model predicts an interaction of endowment and incentive to overprice (H_3end * inc) and an additional interaction between endowment and interdependence (H_4end * inter) for the two outcome schemes involving no incentive to overprice. Thus, endowment effects should be strongest for the individual price condition, smaller for the maximum price condition, and smallest for the summed price condition.

A. The Additive Model

The understanding that endowment effects might be the cause of the increased severity of the anticommons dilemma (as compared with the commons) implies that endowment effects and strategic incentives add up in driving overpricing, as captured in the additive model. In both dilemmas, participants have an incentive to improve their own situation at the costs of others: they exaggerate valuations beyond their true preferences in the anticommons, and they overuse the commons. But only in the anticommons framework may endowment effects add to the strategic behavior by inflating pricing even further, as endowed individuals may start to exaggerate their preference from a higher initial level.

Such an additive model can be derived from the standard account of prospect theory, which views endowment effects as caused by the sellers' loss aversion, which tends to increase the valuation of goods when people own them (Thaler, 1980). According to a strict interpretation of this account, the size of endowment effects should be the product of individuals' basic valuation of the ticket and their loss-aversion lambda. The endowment effect should not be influenced by other factors. Assuming that incentives to overprice have a constant effect in that individuals overstate their preference by a constant amount above their true valuation, endowment effect and overpricing should simply add up. If pricing was driven by these two factors only, there should be no effect of interdependence on valuation, and pricing in the individual price and maximum price conditions should not differ.

B. The Interaction Model

Alternatively, there is also good theoretical grounding for an interaction model in which the endowment effect interacts both with the incentives to overprice and the interdependence of outcomes. A straightforward rational choice assumption would predict that endowment effects disappear if participants set the price in a completely strategic manner, such that they ignore their true valuation and increase their WTA and WTP up to the maximum point at which they believe the transaction will still be concluded. In that case, pricing would be determined by their motivation to maximize their personal outcome, given their belief about what prices other participants might set. Strictly rational participants should ignore their initial valuation that is potentially heightened by endowment effects (unless their valuation already exceeds the price they expect could be accepted). The model leads us to the prediction that the endowment effect should likely disappear in the summed price conditions.

Yet pure strategic decision making is not the only mechanism that predicts an interaction model. Recent process-oriented approaches assume that endowment effects can be explained by changes in the focus of attention, information retrieval, or weighting due to differences in status quo (Ashby et al., 2012; Carmon and Ariely, 2000; Johnson et al., 2007; Nayakankuppam and Mishra, 2005). Specifically, it is assumed that sellers focus and/or weigh value-increasing aspects more than value-decreasing aspects in the process of constructing their preferences (and vice versa for buyers). It seems likely that this process of biased focus of attention and information retrieval could be influenced by changes in the decision-making context. Incentives to overprice may draw the attention of participants to the strategic elements of the game rather than to the value-increasing attributes of the status quo. This shift in attention may undermine the endowment effect. Along the same lines, the attentional focus in the conditions in which outcomes are interdependent may be shifted from the status quo and the value-increasing attributes to the payoffs of the other participants who may suffer from negative externalities (Dhont et al., 2012). Following this concept of the endowment effect, one would predict an interaction model with the endowment effect being reduced once outcomes are interdependent (maximum price condition) and even more so when participants are additionally given strategic incentives to overprice (summed price condition).

IV. Methodological Preliminaries

Arguably, one of the reasons for the low number of studies on anticommons dilemmas is that they are notoriously hard to implement in the lab. Previous studies mainly relied on hypothetical scenarios or hypothetical games (e.g., co-owners of an oil well; Dhont et al., 2012; Van Hiel et al., 2008; Vanneste et al., 2006). Given that my study is mainly interested in the effect of systematically varied incentive structures on pricing in the anticommons, such a methodological approach cannot be convincingly applied here. I therefore newly developed a fully incentivized joint selling/buying paradigm that captures the game-theoretic structure of the anticommons dilemma and resembles the patent example I describe in the Introduction. This paradigm basically extends the Becker–DeGroot–Marschak (BDM; Becker, DeGroot and Marschak, 1964) mechanism (I will explain this standard method for investigating endowment effects in detail later) to market transactions in interdependent buying/selling situations.

Although my method can be applied to all kinds of goods, I selected lottery tickets for my studies, as they nicely capture the distinctive uncertainty of payoffs associated with intellectual property rights and their use (Crouch, 2008; Lemley and Shapiro, 2005; Scherer, 2001; Scherer and Harhoff, 2000). Over the last years, intellectual property literature has extensively discussed anticommons dilemmas in patent law. The endowment effect played a prominent role in explanations used for overpricing (Epstein, 2009; Epstein and Kuhlik, 2004; Heller, 1998, 2005; Heller and Eisenberg, 1998; Lemley and Shapiro, 2007; Mattioli, 2012; Merges, 1996, 2004; Nadler and Diamond, 2008; Walsh, Arora, and Cohen, 2003). Experiment 1 involves tickets for a lottery implemented by the experimenter with relatively small payoffs, whereas Experiment 2 analyzes transactions with real lottery tickets from Eurojackpot in which participants could win up to €34 million.

V. Experiment

A. Participants and Experimental Design

One hundred and eighteen individuals (79 women, mean age 24.8 years) recruited from the MPI Decision Lab subject pool using Orsee (Greiner, 2004) participated in the experiment and were each paid a flat fee of €12 (approximately US-$15.70) for their attendance. They could earn an additional amount of €0.25 up to €10 for trading their lottery ticket, or they had a 20% chance to win an additional €25 if they kept their lottery ticket. Twenty-one of the participants gave wrong answers to at least one of the control questions they had been asked and were excluded from my analysis.[71]

I used a 2 (endowment state: yes versus no) × 3 (outcome structure: summed price versus maximum price versus individual price) between-subjects design that varied the endowment state across three levels of outcome conditions as explained above. The experiment was run in sessions of six to 12 individuals. In the two interdependent conditions (i.e., summed price and max-

[71] The number of excluded participants did not differ between conditions, Fisher's exact *p*-value = 0.242. The numbers of the remaining participants for the three outcome schemes and the two endowment conditions were as follows: (1) summed price, 13 no endow/15 endow; (2) maximum price, 15 no endow/16 endow; and (3) individual price, 18 no endow/20 endow. Including all participants in the analysis did not change the qualitative pattern of results for the main analysis (Table 2), except that the Endowment * Interdependence interaction on price is reduced to *p*-value = 0.10. An example for the control questions I used to check the participants understanding is as follows:
Max has indicated his willingness to sell his lottery ticket at €3. Peter and Hans have indicated their willingness to sell their lottery tickets for €2. The experimenter draws an offer price of €8. What happens?

imum price), three participants with the same endowment state were randomly assigned to a group. Anonymity was assured during and after the experiment, and participants were not allowed to communicate at any time.

B. Procedure

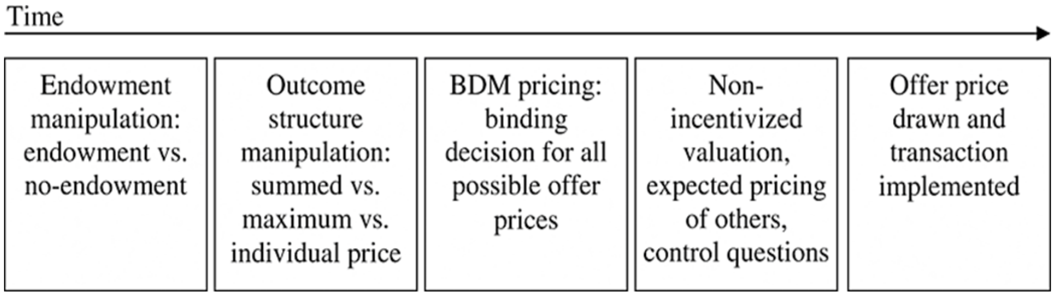
The study was paper-based and Figure 1 gives a brief overview of the general procedure (for the full instructions, see the Appendix). The experiment started with a manipulation of the endowment state in that participants of the endow conditions drew a sealed lottery ticket from a transparent urn, whereas in the non-endow conditions, participants inspected the urn without receiving a ticket. Participants were informed that the urn contained 10 winning tickets worth €25 each, but they were not informed about the number of blanks. This design assured that, on the one hand, participants had a rough estimate of the lottery tickets' value, but, on the other hand, considerable uncertainty remained on the winning probability of their ticket. The design resembles a typical anticommons situation with patents of unknown future value, as described in the Introduction.

Participants were then randomly assigned to one of the three outcome structure conditions. They were provided with instructions that varied the rules of the market for trading the lottery tickets, as illustrated in Table 1. Participants in the endowment conditions indicated the lowest price at which they would sell their lottery ticket (i.e., WTA), whereas, in the no-endowment conditions, they indicated the price at which they would prefer receiving the money over a lottery ticket (i.e., WTP).[72] In all conditions, prices were elicited using modified BDM mechanisms (Becker et al., 1964), in which participants make binding buying/selling decisions and are informed that offer prices are drawn randomly from a pre-specified range of prices. The prices for trading the lottery tickets were allowed to range from €0.25 up to €10, with the distribution of prices within this range being unknown.

[72] I follow the commonly used approach to investigate endowment effects by comparing selling prices (WTA) and prices elicited with a method in which persons chose between money and the good (e.g., Johnson et al., 2007; Lerner, Small and Loewenstein, 2004). In a strict sense, this choosing condition is somewhat different to a situation that involves buying with one's own money, but it does capture the essential property of buying in that persons have to give up money. For simplicity, I refer to this price as a person's WTP. The choosing design allows us strictly to hold our treatments constant when I provide them with incentives to overprice and avoids effects of budget constraints. Lerner et al. (2004, p. 338) summarized the advantages as follows:

A choice price has three advantages over a buying price: (1) It does not require participants to give up money, and hence is not limited by the amount of money participants bring to a study; (2) it confronts participants with a choice that is formally identical to, but framed differently from, selling; and (3) it holds constant the money side of the equation—both selling and choice involve choices between receiving or not receiving money.

Figure 1. Overview of the procedure in Experiment 1



In the summed price conditions, the three participants in each group kept (received) the ticket if the sum of the three indicated selling (buying) prices was higher compared with a randomly drawn total offer price (range: €0.75 to €30). Otherwise, they sold (did not buy) the ticket and received the amount of money they demanded (i.e., their stated price: WTA or WTP). In the maximum price condition, all three selling (buying) prices were compared with a randomly drawn single offer price (range: €0.25 to €10). If all participants indicated a willingness to sell (to prefer money over the ticket) at this price, they all received the same single offer price. Otherwise, they kept (received the tickets. The individual price condition used the same mechanism, except that the decisions were not interdependent; that is, each subject's buying/selling decision was compared with the randomly drawn offer individually without considering the other two subjects' decisions.

Finally, participants completed a post-experimental questionnaire that included the control questions to guarantee their understanding of the procedure and measure several control factors.[73] One further question elicited participants' beliefs of how other group members (or, in the individual condition, other participants) would price the tickets to measure potential differences between own behavior and anticipated behavior of others. At the end of the experiment, the market transactions were really implemented. Participants who kept (or received) a lottery ticket opened it. €25 was added to the payment of the participants who won the lottery. Participants who sold the lottery ticket (or received money instead of the ticket) were paid in accordance with the market rules.

[73] Participants answered a set of questions concerning the transaction, the lottery ticket, and their person. Analyses of these factors did not yield further insights and are therefore omitted.

VI. Results

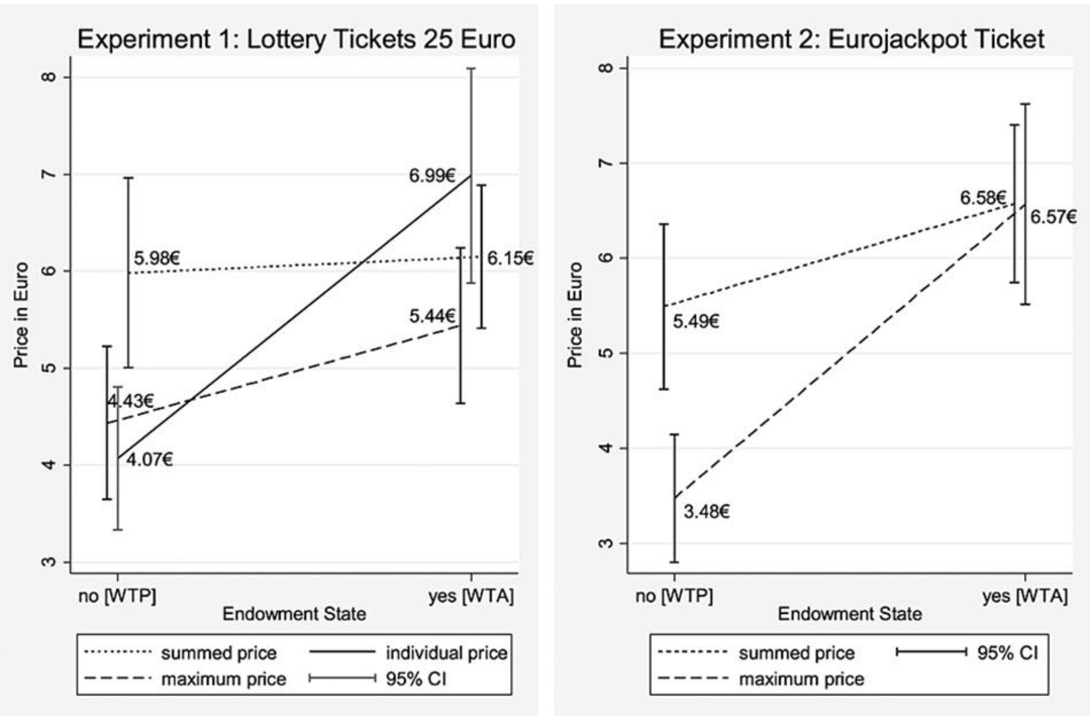
Figure 2 (left-hand side) shows my participants' stated prices (WTA/WTP) for the lottery tickets comparing all six conditions. Overall results fit the interaction models better than the additive model. They are particularly well in line with the predictions of a biased-processing account to endowment effects and less with a purely strategic mechanism.

A. Main Effects: Hypothesis 1-4

In order to test my Hypotheses 1 to 4, I used a general linear model approach and coded my hypotheses in dummy variables to test them as specific contrasts and interaction terms. In particular, price was regressed on endowment state, outcome structure, and their interaction (all variables centered). The three outcome structure conditions were coded in two Helmert contrasts.

The incentive contrast compares the summed price condition that provided an incentive to overprice against the mean of the maximum price and individual price conditions, which both did not involve an incentive to overprice. The interdependence contrast compares the maximum price with the individual price condition, which only differed as outcomes in the maximum price conditions were interdependent.

Figure 2. Mean WTA /WTP Prices in Experiment 1 and 2



Price of lottery tickets by endowment state and outcome structure. The typical anticommons dilemma is summed price and endowment state: yes

In line with H_1endow, I found a significant overall effect of endowment status (Table 2, column 1). Note, however, that the endowment effect disappeared in the summed price condition (i.e., anticommons) and that therefore the size of the overall endowment effect was only €1.41 for an average price level of €5.51 (cf. coefficients for endowment effect and constant in Table 2). I also found a significant effect of incentives on pricing, which supports H_2inc.

Table 2. Regression Model for Lottery Ticket Prices

	Price in Euro (WTA / WTP)	
	Exp. 1: 20€ Lottery	Exp. 2: Eurojackpot
Endowment Status ($H1_{endow}$)	1.41***	2.03***
(yes=1 vs. no=0)	(3.85)	(4.61)
Incentive Effect ($H2_{inc}$)	0.83*	1.07*
(incentive=1 vs. no-incentive & individual=0)	(2.16)	(2.44)
Endowment*Incentive ($H3_{end*inc}$)	-1.85*	-2.01*
	(-2.41)	(-2.27)
Interdependence Effect	0.58	
(no-incentive=1 vs. individual=0)	(1.30)	
Endowment*Interdependence ($H4_{end*inter}$)	1.91*	
	(2.15)	
Constant	5.51***	5.50***
	(30.18)	(25.11)
Observations	97	113
Adjusted R^2	0.218	0.221

* p-value < 0.05; *** p-value < 0.01.

Note: Coefficients are unstandardized bs in Euro. t statistics are provided in parentheses, all variables are centered, and robust standard errors are used. sum, max and ind refer to the conditions summed price, maximum price and individual price.

The third set of contradicting hypotheses H_3end * inc/H_3end * inc concerned the interaction effect of endowment and incentives. In line with the prediction of the interaction model, the endowment effect was significantly smaller in the condition that gave participants incentives to overprice, as compared with the condition that did not. The endowment effect essentially disappeared in the summed price condition.

My last hypothesis concerned the mere effect of interdependence and its impact on the endowment effect. This hypothesis test allows us to discriminate between the purely strategic and the biased-process interaction models. I compare the individual price and the maximum price condition. The results reveal a significant interaction supporting H_4end * inter.

The endowment effect was reduced in the maximum price condition with interdependent payoffs, although the treatment was equivalent to the individual price condition insofar as it was in the participants' best interest in both conditions to reveal their true valuation of the ticket.

To test the robustness of these findings, I analyzed the data using nonparametric tests. The analyses lead to the same conclusion, showing that the data are best explained by an interaction model on the basis of a biased-processing account. Finally, I conducted a 2 × 3 analysis of variance, which also revealed a significant interaction of endowment and outcome structure ($F(2, 91) = 4.92$, p-value <0.01) when testing H_3 and H_4 simultaneously in an overall test.

B. Additional Analysis: Beliefs

For a deeper exploratory analysis of the processes underlying the pricing decisions, I elicited participants' beliefs about other participants' BDM pricing and regressed these beliefs on the full set of manipulated factors presented in Table 2. I observed significantly positive main effects for endowment state, incentive, and interdependence (all p-value <0.05), suggesting that participants anticipated endowment as well as incentive- and interdependence-driven effects. Although these findings could also be explained with a false consensus effect (participants expect others to price in similar ways as they did, as suggested by social projection), I did not find an interaction of endowment and incentive (p-value =0.38). This indicates that participants expected pricing to fit an additive model with particular high pricing in the anticommons situation, whereas the true (BDM) pricing behavior follows an interaction model.

VII. Discussion

The first experiment shows that pricing behavior is influenced not only by endowment status and incentives to overprice but also by the interactions between endowment and incentives and additionally between endowment status and interdependence of outcomes. Overall, the findings provide support for an interaction model and speak against an additive model of pricing in anticommons situations. Endowment effects do not simply add on top of the other effects, worsening anticommons dilemmas compared with a situation in which people do not hold an endowment.

My first experiment was successful in establishing a fully incentivized research paradigm for anticommons dilemmas and provides meaningful results with respect to the models I tested. Still, Experiment 1 has several limitations. First, the sample size was relatively small, and the necessary exclusion of participants (because they had failed to understand the instructions, as revealed by my control questions) might have influenced the data. Second, the usage of an artificial lottery with a relatively low prize for winning (€25) might have limited the size of the endowment effect independent of my treatment manipulations, potentially leading to effects that are unrepresentative of actual anticommons situations. Third, in my modified BDM paradigm, the experimenter acted as the buyer, which might have changed the psychological meaning of the situation, as the payoff and profit of the buyer/experimenter are likely excluded from the subjects' consideration. In an actual anticommons situation, this might be different. I addressed these limitations in a second experiment.

Before running Experiment 2, I conducted a hypothetical pre-study (N =78) to investigate whether endowment effects would indeed be stronger if real lottery tickets were used. Participants were informed about the potential payoffs of their real lottery ticket and the probability that these winnings could occur. They then indicated their WTA or WTP in a between-subjects design. I found a strong endowment effect in that average selling prices were more than 3.7 times higher than average buying prices (WTA = €8.12, WTP = €2.15; six WTAs above €20 were excluded as outliers). As expected, in the individual condition of Experiment 1, this ratio was considerably lower (WTA/ WTP = 1.75). Hence, real lottery tickets that could pay several million induce endowment effects twice as large as the artificial ones with small payoffs of Experiment 1 and were therefore used in Experiment 2.

VIII. Experiment 2

A. Methods

A total of 114 individuals (66 women, mean age 23.1 years; one person did not provide demographic data) were recruited in the same way as for the first study. They were paid a €7 show-up fee plus the outcome of the transaction. Participants were randomly assigned to the cells of a 2 (endowment: yes versus no) × 2 (outcome structure: summed price versus maximum price) design. One person who indicated an invalid price of zero was excluded, leaving us with 113 valid observations. To increase the number of observations per cell, I did not include a further replication of the standard endowment effect (i.e., the individual price condition), as the effect had already been demonstrated in the pre-study.

The procedure was similar to Experiment 1, except for the following important changes (for full instructions, see the Appendix): The experiment involved transactions of lottery tickets from the real lottery Eurojackpot. At the beginning of the experiment, participants were fully informed about the payoffs in the various winning categories of the lottery, as well as about the chances of actually winning the prizes in the different categories.

I also modified my research paradigm in two other important aspects. First, to avoid the problem that participants have to be excluded because they fail to understand the instructions, I rewrote parts of the instructions and improved their readability by including examples. In addition, control questions were provided directly after participants had read the instructions, and participants were only allowed to proceed in the experiment if they had answered all questions correctly. As a minor change, I now informed participants that prices were drawn from a flat distribution such that subjects could (potentially) calculate their expected payoffs.

Second, after the participants had completed the incentivized pricing conditions, I presented them with scenarios that introduced an actual buyer and varied the profit the buyer could make if the deal was realized. In the first scenario, payoffs matched the incentivized conditions, and the buyer was endowed with €30. In the profit scenario, the buyer could resell the lottery tickets to the experimenter for €60. This doubling of profits gave him or her a strong incentive to buy the lottery tickets from the other participants. I also included a survey to learn more about the underlying processes. The survey questions included (among other things) a four-item scale measuring the impact of positive concern about the other players' outcome (i.e., other-regarding preferences or social values) on pricing decisions (Cronbachs $\alpha = 0.72$).[74]

B. Results

The data of the incentivized conditions replicate the findings of Experiment 1. Descriptively, the results show a clear interaction effect between endowment effect and incentive (Figure 2, right): The respective regression analysis provides strong support for H_3end * inc (Table 2, second column), with the magnitude of the effect closely resembling the findings of Experiment 1. The endowment effect was replicated (H_1end). As expected from the results of the pre-study, the effect size was larger compared with Experiment 1, in which I had used artificial lottery tickets with a low payoff. The incentive again led participants to overstate their true preferences, providing support for H_2inc. Interestingly, the incentive effect only reduced but did not completely cancel out the endowment effect in the summed price condition, probably because the endowment effect was so much larger from the beginning, as I can conclude from the pre-study.

Pricing choices in the scenarios were noisy and less predictable by the factors introduced earlier, as indicated by a considerably reduced proportion of explained variance of 4% (12%), as compared with the 22% for the data from the incentivized conditions. Still, the interaction effect was even somewhat stronger in the standard scenario in which the experimenter was replaced by a participant buyer who could potentially benefit from trade ($b = -2.57$, $t = -2.07$, p-value = 0.04). In the high-profit scenario, the effect was still in the expected direction and remained substantial in magnitude (i.e., €1.33), although it did not turn out significant ($b = -1.33$, $t = 0.76$, p-value = 0.43).

Finally, to investigate whether other-regarding preferences reduce endowment effects by interfering with biased processing, I regressed prices on endowment status, the participants' score on the scale measuring their concerns for others (variables centered), and their interaction. Both the main effects and the interaction turned out significant. Prices increased with endowment ($b = 2.30$, $t = 5.66$, p-value < 0.01) and decreased with concerns about the other group members ($b = -0.33$, $t = -4.13$, p-value < 0.01). Most importantly,

[74] Participants indicated agreement or disagreement on an 11-point Likert scale to the items "I feel committed to the other members of my group"; "I have taken potential effects of my pricing decision on other members of my group into account"; "When setting my price, I have tried to avoid negative effects for the other members of my group"; and the reversed item "I did not take into account the other members of my group when determining my price." For explorative purposes, the experiments involved also further items that aimed to measure other social motives (e.g., individualism, competitiveness) as well as further scenarios for individual pricing for which results are not reported.

the interaction showed that, on top of these main effects, endowment effects were particularly reduced for individuals who cared about negative consequences of their decisions for others (b = −0.38, t = −2.39, *p*-value = 0.02).[75]

C. Discussion

Experiment 2 replicates the findings of the first experiment, supporting my hypotheses 1 to 3. It shows that the effects not only hold for artificial lottery tickets, which induce a smaller endowment effect, but also generalize to real lottery tickets with very high payoffs and a very strong endowment effect. Experiment 2 further provides converging evidence on the effect of interdependence formalized in hypothesis 4 by demonstrating a significant effect of concerns about others on endowment effects. The results from the scenarios appear to be noisy and have to be interpreted with caution. They provide initial support that the effects I report hold in scenarios with true buyers or sellers, but I cannot rule out that the results might change in situations in which it is explicitly known that the external buyer or seller can make an excessive profit.

IX. General Discussion

A. Main Results

The fragmented ownership of a bundle of biomedical patents needed to develop a new drug is only one example for the importance of anticommons dilemmas. Heller (1998) described empty storefronts in Moscow to demonstrate how multiple, fragmented ownership can block the efficient use of a resource. Inefficiency and welfare losses for society can result from the fact that individuals have strategic incentives to overprice, leading them to overstate their prices in an effort to maximize their personal gains, whereas it would be in the best interest of society if they revealed their true valuation. Although the anticommons dilemma has been shown to be symmetric, from a game-theoretic perspective on the well-known dilemma of the commons, previous research indicates strong behavioral differences. Anticommons dilemmas lead to more severe overpricing compared with the overuse that is observed in the commons, which increases efficiency losses. One viable hypothesis is that this increased severity is driven by endowment effects (Vanneste et al., 2006). As another factor, the interdependence of outcomes and differences in individuals' awareness of potential negative consequences for others (i.e., negative externalities) has been suggested to contribute to the gap (Dhont et al., 2012).

[75] Concerns for others were not significantly influenced by endowment (b = 0.56, t = 1.21, *p*-value = 0.23) incentives for overpricing (b = 0.75, t = 1.60, *p*-value = 0.11), or their interaction (b = −0.48, t = −0.51, *p*-value = .61). However, I found a trend that persons showed stronger positive concerns for others in conditions with incentives to overprice, as one might have expected.

In two experiments, I aimed at disentangling the relative contribution and the interplay of three psychological sources—namely strategic incentives to overprice, endowment effects, and interdependence of outcomes—on pricing of risky prospects in the anticommons dilemma. A first additive model assumes that endowment effects are driven by loss aversion and that strategic incentives lead to a further additive increase in prices for both endowed and non-endowed individuals. I test the prediction of this additive model against an interaction model, which comes in two implementations. On the one hand, a purely strategic interaction model assumes that individuals ignore their valuation and increase prices up to the level that maximizes their profit (independent of whether they are endowed or not). On the other hand, a biased-processing model states that endowment effects are constructed in a process that gives more weight to value-increasing aspects as compared with value-decreasing aspects for sellers (and vice versa for buyers). According to such a model, the interdependence of outcomes and the social concern about others, as well as the strategic incentives, may interfere with this process, decreasing the size of endowment effects.

My data generally support the interaction model, and the overall pattern of results is particularly well explained by the biased-processing approach. First, besides the main effects for incentive to overprice and endowment effect, I repeatedly find an interaction between the two factors, in that the endowment effect observed in individual decision making disappears (Experiment 1) or is largely reduced (Experiment 2) in the conditions that give participants an incentive to overprice. This finding speaks against the conjecture that the increased inefficiency of anticommons, as compared with a dilemma of the commons, is directly caused by endowment effects (Vanneste et al., 2006).

Second, I find in Experiment 1 that the interdependence of outcomes already affects pricing. Specifically, endowment effects are significantly reduced when outcomes are interdependent even when no incentives to overprice are provided, as compared with an individual buying/selling setup (also without incentives to overprice). Further, I find in Experiment 2 that endowment effects decrease with positive concerns about the outcome of other people. These results support the hypothesis that externalities (and participants' awareness of them) play a crucial role in pricing in the anticommons (Dhont et al., 2012) and that subjects' consideration of these externalities interferes with the process of biased value construction, which causes the endowment effect.

The interaction effects I observe clearly contradict an additive model. The finding that endowment effects decrease if interdependence is introduced and are further reduced or even eliminated when incentives to overprice are provided is in line with predictions of a biased-processing approach, which assumes interdependence of outcomes and incentives as factors interfering with the value construction process. This gradual effect as well as the tendency that prices in the conditions with incentives to overprice are below prices in the individual-endow condition can hardly be explained with purely strategic pricing behavior.

My findings have implications for the general research on the endowment effect. They show that the endowment effect is not as independent of contextual factors as the standard loss-aversion account implies (see also Ashby et al., 2012; Birnbaum and Zimmermann, 1998; Johnson et al., 2007; Johnson and Busemeyer, 2005). Endowment effects are reduced by interdependence of outcomes with potential negative consequences for others, which goes beyond mere main effects of social values and social preferences (e.g., McClintock, 1972; Murphy, Ackerman and Handgraaf, 2011; Van Lange, 1999). Strategic incentives to overprice drastically reduce or eliminate the effect.

Furthermore, from a methodological point of view, I show that the modified BDM paradigm I developed is a valuable instrument to investigate behavior in the anticommons using real trades, full incentivization, and no deception, rather than hypotheticals.

B. Limitations

The reported studies only investigate endowment effects for uncertain prospects. Extrapolating my results to physical goods is not trivial, as I cannot rule out that behavior may depend on the nature of the good that is traded. Such an influence is plausible as some driving forces of the endowment effect (e.g., anticipated regret versus affective attachment) may differ between uncertain (i.e., lottery tickets and patents) and physical goods (i.e., coffee mugs and real estate). The fact that the endowment effect was not completely eliminated for a real lottery ticket as compared with an “artificial” one might point in this direction.

A second limitation concerns my methodological approach to elicit WTP by making individuals choose between money and the lottery ticket instead of asking them to buy the ticket from their own money, to avoid effects of budget constraints and difference in wealth levels between conditions (see footnote 3 for details). Although I see no reason how this methodological choice could have influenced my results, future studies might be conducted to test whether results are different in situations in which individuals buy goods from the money they bring to the lab.

X. Conclusions and Policy Implications

Efficient transactions in anticommons dilemmas are highly important for society. Anticommons situations can reduce the number of successful transactions and lead to market breakdowns. In this paper, I provide results that allow for a more differentiated view on the distinguished behavioral factors that influence the pricing of risky prospects in an anticommons dilemma. I show that behavioral effects are not independent and additive but that incentives and the interdependence of outcomes strongly interact with endowment effects. This result has various policy implications. First, effective regulation of anticommons dilemmas is likely intricate: Although strategic incentives lead to overpricing, they also reduce or even drive out the endowment effect. Thus, the anticipation of takings or other forms of government intervention may reduce incentives to overprice but at the same time may reflate the endowment effect, bringing the trade inefficiencies back, which the regulation wanted to address—only for a different reason. However, how strongly this effect plays out will also depend on other factors, such as the institutions that the parties use for their negotiations.[76] A sophisticated social planner should take behavioral reasons for overpricing into account when designing government intervention, in order to avoid unintended interaction effects. Some studies have questioned whether the anticommons dilemma is a real problem in the innovation sector. These studies argued that market participants may anticipate and prevent anticommons dilemmas by cross-licensing their respective patents, thereby decreasing the risk that partners can block their projects (Chang, 2012; Epstein, 2009; Epstein and Kuhlik, 2004; Merges, 2004; Walsh et al., 2003).

Other empirical and theoretical studies, however, do not share this positive view that private parties will easily find effective solutions for the anticommons dilemma (Heller and Eisenberg, 1998; Mattioli, 2012; Murray and Stern, 2007). They point out that private solutions do not always exist because it will often be more profitable for individual market participants to defect and not accept such agreements, in order to obtain a better deal later when they can threaten to enforce their property rights. Even if parties manage to find agreements, these agreements may not be optimal from a social perspective, as the private parties can be expected to increase prices above competitive levels. Optimal results should only be possible if both parties are equally interested in the patents of the other side, a symmetry that does not always exist. Finally, if market participants are willing to cooperate to prevent the dilemma from realizing, the level of transactions may still not be optimal: Although my study shows that the anticommons situation effectively mutes the endowment

[76] Institutions can debias entitlement holders by allowing them to share decision-making responsibility, thereby reducing the regret people experience over trading and their endowment effect (Arlen and Tontrup, 2015a).

effect, ex ante contract negotiations may—depending on the institutional context in which the decisions are taken—suffer from the bias.

Third, my results suggest, beyond the anticommons dilemma, that although lawyers and economists discuss the endowment effect as a threat to efficient trading (cf. Coase, 1960), it may play a less pronounced role in business interactions than is often assumed. If the effect is caused by a biased processing of information and focus of attention and is thus responsive to elements of the decision-making context such as strategic incentives and interdependency of outcomes, the effect might be less robust than previously thought outside the fully controlled lab environment. Predictions for complex (e.g., real life) settings should be derived with great care and preferably involve testing in the respective domain. The two interaction effects I describe will be present in many more social contexts than anticommons situations alone. My study suggests that it is an important avenue for further research to explore whether it generally holds that the endowment effect is less important in various business domains of strategic decision making.

Chapter 4 – Self-Debiasing

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Abstract

I claim that the endowment effect rarely justifies legal intervention in private ordering. I present the first theory, to my knowledge, to explain how institutions inhibit the endowment effect without altering people's rights to their entitlements. The endowment effect is substantially caused by anticipated regret. I show that people experience regret only when they feel responsible for the decision and can mute regret by trading through institutions that let them share responsibility with others. As entitlement holders typically transact through institutions, I expect most people to make unbiased trading decisions in real markets. I test two common institutions—agency relationships and voting—that divide responsibility between multiple actors. Each caused most subjects to debias and trade in my study. I also show that people intentionally debias by employing institutions in order to share responsibility. Thus, when people can freely transact, private ordering generally overcomes the endowment effect.

I. Introduction

Legal scholars have long argued that the endowment effect requires legal intervention in private ordering. According to the Coase theorem, law need not interfere with private ordering because people will transact until entitlements flow to those who value them most, when there are no transaction costs or other impediments to contracting (Coase, 1960). Endowment effect experiments contradict this claim. They show that people's willingness to accept parting with an entitlement exceeds their willingness to pay (WTP) to obtain it. Consequently, entitlements tend to remain with their original owners, even when others would value ownership more (see, for example, Thaler, 1980; Knetsch and Sinden, 1984; Knetsch, 1989; Tversky and Kahneman, 1991; Kahneman, Knetsch, and Thaler, 1991).

Relying on this evidence, legal scholars claim, in more than a thousand articles, that the endowment effect leads to suboptimal allocation of important entitlements, including intellectual property, contractual default rules, real property, legal settlements, corporate control, consumer debt, employment, and environmental protection. They advocate interventions to reallocate entitlements, alter contractual default rules, or weaken people's sense of endowment by replacing property rules with liability rules or bright-line rules with standards (for example, Sunstein, 1986; Coates and Subramanian, 2000; Jolls, Sunstein, and Thaler, 1998; Korobkin, 1998; McCaffery, Kahneman, and Spitzer, 1995; Rachlinski and Jourden, 1998; Buccafusco and Sprigman, 2011; for more than 1,600 legal articles citing the endowment effect, cf. Korobkin, 2014).

I claim that legal intervention to address the endowment effect is rarely needed. I present the first theory, to my knowledge, to show that common institutions, such as agency relationships and voting, debias the endowment effect without interfering with private ordering.[77] I show that the endowment effect roots in the responsibility entitlement holders have for the decision to trade. Institutions debias when they allow sellers to share responsibility for the decision with others.

My theoretical claim that decision-making responsibility is a prerequisite of the endowment effect builds on the regret account of the endowment effect (for example, Knetsch and Sinden, 1984; Baron and Ritov, 1994; Bar-Hillel and Neter, 1996; for a discussion of the evidence, see Korobkin, 2014).[78] Under regret theory, people resist parting with their entitlements because they anticipate that they may regret the decision to trade and anticipate more regret over trading than over failing to obtain an entitlement in error (Loomes and Sugden, 1982). The disutility of anticipated regret causes owners to reject transactions or insist on additional compensation above the value they place on the asset alone. Anticipated regret can cause an endowment effect across all forms of entitlements. People anticipate regret over parting with an entitlement whose future value is uncertain—either in the market or to the owner personally (Plott, 1996)—because the trade my produce a loss once the future outcome is known. As the future value of most entitlements is uncertain, anticipated regret can induce an endowment effect in a wide range of entitlements. This includes real, intellectual, and personal property; corporate control; legal settlements; securities; and material contract clauses. Regret theory also can explain the endowment effect for simple consumer goods, such as mugs and

[77] Compare with Arlen, Spitzer, and Talley (2002). Under the regret account, the endowment effect can be understood as a motivational bias. People can cognitively identify the rational choice based on standard preferences given the value they attach to the entitlement but are deterred from trading by the emotional cost of making a wrong trading decision. Throughout this article, I use the term "unbiased decisions" to refer to decisions that are unaffected by the endowment effect. I am aware that other biases may be present.

[78] I recognize that the endowment effect also may be caused by loss aversion or attachment (see generally Korobkin 2014). I discuss the interaction of institutions and other sources of the endowment effect in Section V. C.

pens, whose value is known and stable. People experience disutility from anticipated regret when an exchange at their personal valuation would be a bad deal as judged by the market. Consequently, owners who place a lower value on a good than does the market may require more than their actual valuation to trade, even when they cannot sell the entitlement as a result (Weaver and Frederick, 2012).

The literature shows that responsibility is a necessary prerequisite to regret (see, for example, Zeelenberg, van Dijk, and Manstead, 1998). Building on this, I claim that the anticipated regret that causes the endowment effect requires more than endowment alone. People should anticipate regret over losses from trading and exhibit the endowment effect only when they feel responsible for making the decision to trade.

My theory suggests that institutions systematically debias entitlement holders. Institutions, such as agency relationships and voting, involve others in the decision to sell; this divides responsibility for the transaction among multiple actors. Since sharing responsibility mutes regret, people transacting through these institutions should not experience the regret that causes the endowment effect. My theory also reveals that entitlement holders have an incentive to use institutions to debias themselves.

I selected two institutions to test my theory: principal-agent relationships and voting. Agency relationships distribute decision-making responsibility between the principal and agent, which limits the individual responsibility of each. The agent decides and often executes the transaction, and the principal provides instructions, retains veto power, or both. Voting divides the responsibility for the transaction among all voters.

I tested my theory in the laboratory and online. In the basic setting, each subject obtained one of two lottery tickets. Each ticket had a 50% chance of winning. Winners earned a substantial payoff. Each subject was offered the opportunity to exchange his ticket for the other ticket plus a monetary bonus of €0.25. Trading to obtain the bonus is the rational decision. Participants who keep their tickets exhibit an endowment effect. Consistent with prior evidence, more than 70% of the laboratory subjects exhibited an endowment effect (see, for example, Knetsch and Sinden, 1984; Bar-Hillel and Neter, 1996; Isoni, Loomes, and Sugden 2011; Korobkin, 2014).

In my first agency treatment, I assigned each subject an agent who made the initial trading decision, which the subject could accept or veto. The subject shares responsibility with his agent and thus should be debiased. In support of my theory, almost 70% of subjects traded their tickets in the lab, and almost 78% did so online. In a second treatment, each subject was assigned an agent. The agent decided whether to trade the principal's ticket, but the principal could determine whether the agent was incentivized to keep or trade the ticket. In support of my theory, 75.3% of the principals incentivized their agents to trade.

In my voting treatments, subjects determined by majority vote whether all tickets should be traded. In one treatment, subjects were bound by the majority's decision; in the other, each participant could veto the decision for his ticket. Almost 80% of the participants in the former treatment and 85% in the latter voted to trade. Even when participants had a veto, more than 85% traded. In addition, I obtained evidence on responsibility and regret. Subjects reported feeling significantly less responsibility for, and regret over, a negative outcome when trading through agents or voting than when deciding alone, which supports my theory.

In a second set of experiments, I tested my claim that entitlement holders will use institutions to overcome the endowment effect. Legal scholars tend to assume that owners do not self-debias when calling for external intervention. My theory reveals that entitlement holders are motivated to self-debias. They delegate to institutions that distribute responsibility in order to relieve their disutility of regret. To test my theory, I offered subjects the option to employ an institution instead of deciding on their own. In the first treatment, each subject could incur a cost to delegate to an agent. In the second, participants could delegate to a majority vote. I found that approximately half of the subjects delegated, which supports my self-debiasing claim. Participants who delegated reported less anticipated regret and overwhelmingly chose to trade. Providing voluntary access to institutions significantly increased trading and had a debiasing effect similar to mandatory institutions.

My theory and findings have important implications for legal policy. They reveal that the endowment effect seldom justifies legal intervention. People should rarely exhibit the bias because they normally transact through institutions that distribute responsibility. Businesses transact through agents, voting, or both. Individuals selling real property, intellectual property, legal claims, and corporate control typically transact through agents. Institutions are omnipresent because they provide many benefits. In most cases, institutions are used for reasons other than debiasing, including expertise and reduced transaction costs. Some are even mandated by law: corporate shareholders and directors must decide by voting. People also employ institutions solely to self-debias, as I have shown. Regardless of why they are used, institutions that divide responsibility debias. Thus, I expect that in real markets transacting will rarely be affected by the endowment effect. Debiasing is even costless when people would employ the institution irrespective of any benefit from debiasing. I conclude that private ordering will in most cases overcome the bias, which leaves little need for legal intervention. Therefore, I propose a presumption against intervention: unless evidence shows that entitlement holders in a market are not efficiently debiased by available institutions, intervention to address the endowment effect should be considered unnecessary.

This article proceeds as follows. Section 2 presents my basic experimental design. Section 3 presents my test of the debiasing effect of agents and voting. Section 4 tests whether people voluntarily use institutions purely to debias. Section 5 discusses the internal and external validity of my experiment and its implications for legal policy. Section 6 concludes.

II. Methods: Basic Experimental Design and Procedures

A. Base Condition

I conducted my study in the laboratory and online. The basic design was identical. Each subject was endowed with a lottery ticket marked either "heads" or "tails," representing a 50% chance of winning the lottery. A subject won €8 (about $11) in the laboratory or €4 in the online study if the ticket he held at the end of the session matched the outcome of the lottery; otherwise, he earned nothing. A subject could trade his ticket for a ticket with the alternative symbol (heads or tails) plus a bonus of €0.25. Each ticket had the same expected value; thus, expected earnings were higher if the subject traded. Since both tickets had an equal probability of winning the same payoff, neither risk aversion nor uncertainty about the true value of the goods exchanged can confound my results. My lottery design provides a salient rational benchmark and identifies who is biased: a rational subject with standard preferences should trade, and any subject who retains the ticket exhibits an endowment effect. I use this basic setup as my benchmark treatment Base, against which I test the debiasing effect of institutions both in the laboratory and online.[79]

This type of study is well tested. Many foundational experiments on the endowment effect use lottery designs, and participants exhibit a strong bias (see, for example, Knetsch and Sinden 1984; Marshall, Knetsch, and Sinden 1986; Bar-Hillel and Neter 1996).[80]

B. Laboratory Experiment

I conducted the study in a laboratory at the University of Jena. I had 210 participants across treatments. Participants studied a variety of disciplines; I also

[79] The full set of instructions are available in the appendix.

[80] Isoni, Loomes, and Sugden (2011) find an endowment effect following the experimental protocols of Plott and Zeiler (2005, 2007). In their response, Plott and Zeiler (2011) point out that their original articles made statements only for experiments with consumer goods, so the use of a lottery design in Isoni, Loomes, and Sugden does not refute their analysis. The potential misconceptions that Plott and Zeiler suggest might be present in Isoni, Loomes, and Sugden's experiment do not apply to the design I implement, as I explain in Section III.

had nonstudents in the sample. In my post-experiment questionnaire, I obtained demographic variables, gender, and age (18–41 years; mean of 23.4), which were balanced between treatments. I also elicited information such as the subjects' disciplines and whether they worked outside the university. Regression results show that demographic characteristics do not affect my findings.

Subjects were seated in separate booths with no ability to observe or hear each other and received the instructions in writing. Each participant withdrew a sealed ticket from a box containing many tickets. For each subject, a coin was tossed and covered before the subject made any decisions. The instructions clarified that subjects either could accept the unknown outcome or toss the coin themselves after making their decision whether to trade.

After reading the instructions, participants were asked control questions. Participants had to calculate their earnings, assuming that they traded or kept their ticket and won or lost the lottery. They had to answer these questions correctly in order to proceed. Misunderstandings appeared in only one of 10 times. I had to exclude three of the 210 participants.

Subjects had to make all choices explicitly. Thus, when asked whether they wanted to trade, they had to write "Yes" or "No," which imposed the same transaction costs no matter whether they decided to trade or keep their tickets. Subjects who decided to trade turned in their original tickets for the alternative tickets and were paid in cash.

C. Online Experiment

Subjects for the online experiment were students from the University of Munster who were invited to participate by e-mail through the university's server. I had 603 online subjects across all treatments. The dropout rate was less than 10%, probably because subjects were paid only if they completed the study, which took about 8 minutes. To ensure that subjects participated only once, each invitation contained a personal key that became invalid once a subject used it to enter the experiment. Participants received their payment via direct electronic bank transfer or PayPal after completing the study. They had various disciplinary backgrounds, genders were balanced, and few had participated in experiments before. I controlled for demographics in logistic regressions; gender, age, and discipline do not significantly affect the results.

The online experiment largely replicated the laboratory treatments, except that a winning lottery ticket paid €4 instead of €8. I kept the bonus for trading at €0.25. Subjects did not receive physical lottery tickets but instead were told that they had been randomly assigned tickets through a code hidden in the instructions they received for the experiment. The code would identify whether they started with a heads or tails ticket and would be revealed only after the session was over. Subjects determined whether heads or tails won the lottery. At the end of the session, they learned that the e-mail message inviting them to the experiment contained the code stating the type of ticket they were assigned. Thus, subjects could be sure that the outcome of the lottery was determined by chance. The frequency of wins and losses was indeed consistent with chance. I asked the same control questions as in the laboratory experiment.

In addition to measuring trading frequency, I also elicited each subject's sense of responsibility and regret over a negative outcome of the lottery following a decision to trade or keep his ticket. I used a 10-point Likert scale ranging from 1 (very little responsibility and regret) to 10 (very strong responsibility and regret) for this item.

III. Debiasing by Institutions

I claim that entitlement holders anticipate regret and exhibit the endowment effect only when they feel responsible for the decision to trade. Many institutions divide decision-making and outcome responsibility among multiple actors instead of focusing it entirely on the entitlement holder. People operating within such institutions should anticipate substantially less regret over a trade since they share decision-making responsibility with others. As a result, institutions should enable them to make unbiased trading decisions.

In this study I test two institutions commonly used for trading that distribute responsibility between actors: principal-agent relationships and voting. The principal and agent both causally contribute to the transaction. The agent often makes the initial trading decision and executes the transaction. The principal provides ex ante instructions and may retain a veto. Although the division of authority between the principal and agent varies, generally both principals and agents share the responsibility for the decision. The debiasing effect of the institution should enable principals to make unbiased decisions when providing instructions or incentives to their agents ex ante. Principals presented with an agent's optimal recommendation to trade should not be biased against trading and should allow completion of the trade.

The second institution I analyze is voting, which is used in many business contexts, for example by co-owners, boards of directors, and shareholders. Voting divides decision-making responsibility among the voters. A single voter bears responsibility only when he is pivotal. Increasing the number of voters reduces individual responsibility by reducing the likelihood that any particular voter is decisive. Ex ante, all voters share equal responsibility for the outcome. Thus, no matter how they vote, their vote is not biased by anticipated regret. The debiasing effect of majority voting without veto is particularly strong: a majority vote to trade produces an unbiased outcome for all, even if the minority manifests an endowment effect and votes against the trade.

A. Principal-Agent Relationships: Experimental Design

In the *Mandatory* treatment, each principal was instructed that he had been assigned an agent who would decide whether his ticket would be traded for the alternative ticket plus a bonus of €0.25. Agents were real participants who made their choices in the laboratory. Each subject was informed that the agent's Decision would be binding unless the subject vetoed it. A principal who vetoed the agent's choice could decide for himself whether to trade or keep his original ticket. Each principal was instructed that the agent would receive €2 from the experimenter if, but only if, the agent decided to trade the principal's ticket.[81] To rule out other-regarding motivations, principals were informed that their veto decisions would not affect the agents' payoffs and that agents would not learn about their veto decisions. Agents did not share in the outcome of the lottery. Control questions confirmed that subjects understood what the design made salient: the agent could not have better information on the outcome of the lottery than the principal.

In my second agency treatment, *Guided Agent*, each principal was assigned an agent who decided whether the ticket would be traded, but the principal could incentivize the agent's choices. If the principal incentivized trading, then the agent received €2 if he exchanged the principal's ticket and nothing if he rejected the trade. If the principal incentivized the agent to keep the ticket for him, the agent earned €2 if he decided not to trade and nothing if he traded. The agent was incentivized but not bound. Payments were made by the experimenter. A principal could not veto the agent's decision.

To rule out potential confounds, I conducted two control treatments: *Default* and *Information-Only*. The *Default* treatment is identical to the Base treatment, except that subjects were informed that their tickets would be traded automatically unless they vetoed the exchange. The *Default* treatment controls for two alternative explanations of my results in *Mandatory*: a shift of the status quo and omission bias (see Baron and Ritov, 1994). In the *Base* treatment, being entitled to the ticket is the clear status quo. By contrast, in the *Mandatory* treatment, the agent trades the ticket unless the subject vetoes. This may weaken the principal's sense of endowment or even shift the status quo entirely. In the *Guided Agent* treatment, this confound is unlikely because the principal decides whether the agent receives an incentive to trade or keep the

[81] Each agent was assigned to six principals and received €2 for each ticket he traded. Thus, agents could make up to €12 by trading all tickets of their six principals. Agents completed the experiment in separate sessions in advance of the principals' sessions. They were assigned ex post to their principals. Principals were not aware that a single agent was assigned to several principals. As expected, agents followed their strong incentives, and all but one decided to trade.

ticket. Still, the treatment could change the status quo should the principal believe that the agent will trade no matter what the incentives are. A shift in subjects' perception of their endowment status could increase trading independent of responsibility sharing (see Köszegi and Rabin, 2006).

The *Default* treatment also rules out omission bias as an alternative explanation for debiasing in the *Mandatory* treatment. The principal can trade by inaction in the *Mandatory* treatment, while in the Base treatment he has to actively trade his ticket. People generally experience less responsibility for and regret over omissions (see, for example, Ritov and Baron, 1992; Baron and Ritov, 1994). Thus, the switch from action to inaction could reduce regret and the endowment effect. Omission bias is not a concern in the *Guided Agent* treatment, as the principal only takes action.

The *Information-Only* treatment controls for whether subjects follow agents assuming they have superior information or expertise. Participants made the trading decision on their own, as in the Base treatment, except that prior to making their choice they were informed about the trading decision of an agent acting on behalf of a different principal. Thus, the *Information-Only* treatment disentangles the potential effect of an agent's recommendation from responsibility sharing with the agent.

My main observation is the frequency of trades. In addition, I asked principals in the online study to indicate how responsible they would feel, and how much regret they would expect to experience, over a negative outcome following both a decision to trade and a decision to keep their tickets. In the agency treatments, I also elicited how much responsibility principals attributed to their agents for a negative outcome.

B. Behavioral Predictions

The regret theory of the endowment effect predicts that subjects do not trade because they anticipate that they will experience regret should they lose the lottery because they exchanged their tickets. They keep their tickets to avoid the disutility of anticipated regret. Thus, even though trading is the rational choice, I expect a significant number of subjects (tested against the rational choice prediction of trading) to keep their tickets and exhibit a bias in the *Base* treatment (Hypothesis 1).

By contrast, in my agency treatments, the principal-agent relationship should mute the regret that triggers the endowment effect. I argue that regret presupposes responsibility (see Zeelenberg, van Dijk, and Manstead, 1998). People experience regret over losses caused by a decision for which they feel responsible. The agency treatments divide the responsibility for the decision to trade between the principal and his agent. In the *Mandatory* treatment, the agent makes the initial decision and the principal decides whether to veto it. In the *Guided Agent* treatment, the principal provides the agent with incentives and the agent decides whether to trade. Evidence shows that people rank the responsibility for an outcome according to contributions. They attribute the greatest responsibility to the last affirmative action in a causal chain because it is closest to the outcome, even when followed by a subsequent inaction (see Spellman, 1997). In both treatments, agents take the last affirmative action, even though in the *Guided Agent* treatment the principal strongly influences the agent's decision. Thus, principals share the responsibility for the trading decision with their agents and should anticipate less regret. I predict that more subjects in the agency treatments will trade their tickets, and thus not exhibit an endowment effect, than in *Base* (Hypothesis 2).

I claim that the principal-agent relationship debiases because it divides the responsibility for the trade between the principal and his agent. If responsibility is indeed shared, as I hypothesize, principals should report that they anticipate less responsibility and regret over trading in the *Mandatory* and *Guided Agent* treatments than in the *Base* treatment. In addition, principals should indicate that they attribute some responsibility for the trade to their agents (Hypothesis 3).

C. Results

Table 1 presents the summary statistics for all laboratory treatments, while Table 2 shows the summary statistics for the online experiment. Regression results that control for demographic variables (gender, discipline, work experience outside of the university) do not deviate from the nonparametric tests I present here.

Table 1. Laboratory Experiment: Summary Statistics

	Total N	Keep	Trade	Fisher 2-tailed
Base	64	45 (70.3%)	19 (29.7%)	
Information-Only	39	28 (71.8%)	11 (28.2%)	p=1
Mandatory	45	14 (31.1%)	31 (68.9%)	p<0.01** p<0.01**
Optional	59	29 (49.1%)	30 (50.9%)	p=0.04* p=0.03*
Optional: Delegate	29 (49.2%)	3 (10.3%)	26 (89.7%)	p<0.01** p<0.01**
Optional: Not Delegate	30 (50.8%)	26 (86.6%)	4 (13.4%)	p=0.12 p=0.15

I report two p-values: (1) Treat vs Base; (2) Treat vs Information-Only

Hypothesis 1. *In the Base condition, subjects exhibit an endowment effect*

As can be seen in Tables 1 and 2, in the *Base* condition 70.3% of the laboratory subjects and 44.4% of the online subjects do not trade their tickets. The results are significantly different (Fisher Exact test *p*-value <0.01) from the rational choice prediction that all participants should trade.[82] This strong evidence of an endowment effect is consistent with the existing literature (for example, Knetsch, 1989; Bar-Hillel and Neter, 1996; Isoni, Loomes, and Sugden, 2011). I likely observed more trading online than in the lab because the stakes in the lottery were lower and subjects did not have physical possession of their lottery tickets. Both should have reduced the intensity of regret participants experienced. Supporting the theory that regret causes the endowment effect, I find that subjects in the online *Base* condition anticipate significantly more regret if they trade than if they keep (7.2 versus 6.5; Mann-Whitney *p*-value =0.02), as shown in Table 3.[83]

Hypothesis 2. *Agency increases trading*

In support of my theory that responsibility sharing mutes the endowment effect, I find that subjects in the *Mandatory* treatment are significantly more willing to trade than those in the *Base* treatment. In the lab, 68.9% of the *Mandatory* subjects trade, compared with 29.7% of the participants in the *Base* treatment. In the online experiments, 77.8% of the *Mandatory* participants trade, whereas only 55.6% exchanged their tickets in the *Base* treatment, as presented in Table 2. In the *Guided Agent* treatment, 75.3% of the principals incentivize their agents to trade their tickets. Thus, significantly more principals wanted to trade through agents than in the *Base* treatment.

My two agency treatments differ: in the *Guided Agent* treatment the principal decides before and independent of his agent, while in the *Mandatory* treatment the principal has a veto and decides in response to the agent's initial choice. The veto in the *Mandatory* treatment could be a source of confounds. My two control treatments address these potential confounds. The first control condition, *Default*, shows that the debiasing results are not driven by either a shift in participants' reference points or by omission bias. In the *Default* condition, each subject obtains a ticket that the computer will trade automatically unless he vetoes. He cannot share responsibility for his decision because there is no other player. If debiasing is caused by sharing responsibility in the principal-agent relationship as I claim, then fewer participants should trade in the

[82] Note that if I treat the experience of regret as a psychological cost, I can reconstruct the endowment effect in rational choice terms (see Loomes and Sugden, 1982).

[83] In the laboratory, I explored the subjects' motivations by asking them to explain their decision in a free-form format. The majority of subjects who did not trade indicated that they could not improve the probability of winning by trading and preferred not to intervene. Others said more directly that they would feel bad about causing a loss in the lottery.

Default than in the *Mandatory* condition. By contrast, if I observe more trading in the *Default* than in the *Base* treatment, then omission bias or a shift in status quo could have caused my results. I find that significantly more participants trade in the *Mandatory* condition (77.8%) than in the *Default* condition (61.1%), as shown in Table 2. The values in the *Base* and *Default* treatments do not differ significantly. Therefore, neither of the two potential confounds explains my results in *Mandatory*. In the *Guided Agent* condition, principals take action; thus, omission bias cannot affect results. A shift of reference point also is unlikely since the principal influences his agent's decision by setting his incentives; with the *Default* treatment I can rule out this potential confound entirely.

My second control treatment rules out information as an alternative explanation of my results in the *Mandatory* condition. If principals trade more often in the *Mandatory* condition because they assume that the agent's choice revealed valuable information, I should observe an increase of trading in the *Information-Only* treatment. Yet, as shown in Table 1, the frequency of trades in the *Information-Only* treatment (28.2%) is significantly lower than in the *Mandatory* treatment (68.9%) and not statistically different from that in the *Base* treatment. As principals in the *Guided Agent* treatment select incentives ex ante, an information confound is impossible.

Hypothesis 3. *Agency reduces reported responsibility and regret.*

I claim that sharing responsibility debiases by reducing regret over trading. As shown in Table 3, when I compare reported responsibility and regret across treatments, I find that subjects indeed experience significantly less responsibility for and anticipate less regret over trading in the *Mandatory* treatment (4.72 and 6.12, respectively), where subjects can share responsibility, than in the *Base* treatment (6.35 and 7.2). The level of responsibility and regret is also significantly lower in the *Mandatory* treatment than in the *Default* treatment (6.17 and 6.66). I find the same pattern in the *Guided Agent* treatment. Subjects experience significantly less responsibility for (5.64) and regret over (6.24) a decision to trade than in the *Base* treatment.

Table 2. Online Experiment: Summary Statistics

	Total N	Keep	Trade	Fisher 2-tailed
Base	90	40 (44.4%)	50 (55.5%)	
Default	90	35 (38.9%)	55 (61.1%)	p=0.54
Mandatory	81	18 (22.2%)	63 (77.8%)	p<0.01** p=0.03*
Guided Agent	81	20 (24.7%)	61 (75.3%)	p<0.01**
Optional	82	23 (28%)	59 (72%)	p= 0.02*
No Agent	82	38 (46.3%)	44 (53.6%)	p=0.87
Optional – Delegate	43 (52.4%)	6 (13.9%)	37 (86.1%)	p<0.01**
Optional – Not Delegate	39 (47.6%)	17 (43.5%)	22 (56.5%)	p=1
No Agent: Keep Optional: Trade or Keep	38	21 (55.3%)	17 (44.7%)	
Voting without Veto	91	19 (20.9%)	72 (79.1%)	p<0.01** p=0.01*
Voting without Veto (Group of Three)	48	6 (12.5%)	42 (87.5%)	p<0.01** p<0.01**
Voting with Veto (veto decisions)	88	10 (11.4%)	78 (88.6%)	p<0.01** p<0.01**
Voting with Veto (submitted votes)	88	13 (14.7%)	75 (85.3%)	p<0.01** p<0.01**
Optional Voting with Veto	90	22 (24.4%)	68 (75.6%)	p<0.01**
Optional Voting – Delegate	41 (45.5%)	3 (7.3%)	38 (92.7%)	p<0.01**
Optional Voting – Not Delegate	59 (54.5%)	19 (38.7%)	30 (61.2%)	p=0.07+
Base: Keep Optional: Trade or Keep	37 (41.1%)	16 (43.3%)	21 (56.7%)	

I report two p-values: (1) Treatment vs Base; (2) Treatment vs Default. I only report comparisons to Default if the treatment could have changed the status quo.

The principal-agent relationship also limits individual responsibility and regret when I compare results within subjects. In the *Base* and *Default* treatments, participants report feeling significantly more responsibility and regret when assuming that they trade (6.35 and 7.2, respectively) compared with when they assume that they keep their tickets (5.48 and 6.5). By contrast, in the *Mandatory* treatment, participants feel significantly less responsibility for (4.72 versus 7.07) and regret over (6.12 versus 7.38) a negative outcome if they assume that they accept the agents' decisions to trade than if they assume that they keep their tickets. In the *Guided Agent* treatment, the levels for responsibility (5.64 versus 5.87) and regret (6.24 versus 5.99) for both trade and keep barely differ. I also find direct evidence that subjects share responsibility in the principal-agent relationship: assuming that the transaction results in a loss, principals in the *Mandatory* condition attribute a significantly larger part of the responsibility for the negative outcome to their agents (6.23) than to themselves. In the *Guided Agent* condition, they attribute a substantial amount of responsibility to their agents (4.8) even though they incentivize those trading choices (cf. Table 3).

Table 3: Responsibility and Regret

	Resp. Keep	Resp. trade	p-value trade vs. keep	Regret Keep	Regret Trade	*p*-value trade vs. keep	Resp. to Agent (vs. RespTrade)
Base	5.48	6.35	0.01*	6.5	7.2	0.02*	
Default	6.17 0.05$^+$	6.17 0.63	1	6.78 0.39	6.66 0.09$^+$	0.74	
Mandatory	7.07 <0.01** 0.02*	4.72 <0.01** 0.01*	<0.01**	7.38 <0.01** 0.10$^+$	6.12 0.04* 0.16	<0.01**	6.23 <0.01**
Guided Agent	5.87 0.4	5.64 0.09$^+$	0.68	5.99 0.18	6.24 0.02*	0.31	4.8 0.17
Optional	7.06 <0.01**	5.42 0.02*	<0.01**	7.54 <0.01**	6.28 0.01*	<0.01**	6.32 0.02*
No Agent		7.05 0.08$^+$ 0.02*			7.34 0.69 0.44		
Voting – Without Veto		4.75 <0.01** <0.01**			5.47 <0.01** <0.01**		
Voting -with Veto	7.68 <0.01** <0.01**	5.37 0.01* 0.03*	<0.01**	6.65 0.86 0.31	5.89 0.01* 0.05$^+$	0.04*	

All p-values are two-tailed T- tests. I report two p-values: (1) Treatment vs Base; (2) Treatment vs Default. I only report comparisons to Default if the treatment could have changed the status quo.

I also show that responsibility and anticipated regret indeed drive participants' trading choices. Logistic regressions indicate that subjects' choices are strongly correlated with the level of responsibility and regret they report for the decision to trade. Across treatments, the less responsibility and regret subjects report for trading, the more likely they are to trade, as shown in Table 4. This result holds across all treatments. When I control for either reported responsibility or regret for trading in my regression analysis, the effect of agency on the probability of trading disappears. This result supports my theoretical claim that subjects' responsibility for trading triggers anticipated regret and drives the endowment effect. When subjects share responsibility with their agents, they bear lower levels of decision-making disutility and are less likely to be biased against trading.

Table 4. Responsibility and Regret as Motivation for Trading Choices

	Responsibility Trade	Regret Trade
Base	<0.01**	0.02*
Default	<0.01**	0.241
Mandatory	0.02*	0.02*
Guided Agent	0.07+	0.03*
Optional	0.01*	<0.01**
Voting with Veto	<0.01**	0.06+
Voting without Veto	<0.01**	<0.01**
All Treatments	<0.01**	<0.01**

Results logistic regression, p-values for dependent

D. Voting: Experimental Design

I conducted the voting experiment online, using the same basic design and experimental protocol as for the other online treatments. I implemented two treatments and one hypothetical scenario. In the first treatment, *Voting without Veto*, the majority vote decided whether all participants would trade or keep their tickets. In the second condition, *Voting with Veto*, the majority vote decided about the trade, but each subject could veto the application of the majority decision to his own ticket. The right to veto establishes a strong rational benchmark because it allows each subject to determine his own payoff and allows us to directly compare the effect of voting with my control treatment, *Default.*[84]

Participants were informed that the session would include at least 80 subjects.[85] In addition, I presented subjects with a hypothetical scenario asking them to imagine that the group consisted of only three eligible voters. Subjects had to indicate whether they would vote for or against the trade.

My dependent variables are the vote and the veto decision. I elicited subjects' votes whether to trade in both treatments. In the *Voting without Veto* treatment, the majority vote determined the outcome for all. In the *Voting with Veto* treatment, I also elicited each subject's veto decision, as it determined his outcome.

E. Behavioral Predictions and Results

People should feel less responsible for trading their tickets when the decision is determined by majority vote because each voter shares responsibility with the others. Irrespective of a voter's expectation of the outcome of the majority

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[84] Each voter's option to veto the majority bears some similarity to each shareholder's right to use appraisal to reject a merger consideration accepted by the majority vote in favor of a court determination of the fair price.

[85] After 80 subjects had completed the treatment, access was blocked, and only those participants who had already started the experiment were allowed to finish it.

vote, each voter knows that his vote is unlikely to be pivotal and therefore shares responsibility with the others. If sharing responsibility reduces regret, as I claim, participants should be willing to vote for the trade in both voting treatments. In the *Voting with Veto* treatment, voting also should debias participants' veto decisions. As the majority should be unbiased and vote to trade, I expect participants to accept the majority's decision to trade because it allows them to share responsibility. Thus, I hypothesize that subjects will be more likely to decide to trade in both voting treatments than in the *Base* condition (Hypothesis 4).

I was interested in the impact that group size might have on debiasing. While the debiasing effect of voting in a small group could be weaker because responsibility is divided among fewer voters, voting in a small group still distributes responsibility across multiple people. As a result, I expect the trading frequency to be higher in the *Group of Three* treatment than in the *Base* treatment (Hypothesis 5). Finally, subjects should report lower levels of responsibility and regret in all voting treatments than in the *Base* condition (Hypothesis 6).

Hypothesis 4. *Voters are more likely to decide to trade than are subjects in the Base or Default treatment*

Voting strongly increases subjects' willingness to trade: 85.2% of the participants in the *Voting with Veto* treatment and 79.1% in the *Voting without Veto* treatment vote to trade the tickets, as shown in Table 2. Both results support my prediction and are significantly different from the *Base* treatment (55.6%; Fisher Exact test p-value <0.01) but do not differ statistically from one another.

In the *Voting without Veto* treatment, not all participants voted to trade, but the majority rule produced a collective outcome that is unaffected by the endowment effect: everyone traded. By contrast, subjects in the *Voting with Veto* treatment are not bound by the majority vote. Nevertheless, 88.6% followed the majority's vote and trade their tickets.

I compare my results with those for the *Default* treatment to establish that they are not caused by either a shift in subjects' reference point or omission bias. In the *Voting with Veto* treatment, subjects know that the majority voted to trade before they decide their veto choice. The expectation to trade could cause them to feel less entitled to their tickets. They also trade through inaction, which potentially implicates omission bias. In the *Default* condition, 61.1% trade their tickets, and 38.9% keep them. By contrast, in the *Voting with Veto* treatment, significantly more subjects trade their tickets (88.6%). I conclude that responsibility sharing causes the debiasing effect I find.

Hypothesis 5. *Voting in small groups increases trading compared to Base and Default rates.*

Group size does not have an impact on my results. In my hypothetical small group, *Group of Three*, 87.5% of the subjects report that they would vote for the trade, which is a significantly higher rate than in the *Base* and *Default* treatments. There is no difference compared with the treatments with large voting groups. Since the tickets cannot be traded unless at least two subjects share the responsibility for the trade, the endowment effect is muted.

Hypothesis 6. *Subjects experience less responsibility and regret in the voting treatments.*

As shown in Table 3, participants in the Voting with Veto treatment indicate that they would feel less responsibility (5.37), assuming that they traded and accepted the majority decision, than subjects in the *Base* (6.35) and *Default* (6.17) conditions. *Voting* also significantly reduces regret over trading (5.89) compared with that in the *Base* (7.2) and *Default* (6.66) conditions. The results for the Voting without Veto treatment show the same effect (see Table 3).

Subjects in the *Voting with Veto* treatment report that they expected to feel less responsibility when they accept the majority's vote to trade (5.37) than if they veto the majority in order to keep their tickets (7.68), which supports my theory. Similarly, subjects anticipate significantly less regret (5.89) when they vote with and accept the majority's vote to trade than if they veto the majority vote (6.65).

Responsibility and anticipated regret motivated the participants' trading choices in both voting treatments. The subjects' decisions to trade or keep their tickets are strongly correlated with the level of responsibility and regret they report. My logistic regressions show that they are more likely to trade the less responsibility (p-value <0.01) and regret (p-value = 0.06 with veto; p-value <0.01 without veto) they expect to experience over a negative outcome caused by their trading choice, as shown in Table 4.

IV. Voluntary Debiasing

When they assert that external intervention is required, legal scholars often implicitly assume that entitlement holders have no ability or incentive to self-debias.[86] By contrast, I reveal that people are able and motivated to self-debias. This concept of self-debiasing is new to the literature to my knowledge.

[86] Thus, even though studies show that agents are unbiased when trading on behalf of principals (see Marshall, Knetsch, and Sinden 1986), scholars do not expect the institution of agency to debias the principal, and they do not expect the biased principal to be motivated to use an agent to debias.

The endowment effect results from the disutility that people experience when they are responsible for the decision to trade. Owners have higher levels of welfare if they reduce their decision-making disutility. I claim that they can do so by intentionally transacting through institutions that divide responsibility. Thus, even when owners have no other reason to use institutions, I predict that they will voluntarily employ institutions in order to debias.[87] Accordingly, private ordering can produce optimal allocations by inducing private debiasing.

In this section, I test whether subjects voluntarily delegate to an institution solely to debias. Participants choose between conducting the trade themselves and delegating the decision to an institution that divides responsibility. In the first treatment, *Optional Agent*, I test whether subjects are willing to pay to delegate their choice to an agent. In the second treatment, *Optional Voting*, I analyze whether participants prefer deciding through a majority vote over deciding alone.

A. Optional Agent: Experimental Design

I conducted the experiment using the *Optional Agent* treatment both in the lab and online. The treatment builds on the same basic design as in the *Mandatory* treatment, with one subject in the role of principal and another in the role of agent. In this treatment, the agent is not mandatory. In the first stage, subjects were offered a choice: they could either decide on their own whether they want to trade or they could delegate the initial decision to an agent. Subjects who delegated could accept or veto the decisions of their agents. If they vetoed, they could decide whether they want to trade (see Figure 1).

Figure 1. Decision Tree for Principals

[87] Our result that entitlement holders are motivated to use institutions to debias is not brought into question by the finding of Loewenstein and Adler (1995) that people fail to predict their behavior under a different endowment status. In our study, because the subjects offered the opportunity to delegate their trading choice are endowed, they do not need to predict the regret they would feel were they hypothetically endowed. They directly experience the regret over trading their entitlement, which motivates them to use institutions to share responsibility.

To ensure that rational subjects should not delegate, I imposed a cost on delegation. I informed laboratory subjects that a decision to delegate would increase the experiment's duration by 10 minutes, which prolonged the study from a total of 15 to 25 minutes. Online participants were instructed that delegation would cost them €.05 (20% of their gains from trade). In addition, I asked online participants to indicate their maximum WTP for using the agent.

As in the *Mandatory* treatment, each subject was informed that the agent would receive his €2 payment only if he traded the principal's ticket. This incentive ruled out curiosity as a reason to delegate and ambiguity aversion as a reason not to, because principals could easily predict what their agents would do. Principals also were informed that they would learn their agent's choice should they decide not to delegate. To eliminate any effect of other-regarding preferences, I instructed the participants that their agents would receive €2 independent of whether they delegated and that a veto of an agent's decision would not influence his payment. In addition, participants also were informed that agents would not learn the participants' decisions. Subjects marked down "Yes" or "No" for each of their choices (delegate or veto and, if veto, trade or not). The experimenter informed them about their agents' decisions.

In the online experiment, I implemented a within-subject design to test whether subjects intentionally delegate to agents in order to debias. I instructed participants that they would complete two separate experiments, one of which would be randomly selected and would determine their payoff (strategy method). Subjects first completed the *Optional Agent* treatment. Afterward, they received new instructions presenting them with a base condition (which I refer to as *No Agent* to distinguish it from the stand-alone Base treatment). Evidence suggests that my within-subject design does not distort results: the frequency of trades in the *No Agent* treatment is not statistically different from the results for the stand-alone *Base* condition.

To show that subjects delegate in order to debias, I directly tested subjects' reported responsibility for and anticipated regret over their decisions, assuming first that they traded and lost the lottery and second that they delegated or did not delegate the transaction to their agents, traded, and lost.

B. Behavioral Predictions

Rational choice theory predicts that participants will not delegate because they can trade as informed and at a lower cost if they decide on their own. By contrast, according to my theory, subjects have an incentive to delegate because having an agent allows them to share responsibility and thereby reduce anticipated regret over the trading decision. Thus, principals have an incentive to delegate even if they would trade on their own, notwithstanding the regret they experience: delegation enables them to reduce the psychological cost of trading. Beyond that, they can earn gains from trade: subjects who would otherwise keep their entitlement because regret prevents them from trading can earn the bonus for selling if they delegate.

Of course, if a principal delegates to an agent, whom he expects to trade, he assumes more responsibility for the outcome than he does in the *Mandatory* treatment, in which he only confirms the agent's choice. Yet delegation still reduces the principal's responsibility, as the agent initially decides about the trade. Recall that people divide the responsibility for an outcome among all actors in a causal chain and tend to attribute the main responsibility to the affirmative action that is closest to the outcome (Spellman, 1997). Thus, a principal in the *Optional Agent* treatment should assign primary responsibility to the agent who makes the trade even though the principal intentionally delegates to him. As in the *Mandatory* treatment, the principal's subsequent decision not to veto constitutes an inaction to which less responsibility is often attributed (Baron and Ritov, 1994). I conclude that delegation should allow principals to reduce regret and hypothesize that a significant number of subjects will delegate in order to self-debias (Hypothesis 7).[88]

My within-subject design identifies the participants who want to debias in order to trade: they trade in the *Optional Agent* treatment when they can delegate and share responsibility, but keep their tickets in the *No Agent* treatment, in which they have to decide on their own. I expect the self-debiasing principals to result in a significantly higher rate of trading in the *Optional Agent* treatment than in the *Base* treatment (Hypothesis 8). Finally subjects in the *Optional Agent* treatment should report feeling a lower level of responsibility and anticipating less regret than in the *Base* or *Default* treatment, as delegation should allow them to share responsibility and reduce regret (Hypothesis 9).

C. Results

Hypothesis 7. *Subjects delegate in order to debias.*

I find that 49.2% of principals in the lab and 52.4% of online subjects delegate the trading decision to their agents, as shown in Tables 1 and 2. Both results are significantly different (Fisher Exact test *p*-value <0.01) from the rational choice prediction that no subject delegates.

If subjects delegate to enable trading at a lower cost, as I claim, then subjects who delegate should trade. I find that 89.7% of the laboratory and

[88] The hypothesis that subjects will delegate to reduce personal responsibility is also supported by evidence from dictator games (Bartling and Fischbacher, 2012; also Hamman, Loewenstein, and Weber, 2010). Instead of choosing an unfair allocation, a dictator prefers to delegate his decision to a second subject whom he expects to make the payoff-maximizing but unfair decision. Third parties attribute less blame to a principal who delegates, even though the agent's choice is predictable and desired by the principal.

86.0% of the online subjects who delegate decided to trade, which supports my theory. My within-subject design shows that 44.7% of subjects used agents to debias: they kept their ticket in the *No Agent* treatment and traded in the *Optional Agent* treatment.

Table 5. Why People Delegate: Responsibility and Regret

<table>
<tr><th></th><th>Respon.
Trade
Optional</th><th>Respon.
Trade
No Agent</th><th>Responsibility
ascribed to Agent
(vs. Resp. Trade)</th><th>Regret
Trade
Optional</th><th>Regret
Trade
No Agent</th></tr>
<tr><td rowspan="2">Delegation
(N=43)</td><td>4.90</td><td>7.09</td><td>6.32</td><td>5.88</td><td>7.1</td></tr>
<tr><td colspan="2"><0.01*</td><td><0.01**</td><td colspan="2">0.01*</td></tr>
<tr><td rowspan="2">No Delegation
(N=39)</td><td>6.03</td><td>7.0</td><td>6.33</td><td>6.72</td><td>7.54</td></tr>
<tr><td colspan="2">0.13</td><td>0.62</td><td colspan="2">0.13</td></tr>
<tr><td rowspan="2">Debias (N=17)
No Agent: keep
Optional: Trade</td><td>4.88</td><td>6.82</td><td>6.88</td><td>6.0</td><td>7.11</td></tr>
<tr><td colspan="2">0.02*</td><td>0.02*</td><td colspan="2">0.09^{+}</td></tr>
</table>

All p-values are two-tailed T- tests

The direct evidence on subjects' reported responsibility and regret confirms my interpretation that delegators debias intentionally. The subjects (N = 17) who delegated and traded in the *Optional Agent* treatment but retained their tickets in the *No Agent* treatment report that they would feel significantly less responsibility (4.88 versus 6.82, Cohen's d = −0.83) and anticipated regret (6 versus 7.11, Cohen's d = −0.58) when trading through agents than when trading on their own in the *No Agent* treatment, as shown in Table 5. The average values do not change when I consider those principals who traded in both the *Optional Agent* and *No Agent* treatments, which suggests that they also delegate to reduce their psychological costs of trading. On average, delegators report significantly less responsibility for (4.90) and regret over (5.88) trading than those principals who decided not to delegate (6.03 versus 6.72).

My claim that debiasing motivates delegation implies that principals who prefer to decide on their own expect to benefit less from delegation. To compare delegators and non-delegators, I subtract the level of responsibility and regret that participants indicated in the *Optional Agent* treatment from the value they reported in the *No Agent* treatment. A positive difference indicates that a subject benefits from sharing responsibility with the agent. I find that delegators expect having an agent to reduce their felt responsibility (2.19 versus .97; Mann-Whitney p-value =0.05) and regret (1.22 versus.82; Mann-Whitney p-value =0.43) significantly more than non-delegators. Moreover, subjects who delegated attribute significantly more responsibility for the trade to the agent (6.32; Mann-Whitney *p*-value <0.01) than to themselves (4.9), (see Table 5). The results suggest that delegators indeed perceive responsibility as being shared with their agents, as I claim.

Finally, evidence on principals' reported WTP for employing agents supports my hypothesis. Principals who use delegation to debias and trade had a significantly higher WTP (€0.37) than the remaining *Optional Agent* subjects, who indicated an average WTP of only €0.13 (Mann-Whitney *p*-value <0.01). The difference suggests that subjects are aware that delegating to agents allows them both to trade (earning them the €0.25 bonus) and to reduce their disutility of decision making. Therefore, they are willing to invest a higher amount into delegation and debiasing than are the other subjects.[89]

Hypothesis 8. *Subjects trade significantly more often in the Optional Agent than in the Base treatment.*

If participants can debias by delegation, I should observe more transactions in the *Optional Agent* treatment. Significantly more subjects traded in the *Optional Agent* than in the *Base* treatment: 50.8% of my laboratory subjects traded in the *Optional Agent* treatment, compared with only 29.7% in the *Base* condition (see Table 1). The treatment effect was replicated online, where 72% of the subjects traded in the *Optional Agent* treatment, compared with 55.5% in the *Base* treatment (see Table 2). The debiasing effect of the *Optional Agent* treatment also holds in my within-subject design: in the *No Agent* treatment only 53.7% of the participants traded, which is significantly less than in *Optional Agent* (Fisher Exact test *p*-value =0.02).

Hypothesis 9. *Optional Agent treatment reduces felt responsibility and regret.*

Subjects indicate significantly lower levels of responsibility (5.42) and anticipated regret (6.28) in the *Optional Agent* treatment than in either the *Base* treatment (6.35 versus 7.2) or the within-subjects setting of the *No Agent* treatment (7.05 versus 7.34), as shown in Table 3. The debiasing effect I observe in the frequency of trades should be caused by a reduction of responsibility and regret over trading, which supports my theory.

D. Optional Voting: Design and Results

The incentive to reduce the disutility of trading should extend to any institution that allows people to share responsibility, as long as the benefits of debiasing exceed the costs of employing the institution. To test the generality of

[89] The finding that the willingness to pay for delegation can exceed the gains from trading is consistent with our theory. Subjects who delegate in order to trade not only obtain the bonus for trading but also reduce their disutility of anticipated regret. In theory, the costs of regret for those who resist trading in the *No Agent* treatment must be larger than €0.25; otherwise, they should have traded without delegation.

this claim, after participants completed the online *Base* treatment, I asked them to indicate whether they would have preferred to make the trading decision through a majority vote that would leave them with a right to veto. In this hypothetical scenario, I asked them to imagine that they would incur a cost of €0.05 if they delegated. I also asked participants whether they would accept or veto the trade of their tickets, assuming that the majority opted to trade. Subjects had no reason to choose delegation in order to learn the outcome of the majority vote. I instructed participants to assume that they would be informed about the result of the vote before making their trading decisions even if they did not delegate.

I expected subjects to choose delegation, as it would enable them to debias, in contrast to the rational choice prediction that they should prefer to decide alone (Hypothesis 10). In addition, more subjects should be willing to trade in the *Optional Voting* than in the *Base* treatment (Hypothesis 11).

Hypothesis 10. *A significant group of subjects should opt for voting.*

45.5% of the participants preferred to decide through majority voting with veto instead of deciding on their own, which is significantly different from the rational prediction that they should not delegate. Only 7.3% of the subjects who preferred to delegate indicated that they would have vetoed a majority decision for the trade. Of the 41 subjects who were willing to delegate their choice, 51.2% wanted to use voting to self-debias: they did not trade in the *Base* treatment but indicated that they would vote for the trade, as shown in Table 2. Those delegators who trade in the *Base* and *Optional Voting* treatments likely were willing to debias to reduce their psychological cost of trading, as my design rules out all other potential reasons for delegation.

Hypothesis 10. *More subjects trade in Optional Voting than in Base.*

In total, 75.6% of the subjects in the *Optional Voting* treatment were willing to trade—which is significantly more than the 55.6% who exchanged their tickets in the *Base* treatment. I conclude that providing people with the option to self-debias significantly facilitates trading.

V. Discussion and Implications for Legal Policy

A. Internal Validity of Results

Plott and Zeiler (2005, 2007, 2011) question the validity of endowment effect evidence, identifying potential confounds and other problems in earlier studies. I structured my experiment to avoid the methodological problems they identify. They argue that an experimenter's decision to endow a subject with one good instead of another may signal its value to subjects, which increases prices. My design eliminates any basis for such a signal, as it gives subjects no reason to conclude that the experimenter cares which good the subject obtains. The tickets are identical, with the same payoff and probability of winning. In addition, the experimenter did not determine the initial endowment: laboratory subjects drew their own tickets, and online participants were informed that the computer assigned the tickets randomly. Plott and Zeiler also contend that subjects often face complicated pricing mechanisms that can distort true valuations of a good. Here, the design of my lottery game is simple. To evaluate the exchange, participants did not need to estimate and compare the expected outcome of the two tickets. They only needed to understand that each ticket had the same prospect of winning and the same payoff, which I verified with control questions.

In addition, Plott and Zeiler (2005, 2007, 2011) claim that experiments often impose higher transaction costs on those who trade, which can deter trading. By contrast, my participants had to take the same action whether they decided to trade or to keep their tickets, which leveled the transaction costs for both choices. My experiment also avoids their concern that subjects who can observe one another are influenced by others' decisions or the desire for social approval, as my participants could not see or hear one another in their individual booths.

Having addressed the concerns of Plott and Zeiler (2005, 2007, 2011), I find strong evidence of the endowment effect. Subjects exhibited the bias independently in three of my treatments: *Base*, *Information-Only*, and *Default*. My results are consistent with Isoni, et al. (2011), who also find evidence of endowment effects using an experimental design intended to control for confounds that Plott and Zeiler describe (but see Plott and Zeiler (2011) for a critique of the Isoni et al. (2011) study that does not apply to my design).

My experiment is designed to ensure that the treatment effects I observe are caused by responsibility sharing alone. To rule out alternative explanations, I conducted two separate control treatments. The first, *Information-Only*, confirmed that participants do not follow their agents' decisions on the misimpression that agents were better informed about the lottery. Participants in the *Voting without Veto* and *Guided Agent* treatments decided on the trade before they learned what choices the other subjects made, which rules out even unconscious information effects. The second control treatment, *Default*, demonstrates that neither a shift in subjects' reference point nor omission bias explains my results. The decision to trade—by the agent in the *Mandatory* treatment and by the majority in the various voting treatments—may weaken the subjects' sense of endowment regarding their tickets. The *Default* treatment controls for this effect, as subjects' tickets are traded unless they veto. Subjects in the *Default*, *Mandatory*, and *Voting with Veto* treatments can trade through inaction, which may trigger less regret. Thus, my finding that the trading frequency is higher in the *Mandatory* and the voting treatments than in the *Default* treatment rules out both explanations as causes for my treatment effects.[90] In addition, omission bias cannot drive my results in the *Voting without Veto* and *Guided Agent* treatments, as both require action. A reference point shift in the *Guided Agent* treatment also seems implausible, as the principal determines the decision of the agent to a large degree.

Other potential confounds of my findings are directly addressed by my experimental design and do not require us to control for them in separate treatments. First, my subjects should not be influenced by risk aversion because they face the same probability of losing the same prize whether they keep or exchange their tickets. I held the risk of loss constant across all treatments in the lab and online. In addition, I ensured that principals were not motivated by other-regarding preferences. In the agency treatments, principals understood that their decisions to delegate and veto could not affect their agents' payoffs and would not be known by the agents. In the voting treatments, each subject affected others only to the extent that his vote determined the outcome of the majority vote. In a group of more than 80 voters, it is unlikely that any voter is pivotal. In the *Voting with Veto* treatment, each participant determined his own payoff alone, as he had a veto.

Finally, the debiasing effect varies slightly across the institutions I test, but the differences do not suggest that an alternative mechanism is driving them. The frequency of trades in the *Optional Agent* treatment is lower than in the *Mandatory* treatment (Fisher Exact test p-value = 0.07) in the laboratory, but the effect is not robust: it disappears in the online experiments. Moreover, subjects who delegate in the *Optional Agent* treatment report as little anticipated regret over the trade as subjects in the *Mandatory* treatment. Comparing the *Optional Voting* treatment with the voting treatments that required action, I find a difference: significantly more subjects trade when voting is mandatory (Fisher Exact test p-value =0.03). Yet the treatments are difficult to compare directly because the *Optional Voting* condition is a hypothetical treatment. While the evidence is not conclusive, it is consistent with my theory predicting that optional institutions should not have a stronger debiasing effect than mandatory institutions. My theory, however, does not provide a clear prediction of whether mandating institutions should increase their debiasing effect, because people may either attribute responsibility largely to the last action in the causal

[90] Omission bias and a shift in the reference point cannot confound either our delegation or our voting results. Subjects in the *Optional Agent* treatment delegate in order to debias even though delegation requires action and even though their initial reference point is the same as in the *Base* treatment. In the voting treatments, the decision to vote also requires action.

chain (Spellman, 1997) or bear more responsibility when they choose to delegate their choice to an institution. Whether debiasing is more effective when institutions are mandatory rather than optional remains a question for future research.

B. External Validity and Implications of Results

My theory and results can be applied broadly beyond the specific settings of this study. My theory reveals that, where the endowment effect is caused by regret, institutions that divide responsibility should mute the bias. People manifest an endowment effect for a wide range of entitlements, from simple consumer goods to legal rights (see, for example, Kahneman, Knetsch, and Thaler, 1991; Depoorter and Tontrup, 2014). Anticipated regret can induce an endowment effect in all these entitlements. Sellers anticipate regret when they trade any entitlement whose future value is uncertain. Even when the transaction maximizes sellers' expected utility ex ante, it may leave them worse off once future values are realized. As the future value of most entitlements is uncertain, anticipated regret causes the endowment effect for real, intellectual, and physical property; creative works; inventions; trademarks; legal claims; settlements; corporate control; and material contract clauses. Anticipated regret also can cause an endowment effect for simple consumer goods whose values are relatively certain. Weaver and Fredrick (2012) show that people will reject a trade at a price that exceeds their true willingness to accept if the offer falls short of the market price for their entitlement, even if they cannot sell their property at all as a result. They reject trading to avoid experiencing the disutility of making a bad deal as judged by the market. Weaver and Frederick's study demonstrates how regret causes subjects to exhibit an endowment effect for easily replaceable goods, such as mugs or pencils. Thus, institutional debiasing should be effective independent of the nature of the entitlement that is to be traded.

My finding that institutions debias applies to the many institutions that distribute responsibility for outcomes among multiple actors, beyond the two I chose to test in this study. For example, principal-agent relationships can be structured in many ways, in addition to the precise forms I analyze. A principal's responsibility should depend on how much control he retains over the trade and how much decision-making authority he allocates to the agent. My study implements a hard test for my hypothesis, providing the principal with a strong control. He has either full outcome control through an ex post veto in the *Mandatory* treatment or ex ante control by determining what action is rewarded in the *Guided Agent* treatment. Real-world principals, by contrast, often share responsibility more broadly when they direct agents through loose guidelines or low-powered financial incentives. Debiasing should at least be equally effective in these cases.

While this study focuses on principals, my theory suggests that the principal-agent relationship should debias both the principal and his agent. I claim that people experience the regret that causes the endowment effect when they feel responsible for a decision that could cause them to suffer a loss. The bias thus depends on both the decision maker's stake in the outcome and his responsibility for the decision. Some agents have no direct or indirect stake in the outcome and therefore should not exhibit an endowment effect. Other agents participate in the success or failure of the transaction or may have other-regarding preferences for the principal. I expect these agents nevertheless to make unbiased decisions because principal-agent relationships usually divide responsibility between the principal and the agent. Principals tend to constrain agents' choices through ex ante guidelines, incentives, or ex post vetoes. Thus, Marshall, Knetsch, and Sinden (1986) find that participants acting as agents provide unbiased advice about whether to trade an asset. Korobkin (1998) seemingly reports a contradictory result: he assigns law students the role of an attorney and observes that they exhibit a status quo bias when choosing a default term of a contract. Yet, in his study, agents both participate in the outcome of their choices and have full authority over the decisions. The study does not include a principal who shares responsibility by giving directives, monitoring, or retaining ex post authority. My theory explains both Marshall's and Korobkin's results and resolves any apparent contradiction: if agents do not share decision-making responsibility and suffer losses, they may exhibit an endowment effect, just like an owner who trades autonomously. Otherwise, if they either have no stakes in the trade or can share responsibility in a principal-agent relationship, they should make unbiased decisions.

Yet even when the principal-agent relationship does not debias the agent, another institution may. For example, corporate directors, who have ultimate authority and usually have residual claims, nonetheless should be unbiased because they decide by majority vote. My findings thus suggest that most agency relationships will produce unbiased agents and principals.

My voting results also apply to many types of group decisions. Whenever groups vote, responsibility is divided among the decision makers. I show that group size is not a crucial factor. Thus, voting by large groups, such as shareholders of publicly held firms, and by small groups, such as boards of directors, should produce unbiased collective decisions. Because only the majority's decision becomes effective, the debiasing effect of voting is very strong: the institution produces unbiased outcomes for all, even if a minority of voters do not overcome their bias. In my study, voting debiases almost 80% of the subject population in the *Voting without Veto* treatment. Thus, in the real world, I rarely would expect biased subjects to be in the majority.

My conclusion that voting debiases is not contradicted by evidence that group discussion can reinforce preexisting biases. Galin (2013) and Blumenthal (2012) report a pronounced endowment effect when subjects are asked to discuss a trade before making a sales decision. I show that voting debiases when people share responsibility for the decision. In the Galin and Blumenthal studies, responsibility is not institutionally divided among decision makers. Blumenthal tests an individual trading decision following a collective discussion. As his subjects could not share responsibility for the sales decision with others, they should have been biased before and after the discussion. Galin asks three-person groups to reach a sales decision by open discussion, without specifying the procedure, and requires them to collectively report their decision. Thus, their discussion would not have been debiased by a decision-making process that effectively divided the responsibility between them. By contrast, under a majority vote, group members should be unbiased from the outset because they share responsibility. Group discussion prior to the vote should strengthen their unbiased preferences.

My results should underestimate the impact of institutional debiasing in real-world domains. In my study, participants transact through institutions that fulfill only one function: they distribute responsibility. The experimental design excludes all other motivations for using the institution. In real-world domains, however, people usually obtain multiple benefits from institutions. For example, agents often provide expertise. A seller should be more willing to delegate his decision to an agent when the agent both enables him to self-debias and provides expertise than in my study. In addition, I test each institution independently to isolate its debiasing effect, yet entitlement holders often transact through multiple debiasing institutions. Most organizations have several layers of agents. Many organizations also use both agency and voting, as when corporate boards rely on agents to negotiate and recommend transactions that they decide on by majority vote. Each institution or organizational layer further divides responsibility and should enlarge the debiasing effect.

C. Should the Law Intervene?

My findings suggest that legal intervention to address the endowment effect is seldom needed because, in most contexts, sellers are debiased by the institutions in which they operate or that they decide to employ. External legal intervention that reallocates or weakens entitlements is unlikely to provide a more efficient remedy for bias than does private ordering.

Debiasing institutions are everywhere, and the welfare costs of private debiasing often are small. People routinely operate within an institution that distributes responsibility for reasons other than debiasing. In some cases, the institution is prescribed by law: under corporate law, shareholders and directors decide by majority vote. In other cases, institutions are used for the many benefits they provide. Businesses usually rely on agents when selling entitlements. Many owners either have insufficient time or expertise to personally conduct every transaction or leave the management of their firms entirely to others. Individuals with valuable entitlements—such as real estate, patents, companies, or legal claims—also tend to transact through agents to take advantage of their expertise and to lower transaction costs. Regardless of why they are used, whenever present these institutions mute the endowment effect and facilitate rational contracting.

Even when people do not employ institutions for legal or instrumental reasons, I find that they are motivated to use institutions to mute the regret they anticipate. People use institutions to debias and trade when they otherwise would be deterred from trading by the endowment effect. Whenever the gains from trade and muting regret exceed the costs of employing an institution, I expect sellers to overcome their bias on their own. In addition, people debias even when the endowment effect does not prevent them from trading. The endowment effect has hidden costs not visible in trading behavior because people who decide to trade also experience disutility from anticipated regret. They trade if the gains from trade exceed the disutility. My results show that people who are willing to trade on their own nevertheless prefer to delegate their choices to institutions when debiasing costs are smaller than the psychological costs they would bear when deciding alone.

The welfare costs of private debiasing will often be small. People routinely operate within or use institutions for reasons other than debiasing. In all of these situations, debiasing does not impose any marginal social costs. Of course, the costs of agency and voting procedures are not trivial, but these costs are attributable to the institution's intended purpose—for example, the provision of expertise or the organization of collective decision making. They are transaction costs but not debiasing costs. Debiasing imposes marginal social costs only when people use institutions primarily to debias. But self-debiasing should be efficient, as it imposes costs only when the entitlement holder expects that the benefits of debiasing will exceed the cost of employing the institution. Entitlement holders who optimally should not trade, as they rationally place an above-market valuation on the good, will not engage in costly debiasing. Thus, unlike legal intervention, which affects all entitlement holders, self-debiasing imposes costs only on those who derive a net benefit from using the institution.

Of course, debiasing will never be perfect. My data suggest that some people's regret is too strong to be muted by the responsibility-sharing effect of institutions. In other cases, people would benefit from trading, but the cost of debiasing exceeds the gains from trade. My results also do not eliminate the possibility that legal intervention could be welfare enhancing when loss aversion (Tversky and Kahneman, 1991) caused by attachment to entitlements is a substantial cause of the endowment effect (cf. Ariely, Huber, and Wertenbroch, 2005). The degree to which people manifest an endowment effect independent of regret is difficult to determine. But my results and those of Weaver and Frederick (2012) suggest that this effect should not be substantial. Responsibility sharing reduces regret but should not affect peoples' valuation for, or attachment to, a good. Nevertheless, institutions that let traders share responsibility effectively mute the bias in my study. Weaver and Frederick completely eliminate or mute the endowment effect for consumer goods by giving subjects a low market price as a reference point for estimating the value of their mugs. As a result, participants do not experience regret over making a bad deal when they trade their entitlement for a second good. The results seem to suggest either that regret is the more dominant driver of the endowment effect or that attachment and regret may be interdependent causes of the bias. Emotional attachment plausibly increases regret because sellers should anticipate more regret over parting with an entitlement to which they feel attached. For other evidence of the endowment effect, a similar interaction may apply: for example, the cognitive process that focuses people's attention on their endowment (Johnson, Häubel, and Keinan, 2007), rather than on the gains from trade, also may trigger the experience of regret and induce the endowment effect. Should regret and other causes of the bias interact, then institutions that distribute responsibility should reduce regret fueled by attachment or attention as well. This remains a subject for future research.

My findings suggest that private ordering will, in most cases, lead to transactions and contracts undistorted by the endowment effect. This stands in contrast to the view of many legal scholars, who assume that private transacting cannot overcome the endowment effect and, thus, that external intervention is required. Consequently, these interventions are designed for a world in which owners are assumed to be biased. I argue that the welfare effects of legal interventions have to be assessed assuming that most entitlement holders debias through the institutions they use.

In response to experimental evidence that would seem to suggest that people usually exhibit an endowment effect in real-world transactions, scholars have offered policy proposals that fall into two broad categories. Some proposals favor reducing the negative impact of the endowment effect by adopting contractual default rules and resource allocations that favor those groups who truly value the entitlement more but who may not get the entitlement when trading is biased (see, for example, Jolls, 2000; Jolls, Sunstein, and Thaler, 1998; Korobkin, 1998; Sunstein, 2002). Social welfare generally is maximized when resources are allocated to those who value goods more, and contractual default rules incorporate the provisions that maximize most people's welfare. When people are not biased, policy makers can identify the potentially welfare-maximizing allocations and contract terms by examining people's choices when contracting is efficient (Coase, 1960). Yet, when people are biased, relying on choices to determine optimal allocations and default rules is misleading. In this case, some scholars argue that policy makers can enhance welfare by adopting the provisions they conclude are optimal in their own best judgment, even when they may conflict with private ordering (for example, Sunstein, 1986, 2002). These interventions can enhance welfare as long as people are biased. As my study shows, however, in most situations the outcomes of private contracting are not distorted by the endowment effect. As traders know best what their preferences are, the endowment effect will usually not provide a reason for policy makers to deviate from private ordering.

A second approach is to reduce the endowment effect by weakening entitlements, for example by substituting liability rules for property rules, even when property rules would be superior were the endowment effect absent (see Buccafusco and Sprigman, 2011; Rachlinski and Jourden, 1998). When owners are unbiased, property rules often are superior because private ordering should optimally allocate entitlements, while liability rules potentially allow people to appropriate entitlements for less than the owner's true valuation (Calabresi and Melamed,1972). Of course, liability rules may enhance welfare when most entitlement holders are biased because they allow involuntary appropriations that can bring goods to those who value them more. But in a world in which transaction costs are low and the majority of owners make unbiased trading choices, liability rules can facilitate only a few entitlement transfers that would otherwise not take place while imposing costs on all owners regardless of whether they are biased. In general, both forms of external intervention are plagued by the inability to distinguish between transactions that should occur absent the bias and those that should not. Unlike self-debiasing, external intervention imposes substantial costs on all potential transactions. Thus, even where voluntary debiasing is imperfect, intervention will rarely be a more efficient alternative. Recall that debiasing through institutions imposes no marginal cost when institutions are primarily used to conduct the transaction and not for the purpose of debiasing. When people do not naturally operate within institutions, debiasing should still be optimal, as people will self-debias only if the gains exceed the costs. I therefore suggest that policy makers shift the burden to proposals for external intervention to establish through direct evidence both that a substantial endowment effect persists in a particular domain, notwithstanding institutional debiasing, and that the benefits of intervening in private ordering exceed the costs.

VI. Conclusion

Legal scholars often assume that the endowment effect requires external intervention in private ordering. In this study, I show that intervention to address the endowment effect is rarely needed because people seldom exhibit the bias in the real-world contexts in which actual trading decisions are made. I present the first theory, to my knowledge, to explain how institutions that distribute decision-making responsibility mute the endowment effect without altering peoples' rights to their entitlements. The endowment effect is caused by anticipated regret over negative outcomes that could result from trading. I claim that people experience this regret only to the extent that they feel responsible for the decision to trade. Trading through institutions allows them to share responsibility with others involved in the transaction. As most transactions are conducted through institutions that divide responsibility, I expect the majority of people to make unbiased trading decisions in real markets. I test two common institutions—principal-agent relationships and voting—that divide responsibility between or among multiple actors. Both cause most subjects to debias and trade in my study. I test two common institutions—principal-agent relationships and voting—that divide responsibility between or among multiple actors. Both cause most subjects to debias and trade in my study. I also show that people intentionally employ institutions in order to self-debias. As a result, when people can freely transact, in most cases private ordering should mute the endowment effect on its own, which makes external intervention unnecessary.

This result suggests that policy makers should adopt a presumption against external intervention. Proposals to intervene in private ordering should present proof that the bias does in fact exist in a particular domain and that intervention is efficient. My study also opens new paths for future research. Sellers make their decisions in a broader social context than the institutions I analyze in this study. This social context may both allow entitlement holders to attribute responsibility to others informally and affect their regret over trading. For example, traders usually can observe the behavior of other market participants. Evidence on herding suggests that people are motivated to conform their behavior to a group to reduce their decision-making responsibility and mute regret. This herding may extend and amplify the debiasing effect of institutions. I have shown that institutions debias most entitlement holders who use them for their transactions. But even when owners do not transact through institutions, they should be motivated to conform to the dominant behavior in a market, as this reduces their regret over trading. When these traders follow an unbiased majority, herding should also lead them to unbiased outcomes.

Finally, my study demonstrates the importance for legal policy of experiments that analyze decision making embedded in the institutional and social contexts in which people operate. Laboratory experiments designed to establish the existence of decision-making biases tend to abstract from institutions to isolate the behavioral phenomenon being tested. Yet, in real-world markets, people operate within institutions that alter decision making. In order to formulate adequate legal responses to behavioral anomalies, I must understand the interaction between decision making and the institutions that people use. The endowment effect may be just one example of a well-established anomaly that is debiased and that disappears in institutional contexts.

Chapter 5 – Strategic Bias-Shifting: Herding towards Rationality

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Abstract

This article shows experimentally that individuals can adapt their decision making to social environments, like markets, and respond strategically to biases, such as regret aversion. I find they can employ herding as a behaviorally rational strategy to improve their expected outcomes and shift anticipated regret when regret would otherwise bias them towards a suboptimal status quo. Herding can improve decision making when people observe the choices of professionals and businesses, who are less likely to be biased by regret. Focusing on others' choices can allow decision makers to shift their reference point, and their bias, to favor their optimal choice. I find that decision makers exploit this process to shift their bias strategically. They seek information when their reference point is not optimal, but block it otherwise. They also strategically select among different types of decision makers and focus on those that made the better decision. My research suggests that decision makers employ strategies to reduce the welfare effects of biases in certain domains. Policy responses may support private ordering by seeking to complement, rather than substitute for, these strategies.

I. Introduction

People are not the rational actors that Rational Choice Theory predicts them to be. Their decision-making often is biased. Yet these biases need not cause them to make suboptimal decisions. I claim that people can adopt behaviorally rational strategies to improve their decision-making. In this study, I focus on regret aversion, which causes behavioral anomalies like the status quo bias, endowment effect, and the sunk cost fallacy. I analyze how decision makers in social contexts, like markets, can use herding to improve expected welfare and reduce anticipated regret.

Decision-making is biased by regret aversion across a wide range of domains. People anticipate regret over any decision whose future outcomes are uncertain, and could fall short of expectations. Anticipated regret causes disutility, reducing the expected welfare of the choice it burdens. It also can bias decision-making towards a suboptimal choice, because people anticipate systematically more regret over losses from some choices than others. For example, decision-makers tend to experience more anticipated regret over losses if they decide to deviate from the status quo than if they decide to retain it, even when their expected economic welfare would be higher if they deviate. Thus, they exhibit a status quo bias. Anticipated regret also induces an endowment effect: owners resist selling entitlements or increase the sales price because they experience more anticipated regret over selling in error than over failing to make a deal when they should have (Thaler, 1980; see Loomes and Sugden, 1982; Knetsch and Sinden, 1984; Landman, 1987; Ritov and Baron, 1992; Bar-Hillel and Neter, 1996; Connoly and Zeelenberg, 2002; see Nicolle et al., 2011, providing fMRI evidence; see generally Korobkin, 2014).

Scholars have proposed intervening in private ordering to address the biases caused by regret. Some recommend shifting the status quo by altering default terms in contracts; others offer proposals to weaken the status quo, for example, by substituting liability rules for property right protection of entitlements (see e.g. Sunstein 1986; McCaffery, Kahneman and Spitzer, 1995; Jolls, Sunstein and Thaler, 1998; Korobkin, 1998; Rachlinski and Jourden, 1998; Buccafusco and Sprigman, 2011).

My research project has a different goal. I do not focus on possible government interventions in private ordering. Instead, I analyze whether individuals can adopt behaviorally rational strategies to improve expected outcomes and reduce regret. In a prior study, I found that people can mute anticipated regret, and thus debias, by deciding through institutions, such as agents and voting, that allow them to share decision-making responsibility. People anticipate less regret over decisions when they share responsibility (Arlen and Tontrup, 2015). In this article, I show that individuals can improve their expected outcomes even when they cannot mute overall regret: they use herding as a strategy to shift regret bias to favor the choice that would be optimal absent regret.

To explain why regret tends to bias people towards the status quo, and how herding can shift this bias, I must analyze how reference points used in decision-making affect anticipated regret. Decision makers anticipate regret over potential losses that they blame themselves for because they could have avoided the loss by making an alternative choice. Anticipated regret thus roots in a comparison: the decision maker compares the potential low outcome of the choice in question with the perceived benefit of the alternative choice (see

Bell, 1982; Loomes and Sudgen, 1982; Connolly and Zeelenberg, 2002; Zeelenberg and Pieters, 2004).[91] Therefore anticipated regret over a potential loss is stronger the greater the perceived benefit of the path not chosen (Loomes and Sugden, 1982; Sugden, 1985; van Dijk and Zeelenberg, 2005).

Anticipated regret can bias decision-making towards a suboptimal choice because people assessing the opportunity cost of alternative choice options tend to focus on the more salient choice, taking it as their "reference point" (Carmon and Ariely, 2000; Ashby, Dickert and Glöckner, 2012; Bhatia and Turan, 2012; Pachur and Scheibehenne, 2012; Glöckner, Tontrup, and Bechtold, 2015).[92] Because of this focus, they more readily construct the benefits of their reference point choice when they make a decision (Kahneman and Tversky, 1982). As a result, they give more weight to the foregone benefits of their reference point choice than to the benefits of the alternative choices when they assess anticipated regret in the decision-making process (Johnson, Häubel and Keinan, 2007; Bhatia and Golman, 2012). Therefore, they anticipate more regret over a loss if they decide to deviate from the reference point because the foregone benefits of the reference point choice loom larger than those of the alternative choice. The asymmetry in anticipated regret can bias decision makers towards the reference point choice.

This decision-making process explains why individuals tend to exhibit a status quo bias. They take the status quo as their reference point when the existing state of the world is more salient than the alternative state (Bordalo, Gennaioli and Shleifer, 2012). The status quo choice tends to be salient because it is immediately available, while the alternative choice is often hypothetical. As a result, people give greater weight to foregone benefits of the status quo than to foregone benefits of the alternative choice (see Kahneman and Miller, 1986; Connolly and Zeelenberg, 2002; see also Johnson, Häubel and Keinan,

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[91] Anticipated regret also is larger the more responsibility the decision maker experiences for the decision that could produce a loss (Simonson, 1992; Zeelenberg, van Dijk and Manstead, 1998; Arlen and Tontrup, 2015). Thus, institutions that enable decision makers to share responsibility with others, such as voting and agency relationships, can reduce or eliminate anticipated regret by letting people share perceived responsibility for the decision (Arlen and Tontrup, 2015). Regret aversion also should be affected by the individual's personal level of regret sensitivity (see Schwartz et al. 2002)

[92] Our concept of reference point dependence is different from the conception of Kőszegi and Rabin (2006). For Kőszegi and Rabin, an individual has exogenous expectations about her own behavior and her future outcomes that determine her reference point. The reference point determines whether an individual perceives an outcome as a gain or a loss. For example, a decision maker who expects to earn 5, will treat 5 as the reference point. He will perceive any outcome lower than 5 as a loss, and anything larger as a gain. By contrast, I assume that the reference point is determined by attention and the focus of the decision maker. The decision maker focuses on salient choices, such as the status quo, treating it as her reference point for many decisions. The reference point determines the weight benefits receive in the decision-making process when assessing anticipated regret. In our theory, the reference point can be endogenous. The decision maker may shift focus to the choices of others, and treat the majority choice as a reference point, in order to reduce regret and make a better choice.

2007, providing evidence that the benefits of the status quo receive more weight). This asymmetry in the perception of foregone benefits leads them to anticipate more regret over a potential loss from a decision to deviate from the status quo than from a decision to if retain it (Landman, 1987; Baron and Ritov, 1994; Connolly and Zeelenberg, 2002; Nicolle et al., 2011).[93]

Yet decision makers are not bound to treat the status quo as their reference point. As the reference point depends on the subjects' attentional focus, it is an endogenous element of the decision-making process in my theory. I claim that individuals can use herding to change their reference point by focusing on the choices of others. People who make decisions in social environments, like markets, can focus on the choices of others to shift their bias to favor the majority choice. Herding can improve decision-making because many markets are dominated by professional traders and businesses that decide through institutions, such as agency relationships and voting; these decision makers are less likely to be biased by (List, 2003; Arlen and Tontrup, 2015) regret aversion. An individual who focuses on the majority choice can better construct the benefits of this choice. As a result, she gives greater weight to the foregone benefits of the majority choice, and thus anticipates less regret if she selects this choice than if she retains the status quo. Thus, a biased individual can improve expected outcomes by shifting his reference point to the choice of this majority.

I expect people to use herding strategically to alter their decision-making process. I have shown in previous research that people are aware when regret biases them against a choice that would improve their expected welfare (Arlen and Tontrup, 2015). The opportunity to make a better choice without raising regret costs should motivate decision makers to use strategies like herding, even when other motivations to conform their behavior—such as information and peer effects (Asch, 1995; see generally Thaler and Sunstein, 2008)—are not present. I claim that people can make a strategic decision to seek information in order to deliberately change their own reference point. I assume that their decision making is behaviorally rational: when biased toward a suboptimal choice, individuals seek information on others' decisions and use this new reference point to improve their expected welfare. By contrast, when their reference point is optimal, they should refrain from getting

[93] Reference point choices also can explain why some types of decision makers do not exhibit the status quo bias. Some people do not always treat the status quo as their reference point when making decisions. For example, people who acquire entitlements with the intention to trade them can be expected to have selling as their reference point decision (see Heath, Larrick and Wu, 1999). They should place greater weight on benefits from trading and focus less and place less weight on benefits of the status quo. As a result, they should not anticipate heightened regret over the decision to sell (see Johnson, Häubel and Keinan, 2007; see List, 2003, finding that sports cards owners who regularly trade their cards do not exhibit an endowment effect).

that information. In this case, information on others can make them worse off by biasing them against their optimal choice if others made an inferior choice.

I also expect people to respond strategically when presented with multiple types of decision makers who make different decisions. Markets regularly are populated by different types of decision makers, some more likely to be biased than others. For example, agents, corporate actors, and professional traders are less likely to be biased than occasional buyers and sellers (Arlen and Tontrup, 2015; see List, 2003). When decision makers can observe different groups of market participants, I predict they will select their reference point strategically: they should focus on the decision which will lead them to the higher expected outcome.

I test my theory with a series of experiments in the laboratory and online. My *Base* condition builds on an experimental design that has been shown in past studies to induce a robust status quo bias triggered by regret (e.g., Knetsch and Sinden, 1984; Bar-Hillel and Neter, 1996; Isoni, Loomes and Sugden, 2011; Arlen and Tontrup, 2015). I endowed each participant with a lottery ticket. The lottery contained two tickets, marked either Heads or Tails. Each ticket was equally likely to win the same monetary payoff. Subjects were offered the option to exchange their ticket for the alternative ticket plus a bonus of 25 €-cent. I obtained evidence on trading choices and reported anticipated regret over losing the lottery after deciding to trade or keep the ticket. Rational Choice Theory with standard preferences (hereafter RCT)[94] assumes that all subjects should trade. By contrast, and as expected, I find a significant number of subjects exhibit a status quo bias and keep their ticket compared to RCT's prediction.

In my *Herding* treatment, subjects were accurately informed that the majority of participants in a prior study decided to trade. The subjects understood that the participants in the earlier study could not have better information about the outcome of their lottery. Supporting my theory, I found that subjects in *Herding* were significantly more willing to trade than subjects in *Base*. As I predicted, subjects in *Herding* reported less anticipated regret over trading than keeping, whereas subjects in *Base* reported more anticipated regret over trading than keeping. Two additional treatments support my claim that Herding improves outcomes because participants shift their reference

[94] RCT with standard preferences predicts that choices are made based on expected monetary returns alone, absent regret. I therefore refer to the behavior of the subjects in our study as behaviorally rational. I assume that people are behaviorally rational in that they make decisions that maximize their welfare, given that they anticipate experiencing regret over making a decision. When they make a choice, they treat regret as any other non-psychological cost category. Thus I can refer to herding as a behaviorally rational strategy because it can enable people to reduce the burden of regret on the choice with the higher expected welfare, shifting regret to the choice with the lower expected outcome. They can make the better choice with lower regret costs.

point choice from keep to trade. In one treatment, I placed subjects in the *Base* condition except that I induced them to focus on the decision to trade. I predicted they should behave like subjects in *Herding*. In line with my claim, these subjects trade as frequently as those in *Herding*, reporting less anticipated regret over losses from trading than keeping, just like Herding subjects. In the other treatment, I placed subjects in the *Herding* condition except that I focused them on the benefits of keeping. I expected them to behave like subjects in *Base*. As predicted, after learning that the majority traded, these subjects nevertheless exhibit a status quo bias.

In a final set of three treatments, I tested my theory that people herd strategically. In *Seek Information*, I analyze whether subjects are willing to incur an opportunity cost to seek information on other participants' choices in order to trade with less regret. Questionnaires confirmed that subjects are aware that others are not better informed. RCT suggests that no subject should incur a cost to seek the information. By contrast, and supporting my theory, I find that half of the subjects get the information notwithstanding the opportunity cost. As expected, subjects who seek information overwhelmingly trade and report less regret over trading than keeping. Comparing treatments, I find that access to information leads to significantly more trading than in *Base*. Subjects' motivation to become informed should depend on how much they expect the information to reduce the anticipated regret over trading. Supporting my theory, I find that the subjects who get information anticipate a greater reduction of regret over trading than those participants who did not get information. My results suggest that subjects make a behaviorally rational decision: they seek information if the expected benefit of trading with reduced regret outweighs the opportunity cost; otherwise, they decide without the information.

In *Block Information* I pay my subjects 25 €-cent if they keep their ticket and instruct them that they will learn subjects' choices from a previous study unless they incur an opportunity cost to the block information. Compared to the RCT prediction that participants should not incur a cost to avoid the information, I find that a significant number of subjects do block it. The information cannot make them better off as they already have the optimal reference choice (the status quo), but learning that the majority traded could reduce their welfare by biasing them against their optimal choice. I also show that the subjects who block information expect a greater increase in regret over making the optimal choice to keep if they learn the majority traded than those who decided not to block.

In the last treatment, I present my subjects with two different trading decisions from separate treatments of a prior study: in the first treatment, a majority of participants traded; in the second treatment, the majority kept their ticket. Each group provides a potential reference point for my subjects.

As predicted, I find that subjects were significantly more willing to trade than in *Base* and were as likely to trade as in *Herding*. They also reported a significantly lower level of regret over trading than subjects in *Base*. The results are consistent with most subjects using the group that made the optimal decision as the reference point for their own choice.

This article suggests that individuals adapt their decision-making strategically to their social environment when regret aversion prevents them from making an optimal choice. My research illustrates the benefits of identifying behaviorally rational strategies and determining the domain in which they are, and can be used, effectively. I suggest that policy responses may aim to enhance private ordering by seeking to complement, rather than substitute for, these strategies.

This article proceeds as follows. Section 2 presents a formal model of my basic experimental setting. Section 3 presents the experimental test of how herding influences the decision-making process. Section 4 first models strategic herding and then present's evidence that people in fact use the strategy. Section 5 discusses the internal and external validity, and the policy implications of my results. Section 6 concludes.

II. A Formal Model

This section presents a formal model of my theory. I show how anticipated regret can bias decision-making toward a suboptimal status quo and how herding can reduce regret by shifting people's reference point, enabling them to make a better choice. The model is based on my experimental setting.

Consider an individual endowed with an entitlement, E1. Assume that there is a 50 percent chance that this entitlement will have a high value, given by B, and a 50 percent chance that it will be worthless. The individual can trade this entitlement for an alternative entitlement, E2. To focus on the role of regret, I assume that E2 earns nothing in the state of the world where E1 earns B, and earns B + g in any state of the world where E1 is worthless (where $0 < g < B$). Thus, the expected value of E1 is .5B while the expected value of E2 is .5(B + g). RCT with standard preferences predicts that everyone will trade in this setting since the expected benefit of E2 is higher.

The expected valuations alone do not represent the expected utility that an individual would derive from trading E1 for E2, however. Individuals predicate decisions on what they perceive as the expected outcome minus the regret they anticipate over making the choice (Loomes and Sugden, 1982).

Anticipated regret depends on the loss the selected choice may cause. This loss is determined by the low outcome of the choice under consideration, here 0, as compared with the benefit the decision maker would have obtained had he selected the alternative choice.[95] Since the low outcome of E1 and E2 is zero, the loss of selecting E1 or E2 is based on the benefit of the alternative entitlement, which is B + g or B, respectively.

Yet how an individual values the benefits of the foregone choice depends not only on their actual magnitude, but also on the weight they receive in the decision-making process. Some choices are more salient than others. People tend to focus on the choice that is more salient, taking it as their reference point choice. In the process of decision-making they more readily construct, and thus give greater weight to, the benefits of their reference point choice. Since the foregone benefits of the reference point choice loom larger, people tend to anticipate more regret over a loss if it is caused by a decision to deviate from the reference point than if it results from a decision to retain it. Thus, anticipated regret can bias a decision-making against their optimal choice. To model this bias, I assume that, when assessing anticipated regret, the benefits of the reference point choice receive a weight of one, whereas the benefits of the alternative decision are discounted by α, where $0 < \alpha < 1$.

This process allows us to model status quo bias. People tend to take the status quo as their reference point choice (Kőszegi and Rabin, 2006; see Johnson, Häubel and Keinan, 2007). Thus, relative losses from a decision to deviate from the status quo choice loom larger, all else equal. I model the status quo bias by assuming that the decision maker gives a weight of 1 to the benefits of the status quo, B, but give a weight of α to the benefits of the alternative entitlement, B + g, when assessing anticipated regret. Thus, in my model, a person endowed with E1 will predicate anticipated regret over a decision to retain the status quo on the difference between the low outcome of that choice, 0, and the foregone benefit of selecting E2, which she discounts by α:

$\alpha(B+g)-0$

By contrast, if the person selects E2, she will predicate anticipated regret over a decision to trade on the difference between the low outcome of E2, 0, and the benefit of E1, B, which receives full weight:

$B-0.$

As a result, even though the decision to trade E1 for E2 produces a lower potential loss when compared with the actual foregone benefits of the alternative, B, the decision maker will anticipate more regret over a decision to trade

[95] In addition, anticipated regret depends on the decision maker's perceived responsibility for the decision (Zeelenberg, van Dijk and Manstead, 1998; Zeelenberg and Pieters, 2007; Arlen and Tontrup, 2015).

whenever α is sufficiently small relative to the gains from trade, g, such that $(1 - \alpha)B > \alpha g$. In this situation, anticipated regret biases the individual towards the status quo. The decision maker will fail to trade if the gain from trade, 0.5g, is less than the difference between regret-trade and regret-keep.

I can now also model the bias-shifting effect of herding. People are not invariably biased towards the status quo because their reference point choice can change. Consider a decision maker who learns that the majority of others traded E1 for E2. If she focuses on their decision, she will take it as her reference point. As a result, she should accord more weight to the benefits of trading, B + g, and should discount the benefits of the status quo by α. Thus, the regret she anticipates over a low outcome from keeping E1 is based on a perceived loss of

$(B+g)-0.$

By contrast, she anticipates regret over a decision to trade based on perceived losses of

$\alpha B-0.$

As the latter is always less than the former, the decision maker is not biased towards the status quo. Instead, herding shifts her bias towards the optimal decision to trade. With the majority choice as reference point, trading E1 for E2 provides both the higher perceived economic benefit and the lower anticipated regret. Herding thus leads her to trade.

III. Basic Herding Experiment

In this section, I test my theory that herding can shift the bias of people who are biased by anticipated regret towards a suboptimal status quo. I find herding improves outcomes and reduces regret over trading even when individuals do not have any information or social approval reasons to herd.

A. Experimental Design

My *Base* condition is designed to replicate the status quo bias in an experimental setting in which subjects make their choices in strict isolation, without observing or being observed by others. The condition provides us with a benchmark against which I measure the welfare-enhancing effect of herding on others' choices.[96] The *Base* condition contains the fundamental structure of the experimental design shared by all treatments. I conducted my treatments both in the lab and online with marginal differences in the design that I will report below.

[96] The full instructions for all our treatments are available in the Appendix.

Each subject was endowed with a randomly selected lottery ticket marked either “Heads” or “Tails.” Subjects won 8 € (~$11) in the laboratory or 4 € in the online study if the ticket they held at the end of the session matched the outcome of the lottery (“Heads” or “Tails”); if their ticket lost, they earned nothing. Thus, both tickets had a 50 percent chance of winning the same payoff. Participants were informed that they could trade their original ticket for the alternative ticket plus a monetary payment of 25 €-cent. Since each ticket had the same expected value, RCT predicts that each subject should trade.[97] Any subject who retains the ticket deviates from RCT, and exhibits a status quo bias.

My *Herding* treatment tests whether subjects are more likely to trade their ticket after being instructed that a majority of participants in an earlier treatment traded their lottery ticket. In the laboratory, I informed subjects that 31 out of 45 subjects in a prior treatment traded. The data was obtained from an earlier experiment in which most subjects traded because a principal-agent relationship allowed them to share responsibility for the trade with an agent, reducing the regret they anticipated over the decision (Arlen and Tontrup, 2015). I informed subjects accurately that the data was collected in a prior treatment in the same laboratory with the same subject pool. Participants were not given any further experimental details about the prior treatment.

The online *Herding* treatment used the same design except that I informed subjects not only about the trading outcomes of a prior treatment but also about the decision-making process. Subjects learned that participants in the prior study voted about whether they wanted to trade, with each subject having a right to veto the majority decision as applied to their own ticket. Subjects learned that 89% of these participants traded their tickets (Arlen and Tontrup, 2015). This design tests whether people will nevertheless take others’ choices as their reference point even when they know that the other participants were placed in a different decision-making context and procedure.

To analyze whether people herd to reduce anticipated regret over an optimal decision to deviate from the status quo, I structured the experiment to eliminate alternative motivations for herding, such as asymmetric information, expertise, peer pressure, and social approval (see generally Thaler and Sunstein, 2008). In my study, subjects had no reason to rely on the majority’s choice for its informational value because no one else had better information than they did. Each ticket had the same expected value with its outcome being entirely random; the game itself was easy to understand. Control questions confirm that subjects understood that trading lead to the better expected payoff. I also asked participants whether they learned anything about the potential

[97] This conclusion holds regardless of risk preferences since the decision to trade did not affect either the expected value or the variance of the outcomes of the lottery.

outcome of their decision from the other participants' choices: 97% answered correctly that they did not. As I explain below, my subjects decided anonymously and in isolation to ensure that they were not influenced by a preference for social approval.

My main observation is the trading choice. In addition, in my online treatments I obtained data on anticipated regret. I asked subjects to report their anticipated regret over losing the lottery after subjects made their trade decision but before they learned the outcome. In online *Base*, participants were first asked to report how much regret they expect to experience assuming that they lost the lottery after trading their original ticket; they then were asked to report anticipated regret assuming that they kept their ticket and lost. For both items I used a ten-point Likert scale ranging from 1 (very little regret over the negative outcome) up to 10 (very strong regret). In the online *Herding* treatment, I asked subjects to report the regret they expect to experience assuming first that they kept their ticket and lost after learning the majority traded and, second, that they traded their ticket like the majority and lost.

B. Experimental Procedures

I conducted the laboratory study at the University of Jena. In order to implement a clean control treatment, I ensured that subjects in the *Base* condition made their decisions without any influence from other participants. I seated subjects in sound-isolated separate booths with no ability to observe the other subjects. The online study provides a similar degree of isolation.

In the laboratory experiment, the subjects received paper-based instructions. Subjects obtained their lottery ticket, marked Heads or Tails, by withdrawing a ticket from a box containing multiple sealed tickets. To ensure that subjects were aware that the outcome of the lottery was truly random, the experimenter tossed a coin for each participant, covering it without revealing the outcome. Subjects were informed that they could either accept the outcome of the experimenter's toss or toss a coin for themselves after the session was over. Prior to making their choices, subjects were asked questions to control for their understanding. They had to calculate their earnings, assuming that they traded or kept their ticket and won or lost in the lottery. They had to answer these questions correctly to proceed with the experiment. All but two subjects succeeded and completed the laboratory study. In order to impose the same effort on subjects regardless of whether they kept or traded subjects had to express their decision explicitly by marking either "Yes" to trade or "No" to keep. Subjects who decided to trade were given the alternative ticket and handed their original one to the experimenter. Payments were made in cash.

Most participants were students from a variety of disciplines. I obtained demographic variables; as reported below, these demographic characteristics did not affect my main findings.

The online experiment required two modifications of the general procedure. First, subjects did not receive a physical lottery ticket. I sent them a code assigning them a Heads or a Tails ticket before the experiment began. Participants were instructed that the code would be explained after the session and would reveal whether they initially had received a Heads or Tails ticket. To convince subjects that the lottery's outcome was truly random, subjects determined themselves whether the Heads or Tails ticket won the lottery.

All subjects in the online study were students from the University of Münster from various disciplinary fields. To ensure that subjects could not repeatedly participate in the study, each e-mail invitation contained a personal key which became invalid once the subject used it. Online participants were paid immediately after completing the study via electronic bank transfer or PayPal.

C. Behavioral Predictions

I expect participants in *Base* to use the status quo as their reference point. Subjects who focus on the status quo should give greater weight to the benefit of the status quo ticket than to the benefit of the alternative ticket, causing them to anticipate a greater sense of loss over the foregone benefit if they trade their ticket and lose than if they retain their ticket and it loses. I expect this a symmetric perception of foregone benefits to lead subjects to experience more anticipated regret over losses from a decision to trade than a decision to retain the status quo. Accordingly, I predict that subjects should exhibit a significant status quo bias induced by regret aversion in *Base*, measured against the prediction of RCT[98] that everyone should trade (Hypothesis 1).

In *Herding*, I expect the reference point to change because subjects will tend to focus on the decision of the majority, using it as the reference point for their own choice. Subjects who treat "trade" as the reference point choice should experience more regret over a decision to keep than a decision to trade because they give more weight to the foregone benefit of trading. This enhances their anticipated regret should they keep their ticket and learn that they would have won the lottery had they traded. Thus, as the decision to trade produces both the higher expected welfare and less anticipated regret, I expect subjects who adopt "trade" as their reference point to trade. Not everyone should trade however. Because subjects in *Herding* are presented with two potential reference points, the status quo and the majority decision, some may

[98] See supra note 4 (defining RCT).

retain the status quo as their reference point. These subjects should exhibit a status quo bias. On treatment level I predict that significantly more subjects will trade their ticket in *Herding* than in *Base* (Hypothesis 2).

I expect to observe two effects on regret: one within subjects and one between treatments. First, my theory predicts that *Herding* subjects should tend to take the majority's decision to trade as reference point, leading them to report less regret over losing the lottery following a decision to trade than a decision to keep (see Hypothesis 2). Second, subjects should anticipate less regret over losses from a decision to trade in Herding than in *Base* (Hypothesis 3).

D. Results

Table 1 presents the summary statistics of participants' trading behavior across treatments, in the laboratory and online. In the logistic regressions I report in Table 3 I controlled for demographic variables (sex, discipline, work experience outside of the university). I found that including the demographic variables in the analysis did not change the pattern of my results.

Table 1. Trading Choices

	Total N	Trade	Keep	Fisher test 2-tailed *(compared to Base)*
Laboratory Study				
Base	64	19 (29.7%)	45 (70.3%)	
Herding	49	27 (55.1%)	22 (44.9%)	p<0.01**
Online Study				
Base	90	50 (55.5%)	40 (44.4%)	
Herding	82	59 (72%)	23 (28%)	p=0.02*
Base With Focus on Trade	65	49 (75.3%)	16 (24.7%)	p=0.01* p=0.70 *(Herding)*
Herding with Focus on Keep	63	32 (50.7%)	31 (49.3%)	p=0.62 p=0.01***(Herding)*
Strategic Herding				
Multiple Reference Points	91	64 (70.3%)	27 (29.7%)	p=0.04*
Seek Information	64	50 (78.1%)	14 (21.9%)	p<0.01**
Block Information	63	34 (54%)	29 (46%)	p<0.01**

(+0.1 > p > 0.05; *0.05 > p >0.01; **0.01 > p)

Hypothesis 1: *A significant number of subjects in* Base *should keep their ticket compared to the* RCT *prediction that everyone trades.*

As expected, subjects in the *Base* condition exhibit a strong status quo bias: 70.3% of the laboratory subjects and 44.4% of the participants in the online treatment keep their ticket, as shown in Table 1. Both results are significantly different from the RCT prediction that all participants should trade (Fisher Exact test *p*-value < 0.01) and are consistent with the findings reported in the literature (e.g., Knetsch, 1989; Bar-Hillel and Neter, 1996; Isoni, Loomes and Sugden, 2011). My finding that fewer subjects trade in the laboratory than online is consistent with the different experimental protocol I used. The stakes were half as large in the online study as in the laboratory study. Thus, subjects should anticipate more regret over trading in the laboratory. Also, the status quo in the online study may have been less salient because the subjects did not have physical possession of their lottery ticket.

Table 2. Anticipated Regret

	Regret *Trade*	Regret *Keep*	P value trade vs. keep	Regret Difference (*Trade-Keep*)
Base	7.2	6.5	0.02*	0.7
Herding	5.12 0.01*	6.40 0.79	<0.01**	-1.28 <0.01**
Base *with Focus on Trade*	5.53 <0.01** (0.35 – vs. *Herding*)	6.43 0.88 (0.92 – vs. *Herding*)	0.04*	-0.91 <0.01** (0.37 – vs. *Herding*)
Herding *with Focus on Keep*	6.3 0.02* (0.03* vs. *Herding)*	5.52 0.02* (0.02* vs. *Herding)*	0.09+	0.78 0.85 (<0.01** vs. *Herding*)
Multiple Reference Points	4.9 <0.01**	6.23 0.45	p<0.01**	-1.33 <0.01**
	Regret *Trade with Info*	Regret *Trade without Info*	P value with vs. without Info	Regret Difference (*Trade with Info -Trade Without Info*)
Seek Information	5.49 <0.01** (0.04* - vs. *Herding)*	7.52 0.68 (vs. Base Regret Trade) (<0.01** - vs. *Herding)*	<0.01**	-1.58 <0.01** (0.49** - vs. *Herding)*
	Regret *Keep with Info*	Regret *Keep without Info*	P value with vs. without Info	Regret Difference (*Keep with Info – Trade Without Info*)
Block Information	7.11	6.03	<0.01**	1.08

P-values are two-tailed t-tests. I report two p-values: (1) *Herding* and *Multiple Reference Points* versus *Base*; (2) the comparison of trade versus keep for regret

Hypothesis 2: *Subjects trade significantly more often in* Herding *than in* Base*.*

I find that significantly more subjects exchange their ticket in Herding than in *Base*, both in the laboratory and online. As reported in Table 1, 55.1% of the laboratory participants trade their tickets in *Herding*, as compared to only 29.7% in the *Base* condition (Fisher Exact test p-value < 0.01). Similarly, 72% of the online subjects trade in *Herding* as compared with 55.5% in *Base* (Fisher Exact test p-value = 0.02). The finding that some subjects nevertheless keep their ticket in *Herding* is consistent with these participants not switching their reference point from the status quo to the majority choice.

Hypothesis 3: *In Herding subjects anticipate less regret over losses from a decision to trade than from either the decision to keep or a decision to trade in Base.*

Subjects in *Base* report less anticipated regret over losses from a decision to keep than a decision to trade (6.5 versus 7.2; Mann-Whitney p-value =0.02; regret-Keep – regret-Trade = −0.7), as shown in Table 2), suggesting a bias towards the status quo. By contrast, in *Herding* I find the opposite asymmetry: subjects indicate significantly more anticipated regret over losses assuming that they keep than if they assume that they trade their ticket (6.40 versus 5.12; Mann-Whitney p-value <0.01; regret-Keep – regret-Trade = +1.28).

Table 3. Regret Motivates Trading Choices

Treatment	Regret *Trade*
Base	0.02*
Herding	<0.01**
Base with Focus on Trade	0.13
Herding with Focus on Keep	0.02*
Multiple Reference Points	0.03*
Seek-Information	0.01*
Block-Information	1

+ 0.1 > p > 0.05; *0.05 > p >0.01; **0.01 > p; Results logistic regression, p-values for dep. variable trade.

I also find the expected treatment effect when I compare *Base* and *Herding*: participants in *Herding* reported to anticipate significantly more regret over the decision to trade than did subjects in *Base* (5.12 versus 7.2, Mann-Whitney p-value <0.01), as shown in Table 2.[99]

[99] As reported in Arlen and Tontrup (2015), in a pilot study I explored subjects' motivations for keeping by asking them to explain their decision in a free-form format. The majority of subjects who

Logistic regressions show that subjects' trading behavior in *Base* and *Herding* is strongly correlated with the level of anticipated regret they report: the less regret they report over trading, the more likely they are to trade, as shown in Table 3.

IV. Induced Reference Point Treatments

According to my theory, the information that others traded induces subject to focus on the benefits of trading. This shift of attentional focus changes their reference point and increases the relative weight they give to the benefits of trading over the benefits of keeping, causing subjects to anticipate less regret over trading than keeping their ticket. My experimental design rules out some alternative explanations, such as a goal to benefit from the expertise of others or a preference for social approval.

Yet the literature suggests an additional explanation for herding: subjects may choose not to choose (Sunstein, 2015). Thus, rather than being motivated to reduce regret and improve their choices, as I theorize, people might herd because they prefer to let others decide for them. To test whether my Herding results are driven by a shift of focus, as I claim, or subjects' preference to not make their own choice, I conducted an additional pair of online treatments: *Herding with Focus on Keep* (HFK) and *Base with Focus on Trade* (BFT).

A. Treatments and Behavioral Predictions

Herding with Focus on Keep places subjects in the *Herding* condition except that they were first asked to list reasons favoring a decision to keep their ticket. After they completed this task, I instructed them to write down reasons favoring a decision to trade. This design should induce them to focus on, and in consequence give greater weight to, the benefits of keeping (see Johnson, Häubel and Keinan, 2007), even though they were informed that the majority had traded.

Base with Focus on Trade is identical to *Base* except that subjects were first asked to list the reasons favoring the decision to trade. After participants completed this task, I asked them to write down the reasons favoring a decision to keep the ticket. This design should induce subjects to focus on, and give greater weight to, the benefits of trading, when making their decision, even though there are no choices of others that could shift their attention from the status quo to "trade." In both treatments, I collected data on trading outcomes and reported regret over losses from a decision to trade and a decision to keep.

did not trade indicated that they could not improve the probability of winning by trading and preferred not to intervene. Others said directly that they would feel bad about causing a loss in the lottery by trading.

If subjects in *Herding* would indeed follow others because they prefer to let them decide my manipulations in these two treatments should not substantially affect trading outcomes or anticipated regret: In *HFK* subjects should behave like subjects in *Herding* and in *BFT* they should behave like the participants in *Base*. By contrast, my theory suggests that I should find the opposite results. If the *Herding* treatment influences behavior in my study by shifting people's reference point, as I theorize, then subjects induced to focus on the benefits of a particular choice should be biased towards making that decision, independent of whether they receive information about the majority decision or not.

Thus, my manipulation of the subjects' focus in *HFK* should induce them to take "keep" as their reference point. Accordingly, *HFK* subjects should exhibit a status quo bias and experience more anticipated regret over trading because they give more weight to the benefits of keeping. They also should report more anticipated regret over trading than subjects in *Herding* (Hypothesis 4).

BFT should focus subjects on the benefits of trading, changing their reference point to trade. As a result, subjects should focus on the benefits of trading. They should be significantly more likely to trade, and should report less regret over trading, than subjects in *Base*. They should also anticipate less regret over trading than keeping (Hypothesis 5).

B. Results

Hypothesis 4: *Subjects in* Herding with Focus on Keep *should be less likely to trade, and should experience more regret over trading, than subjects in* Herding.

Supporting my theory, subjects in *HFK* do not follow the majority decision to trade that they were informed about. Instead I find that *HFK* subjects are significantly less willing to trade than subjects in *Herding* (59.7% versus 72%; Fisher Exact test p-value =0.01). Indeed, trading frequency is not significantly different from *Base* (55.5%; Fisher Exact test p-value =0.62).

Reported regret results also are in line with my predictions. I find that *HFK* subjects anticipate significantly more regret over losses from trading than participants in *Herding* (6.3 versus 5.12; Mann-Whitney p-value =0.03). *HFK* subjects anticipate more regret over trading than keeping, consistent with subjects in *Base*: regret-Keep – regret-Trade = −0.78 for *HFK* subjects and −0.7 (Mann-Whitney p-value =0.85) for participants in *Base*. By contrast, in *Herding*, regret-Keep – regret-Trade is strongly positive (1.28); the difference is highly significant (Mann-Whitney p-value <0.01). Thus, on average *HFK* subjects exhibit a regret-driven status quo bias, whereas subjects in *Herding* are biased towards trading, as predicted by my theory and inconsistent with a preference for not deciding.

Hypothesis 5: *Subjects in* Base with Focus on Trade *should be more likely to trade, and should experience less regret over trading, than subjects in* Base*.*

The results of *BFT* also provide support for my theory. Subjects in this treatment trade significantly more often than subjects in *Base* (75.3% versus 55.5%; Fisher Exact test *p*-value =0.01) even though they do not obtain information on others' choices. Thus, a motivation to follow others and let them decide cannot explain their behavior. Trading frequency in *BFT* is not significantly different from the trading frequency I observe in *Herding* (75.3% versus 72%; Fisher Exact test *p*-value =0.7), as my theory suggests.

Reported regret provides further support for my theory. As predicted, *BFT* subjects anticipate significantly less regret over losses from trading than subjects in *Base* (5.44 versus 7.2; Mann-Whitney -value < 0.01), as shown in Table 2. The level of regret over trading is similar to *Herding* (5.44 versus 5.12; Mann-Whitney *p*-value = 0.35), as I would expect if both herding and my manipulation of subjects' focus shifts subjects reference point from "keep" to "trade."

I conclude that subjects in *Herding* are not motivated to trade because they prefer to let others decide for them. Instead, the information that others traded induces them to focus on the benefits of trading, shifting their reference point from "keep" to "trade".

V. Strategic Herding

In this section, I show that people can use herding as a behaviorally rational strategy to improve their decision-making. Individuals can recognize when anticipated regret deters them from making a better choice. They understand that information on how others decide can help them to reduce the regret they anticipate. I present evidence that people strategically seek such information when regret would otherwise keep them from making an optimal decision. They refrain from seeking the information if they do not expect it to reduce their anticipated regret over making an optimal choice; they even avoid it if the information may lead them to a suboptimal choice. My results also suggest that people behave strategically when they observe multiple groups that each selected a different decision. They tend to focus on the benefits of the decision made by the group that selected the optimal choice. This leads them to a better outcome with the least regret. I conclude subjects behave rationally given their bias. I test my claims in the following section.

A. Strategically Seeking and Blocking Information

A strictly rational actor would not benefit from seeking the information on the decisions of others in my setting. She cannot learn from the information, as the other participants do not have better knowledge about the lottery's outcome, and as the rational actor is not biased, she cannot benefit from reducing anticipated regret either; she can always make the optimal choice on her own. By contrast, behaviorally rational actors who would make a suboptimal choice because they are biased by anticipated regret can improve their expected welfare by obtaining information on others' choices. When they recognize that they are biased against the optimal decision, they can respond rationally and seek information to reduce their expected anticipated regret over selecting their optimal choice.[100]

People can realize when regret prevents them from making a better choice. They are motivated to reduce their regret over making their preferred decision to increase their expected welfare (see Connolly and Zeelenberg, 2002; Arlen and Tontrup, 2015).[101] I assume that they expect that others may not be biased towards the status quo, and may thus be able to trade. For example, professional traders (List, 2003) or people who decide through institutions that let them share responsibility (Arlen and Tontrup, 2015) tend not to be biased against trading.[102]

My claim that people herd strategically implies that they understand that they can shift their anticipated regret by obtaining information on others' choices. In my theory, the majority decision reduces regret by inducing decision makers to focus on the benefit of the majority choice and take it as a reference point. Yet people do not have to understand this mechanism in order to herd strategically. It is sufficient if they expect to experience less anticipated regret when they decide like others.[103]

I claim that people adapt their decision-making and respond rationally to their bias by seeking information on others' decisions. I expect them to seek information if their expected welfare is higher if they are biased towards the majority decision. By contrast, they should not access information when they are at least as well off deciding on their own. Thus, whether the decision maker seeks information depends on her initial reference point and the expected quality of the others people's choices. I expect decision makers to act behaviorally rational. They should always seek information when regret prevents them from making an optimal choice and the decision they face is binary. In

[100] In our study all decision makers have the same information on the economic costs and benefits of each choice. I discuss other situations in Section V.
[101] People can employ herding as a behaviorally rational strategy if the decision maker correctly identifies the optimal decision absent regret. Yet the decision maker can be unsure of, or may incorrectly identify, the optimal choice. I discuss the implications of this scenario in Section V.
[102] In Arlen and Tontrup (2015) I show that people employ institutions strategically to self-debias. The results show that decision makers understand that people deciding through institutions are likely to be less biased by regret.
[103] People may predicate this latter expectation on past experience of feeling less regret when deciding consistent with others. Alternatively, they may simply have imagined how they would feel at the end of the experiment with and without the information.

this case the information will improve outcomes and reduce regret if others made the optimal choice; it will not reduce welfare if others selected the inferior choice. If the decision maker faces a range of choices, then she will seek information if the expected choice of the majority provides greater welfare than her own reference point decision.

By contrast, people whose reference point is optimal should not seek information on others. Accessing the information cannot benefit them and might reduce welfare if the majority selected an inferior choice.

B. Formal Model Strategic Herding

I can formalize strategic herding using the basic model from Section II. B. Assume that the individual facing the choice of whether to trade E1 for E2 can incur a cost of ε in order to learn how others decided before she makes her own decision. I now consider whether the individual will seek this information, even though the people whose choices she can observe are not better informed about the decision she has to make. I assume that

$(1 - \alpha)B > \alpha g$.

Thus, absent the information, she is biased by regret towards the status quo. If she obtains the information, the probability she learns that the others traded is given by β, where

$0 < \beta < 1$.

The decision maker in my model predicates her decision whether to seek information on her expected welfare, with and without this information. Without information, she selects the status quo. Accordingly, her expected welfare is given by

$$0.5B - 0.5\mathrm{Regret}(\alpha(B+g) - 0).$$

where $\mathrm{Regret}(\alpha(B + g) - 0)$ is experienced over keeping and losing when her reference point choice is keep.

Should she decide to get the information, the decision maker expects to be biased by regret towards the choice the majority made. Thus, if she learns that the others retained E1 she will keep E1 as well. If she learns the others traded, she also will trade, since trading would provide the highest expected outcome $(0.5(B + g))$ and the lowest anticipated regret. Thus, her expected welfare with information is given by

$$\beta[0.5\ (B+g) - 0.5\mathrm{Regret}(\alpha B - 0)] + (1-\beta)[0.5B - 0.5\mathrm{Regret}(\alpha(B+g) - 0)].$$

Hence, obtaining information only enhances welfare if she learns that others traded, which occurs with probability β. Accordingly, the marginal benefit of obtaining the information is given by

$\beta(0.5g)+\Delta Regret>0.$

where 0.5g is the marginal expected benefit of trading instead of keeping (absent regret) and ΔRegret is the reduction of anticipated regret over selecting the reference point choice when it is optimal.[104] As long as this marginal benefit exceeds the opportunity cost of accessing information, ε, she will seek the information. The strategic decision to seek information increases her expected welfare.

By contrast, the decision maker will not seek information if her initial reference point is the optimal choice. To see this, assume that my decision maker starts with a reference point of "trade." Absent information, she will trade, since this maximizes expected outcomes and produces less anticipated regret. By contrast, should she get information on others' decisions, she may learn that they traded, in which case she be no better off than she would be without the information (aside from the opportunity cost, ε). But if she gets information, she faces a $(1 - \beta)$ probability that she will learn that the others retained E1. In this case, the information would make her worse off because it would lead her to use the status quo as her reference point, biasing her towards keeping E1. Accordingly, she is worse off if she seeks the information compared to making the decision on her own. My decision maker thus will decide strategically whether to obtain information or not: She seeks information when she otherwise is biased against the optimal choice but she does not get it when she would make the optimal choice without the information.

C. Seek and Block Information: Design and Procedure

I tested my claim by implementing two treatments: *Seek Information* and *Block Information*. Both treatments were conducted online using the same subject pool and experimental procedure that I reported in detail in Section III. C.

Seek Information is based on *Herding* except that subjects were not automatically informed about the trading decisions of the subjects in the prior treatment. Instead, before making their trading decision, participants were offered a choice to either obtain information about the majority choice or decide without this information. If participants decided to access the information, they were given the same information about the majority' trading choice as in the *Herding* treatment (cf. supra Section 3); then they made their decision whether they wanted to trade or not.

[104] Regret over selecting the reference point choice should be smaller when the reference point is the optimal choice to trade because the perceived relative loss from a decision to trade is smaller than the perceived relative loss from a decision to keep: $\alpha(B - 0) < \alpha(B + g) - 0$.

To ensure that subjects only seek the information if they value it for making their choice, subjects had to incur a cost if they wanted to get the information. If they wanted to be informed, they had to wait one minute in front of an empty screen prior to the data being displayed. If they decided not to seek the information, they immediately proceeded to the trading decision. This opportunity cost takes the same form that people often experience in reality: they have to invest time to research what others decided to do. To ensure that online participants had to actively wait, instead of checking their e-mails for example, subjects had to press a button within three seconds after the minute was over to receive the data. I calibrated the waiting time by estimating the amount a subject would earn per minute in an average student job. In Münster, students earn around 10–12€ per hour, yielding per minute earnings of 16–20 €-Cent. I implemented a one-minute delay to keep the opportunity cost below the 25 €-Cent bonus subjects are paid for trading in the study.

My design ensures that subjects will seek the information only in order to trade with reduced regret. As in the Herding treatment, the choices of the others have no informational value. The subjects' identity and the decision she made remained unknown to other participants—ruling out learning and peer pressure as alternative motivations. I used control questions to confirm that subjects understood their payoffs and knew that the other subjects were not better informed about what choice would lead to a better outcome. Subjects were instructed that if they did not get the information, they would not learn it at any later stage in the experiment.

I elicited subjects' choice to seek or reject information and their trading decision. In addition, I asked subjects to report the regret they expected to experience over a loss from trading assuming two separate scenarios. First, subjects had to assume that they decided to trade and lost the lottery after they obtained the information and learned that the majority had traded. Second, I asked them to assume that they did not obtain the information, traded, and lost in the lottery. The difference between these two scenarios measures by how much subjects expect the information to reduce their anticipated regret over trading.

Block Information tests whether subjects are willing to incur a cost in order not to learn how the others decided when the information may lead them to make an inferior choice. In this treatment, participants were offered a 25 €-Cent bonus to keep their ticket, making the status quo the optimal choice. Subjects were instructed that they would be automatically informed about the decision of subjects in a prior treatment unless they decided to block this information. The instructions explained that subjects who decided not to be informed would be required to wait one minute in front of a blank screen before proceeding with the experiment. To ensure that subjects waited actively, subjects had to press a button within three seconds after the minute was over. Subjects who did not block the information were immediately informed that 91% of the participants in a prior treatment kept their ticket.[105] Subjects then decided whether to trade or keep their ticket.

Subjects in *Block Information* also were asked to report the regret they anticipated to experience in two scenarios. First, they were asked to assume that they kept their ticket, and lost in the lottery, after they received the information that the majority traded. Second, they were asked to assume that they blocked the information, kept their ticket, and lost in the lottery. Measuring the difference between the two scenarios allows us to determine how strongly subjects expected to reduce their regret over keeping by blocking the information.

D. Behavioral Predictions Strategic Herding

RCT predicts that subjects should not incur a cost to obtain information on others' decisions in my experiment because the information has no value for a rational unbiased decision maker. By contrast, I predict that subjects in *Seek Information* should be willing to incur a cost to obtain information because if they learn the majority trades they can trade with less regret. The information provides the largest benefit if anticipated regret would otherwise deter subjects from trading. In this situation, subjects improve their expected outcomes and reduce regret by seeking information.[106] Yet participants who would have traded in spite of being biased towards the status quo also can benefit from the information: it enables them to trade with less regret. Both types of subjects should obtain the information if the benefit exceeds the opportunity cost of the waiting time.

Not all subjects should decide to obtain the information, however. Subjects whose initial reference point is "trade" have no reason to seek information. Also participants who have only a small bias may expect little benefit from a shift in reference point. These two types of subjects should trade in *Base* and should report little difference in anticipated regret over trading with and without the information.

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[105] I informed subjects about the actual decisions of participants in; a separate treatment that I conducted only to avoid deception. I do not report the treatment, in which participants received an incentive to keep their ticket, in the paper.

[106] Subjects increase their expected welfare if they learn the majority traded and are unaffected should they learn the majority kept.

Accordingly, compared to the RCT prediction that no subject should incur a cost to learn the information, I expect a significant number of subjects in *Seek Information* to ask for the information (Hypothesis 6).

Participants who seek information should trade because the information that others traded should induce them to experience less regret over trading than keeping. In this group, I expect participants who would have kept their ticket absent the information, as well as those who would have traded in *Base* even without the information, to trade. As a result, on average subjects in Seek Information should be more likely to trade than in *Base* (Hypothesis 7).

I expect subjects to anticipate less regret over trading if they seek information and learn the majority traded. By contrast, those who decide not to access the information should anticipate the same regret over trading as they would in *Base*. Thus, comparing treatments, subjects in *Seek Information* should on average anticipate lower regret over trading than subjects in *Base* (Hypothesis 8).

I posit that subjects are motivated to seek information to reduce their regret over trading. Subjects should be more willing to seek information the greater its impact on anticipated regret over trading. I expect that the difference between *regret-Trading Without Info* and *regret-Trading With Info* should be greater for subjects who seek information than for those who decide not to (Hypothesis 9).

The predictions for *Block Information* mirror those I presented for *Seek Information*. In *Block Information*, the status quo (keep) is the optimal choice. Subjects whose reference point is the status quo are biased towards the optimal choice without the information. They should not be interested in the information as it does not provide a benefit. Indeed, it can only be detrimental as it may lead them to either keep with more regret or trade should they learn that the majority traded. Therefore, contrary to the RCT prediction that all subjects should accept and ignore the information rather than incur an opportunity cost to avoid it, I expect a significant number of participants to incur the opportunity cost and choose to block the information to avoid learning the majority's choice (Hypothesis 10). Yet subjects who do not expect the information to shift their reference point should not be willing to incur a cost.

I assume that subjects block the information in order to keep their ticket. Thus, any subject who blocks the data should not trade, leading us to the hypothesis that the rate of subjects who keep their ticket after blocking should not differ from the RCT prediction that everybody keeps (hypothesis 11).[107]

[107] Participants who do not block the information learn that the majority kept. Thus, they keep as well, since keeping is both the reference point choice and the optimal choice.

I expect subjects should be more motivated to block the information the greater its expected impact on their anticipated regret over keeping. I measured the difference between *regret-Keep with Information* and *regret-Keep Without Information*. This difference should be significantly greater for subjects who block the information than those who do not (hypothesis 12).

E. The Results for Strategic Herding

Hypothesis 6. *Subjects in* Seek Information *incur an opportunity cost to learn the majority choice.*

I find that half of the subjects in *Seek Information* (32; *N* =64) decide to get the information, as shown in Table 4. The result is significantly different from the RCT prediction that people will not incur any opportunity cost to obtain the information (Fisher Exact test *p*-value <0.01).

Hypothesis 7. *In* Seek Information *subjects trade more often than in* Base*.*

The vast majority (90.1%) of subjects who obtain the information that the majority traded also trade their ticket. As predicted, subjects are significantly more likely to trade in *Seek Information* than in *Base*: 78% versus 55.5%; *p*-value < 0.01, as shown in Table 1.

Table 4. Strategic Herding: Seek and Block Information

	N	**Seek**	**Accept**	***p*-value vs. RAT prediction**	**Trade Overall**	**Trade *with Info***	**Trade *without Info***	***p*-value vs. Base**
Seek Information	64	32 (50%)	32 (50%)	<0.01**	50/14 (78%)	29/3 (90.1%)	21/12 (63.6%)	<0.01**
	N	Accept	Block	*p*-value vs. RAT prediction	Trade Overall	Trade *with Info*	Trade *without Info*	*p*-value vs. Base
Block Information	63	34 (54%)	29 (46%)	<0.01**	9/54 (14.3%)	4/30 (11.8%)	5/24 (17.2%)	

All *p*-values are two-tailed Fisher tests. + 0.1 > p > 0.05; *0.05 > p >0.01; **0.01 > p

To support this evidence, I asked participants in a post-experimental questionnaire whether they would have traded without having the information: 46.8% of the participants who decided to get the information and then traded indicated that they would have kept their ticket without the information.

Hypothesis 8: *On average subjects in Seek Information should anticipate less regret over trading than subjects in Base.*

As predicted, subjects in *Seek Information* report that they anticipate significantly less regret over trading if they assume that they obtain information and learn the majority traded than subjects in *Base*: 5.49 versus 7.2

(Mann-Whitney *p*-value <0.01), as shown in Table 2. Also, in support of my theory, subjects who assume that they do not access the information report that they anticipate a similar degree of regret over trading as subjects in *Base* (7.52 versus 7.2; Mann-Whitney *p*-value = 0.68).

I condition regret on whether subjects in fact sought information or not in order to estimate the effect of access to information on average anticipated regret over trading. Subjects who seek information (*N* = 32), report anticipated regret over losses from trading of 5.91 assuming that they obtain information and learn the majority traded. Subjects who reject the information (*N* =32) report an average anticipated regret over trading of 7.06. The results lead to an average regret over trading of 6.48, conditioning regret on subject's actual decision whether to get information or not. A comparison with *Base* (7.2) yields that access to information leads to a significantly lower level of anticipated regret over trading (Mann-Whitney *p*-value =0.08), even though only half of the subjects actually seek information.

Hypothesis 9. *Subjects who seek information benefit more from having the information, and thus are more motivated to get it, than those who reject it.*

Supporting my claim that people seek information because they expect it to reduce the regret they anticipate over trading, I find that subjects in *Seek Information* report they would experience significantly less regret over trading if informed that the majority traded than if they trade without this information: 5.49 versus 7.52 (Mann-Whitney *p*-value < 0.01), as shown in Table 2.

Subjects should be more likely to get the information on others the more they expect it will reduce their anticipated regret over trading. Indeed, I find that the difference, regret-Trading without Info – regret-Trading with Info, is greater for subjects who seek information than for those who do not: −2.26 versus − 1.09; Mann-Whitney *p*-value =0.08, as shown in Table 5.

Hypothesis 10. *Subjects in* Block Information *incur an opportunity cost to avoid learning the majority's choice.*

In *Block Information*, 46% of the subjects (29; *N* =63) decide to reject the information and wait for one minute, as shown in Table 4. The result is significantly different from the RCT prediction that people should accept the information to proceed with the experiment immediately (*p*-value < 0.01).[108]

[108] The finding that 54% of participants accept the information does not contradict our theory. As explained, several types of subjects have no reason to incur a cost to block the information, see hypothesis.

Table 5. Strategic Motivation:
Seek&Block Information Reduce Regret

	Regret – Subjects who ***seek*** Info (Regret trade with Info – trade without Info)	**Regret –** **S**ubjects who ***reject*** Info (Regret trade with Info – trade without Info)	***p*-value** Impact Info
Seek Info (25 €-cent for *Trading*)	-2.26	-1.09	0.08+
	Regret – Subjects who ***block*** Info (Regret keep without Info – keep with Info)	**Regret –** Subjects who ***accept*** Info (Regret keep with-out Info – keep with Info)	***p*-value** Impact Info
Block Info (25 €-cent for *Keeping*)	-1.83	-0.44	0.03*

+ 0.1 > p > 0.05; *0.05 > p >0.01; **0.01 > p

Hypothesis 11. *Subjects who block the information keep their ticket.*

Consistent with my theory, the majority of subjects who block the information keep their ticket: 24 out of 29; 82.8%. Nevertheless, the result differs from the theoretical prediction that everybody should keep their ticket (Fisher test *p*-value =0.06).

Hypothesis 12. *Subjects who block information expect it to have a greater impact on the regret they anticipate over keeping than those who do not block it.*

Supporting my theory, I find that subjects who block the information expect the information to have a greater impact on regret over keeping than those who do not block it. The difference regret-*Keeping-Without-Info* – regret-*Keeping With-Info* is greater for subjects who blocked the information than for those who did not: −1.83 versus −0.44; Mann-Whitney *p*-value <0.03, as shown in Table 5. Thus, subjects who expect to experience more disutility from learning that the majority traded are more likely to block the information.

VI. Multiple Reference Points

Market participants are not all the same type. Some are professional dealers who trade regularly. Others operate within organizations and decide through institutions that divide responsibility. Others transact infrequently and decide alone. These differences can affect regret aversion (Arlen and Tontrup, 2015; see List, 2003). The presence of multiple types can allow decision makers to identify majority decisions of distinct sub-groups that may differ from the dominant behavior in the overall market. As a consequence, a market may not just reveal a single reference point choice, but a set of choices favored by different types of market participants.

I claim that decision makers who can identify multiple groups of market participants—e.g., professionals and nonprofessionals—making different choices, can decide strategically which group to take as a reference point for their own decision. Instead of taking a reference point at random, they tend to select the reference point that allows them to make the perceived optimal choice.

A. Experimental Design and Procedure

The *Multiple Reference Points* treatment (MRP) extends the Herding treatment by presenting subjects with the trading decision of two different groups of subjects who made opposing decisions about whether to trade. Subjects were informed that the first group of participants decided on their own whether they wanted to trade: 70% of these participants kept, 30% traded. Subjects were informed that the second group of participants did not decide alone. This group could either accept or reject the recommendation of an agent who suggested to trade; subjects in this group were instructed that the agent could not have any better information about the lottery's outcome than the principal: 77% of the participants exchanged their tickets.[109] In this setting, subjects could focus on either majority choice when making their decision. Subjects who do not distinguish between the groups but aggregate the information would have faced a nearly equal split between trading and keeping, leaving them with no salient reference point choice. Subjects who therefore did not have a focal group choice could have focused on the status quo as in *Base*. Subjects should only be able to use the information to change their reference point from the status quo of "keep" to "trade," and reduce regret over trading, if they select the group whose majority made the optimal decision to use as their reference point. Note, I measured regret using the same protocol as in *Herding*.

B. Behavioral Predictions

The treatment presents subjects with multiple reference points. One group, who decided by majority, traded their ticket; a second group of participants, who decided alone, kept their ticket. In addition, subjects also could have used the status quo as a reference point for keeping the ticket. Thus, three reference

[109] The agent was incentivized to trade for the principal, but the principal had a right to veto the agent's choice and make the decision himself. As the lottery was random, the agent could not have better information about the outcome than the principal. The data for these treatments is reported in Arlen and Tontrup (2015).

points are available two of which favor keeping the ticket. I predict that subjects will behave strategically and focus on the group who decided to trade, because it enables them to trade with less regret.

If subjects are able to strategically select their reference point, as I claim, and focus their attention on the trading majority, then the trading frequency in MRP should be significantly higher than in *Base*, where the status quo is presented as the only reference point. Instead, the frequency of trading should be similar to the one I observe in *Herding*, as subjects in both treatments should focus on a group who made an optimal choice (Hypothesis 13).

I claim that subjects in *MRP* systematically choose the reference point that leads them to the better outcome. Accordingly, they should report less regret over trading than over keeping their ticket, just as the participants in *Herding*. They also should anticipate less regret over trading than subjects in *Base* (Hypothesis 14).

C. Results

Hypothesis 13. *Subjects are more likely to trade in* MRP *than in* Base*.*

In support of my theory, that subjects can strategically select the reference point that allows them to trade, I find that *MRP* subjects are more likely to trade than subjects in *Base*: 70.3% (64/27; N = 91) of the *MRP* participants trade, which is significantly more than the 55.5% (50/40; N = 90) of the subjects who traded in *Base* (*p*-value = 0.04), as shown in Table 1.

Indeed, my results suggest that subjects can switch to the optimal reference point as effectively as in *Herding*, where they are presented only with a trading majority. I find that subjects in *MRP* are at least equally likely to trade as subjects in *Herding* (70.3% versus 72%; odds ratio = 0.923; confidence interval 95% lower bound[110]: 0.56 compared to confidence interval of perfect equivalent with odds ratio 1 = 0.53; difference < 10%). Note that this result is unlikely to occur if subjects focus randomly on one of the groups, taking either "keep" or "trade" as their reference point. In that case I would not expect them to be as likely to trade as participants in *Herding*.

Hypothesis 14. *Subjects in MRP report less regret over trading than keeping; regret over trading is smaller than in Base.*

My findings on regret support the choice data on trades I reported above. Subjects in *MRP* expect to experience less regret over trading than over keeping: 4.9 versus 6.23, Mann-Whitney *p*-value <0.01. The treatment effect

[110] Note that if subjects are at least equally likely to trade as subjects in Herding, I can conclude that more subjects take the trading majority as a reference point as I claim. Therefore, I refer only to the lower bound of the confidence interval.

suggests that subjects select the group to focus on strategically; they reduce regret by taking a reference point of "trade." Indeed, subjects in *MRP* report they would anticipate significantly less regret over a negative outcome from trading than subjects in *Base*: 4.9 versus 7.2; Mann-Whitney *p*-value <0.01. By contrast, subjects do not anticipate more regret over trading than in *Herding* (5.2), a result that should not occur if subjects take their reference point randomly, rather than systematically choosing the optimal reference point "trade."

My study leaves open how individuals select the best reference point. One possibility is that they use heuristics to identify the group most likely to have made an optimal choice. For example, they may focus on the decisions of professional traders and those deciding within organizations because these actors are less likely to be biased by anticipated regret (see Arlen and Tontrup, 2015).[111] Another possibility is that decision makers evaluate multiple reference points in the process of decision-making, compare results, and then select the reference point that leads them to the better outcome.[112]

VII. Discussion and Policy Implications

In this section I discuss the internal and external validity of my results, and possible policy implications of my research.

A. Internal Validity

Plott and Zeiler (2005, 2007, 2011) analyzed endowment effect studies, and identified methodological problems that can distort results and undermine internal validity. I designed my experiment to address the concerns they identify.

My experimental design ensures that subjects in *Base* should trade their ticket unless they are biased by anticipated regret. The two lottery tickets the subjects could exchange were identical—with the same expected value and risk of loss. Trading earned the subjects a bonus of 25 €-Cent. Each subject received her original ticket through a random process: Laboratory subjects drew their own ticket and online participants were informed that the computer assigned the tickets randomly. The process ensured that subjects could not believe that the experimenter assigned them a more valuable ticket. Subjects also incurred the same transaction cost whether they kept or traded their ticket: in each case they had to actively select their choice. To avoid another source of

[111] The finding of Arlen and Tontrup (2015) that subjects use agents and voting to mute anticipated regret suggests that they understand that people deciding through institutions are less likely to make decisions that are biased by anticipated regret.
[112] Of course, this selection may be biased by the processing order. The order may influence how people perceive and how much weight they assign to the information during the rest of the evaluation.

error that Plott and Zeiler discuss, misperception and confusion, I did not employ a complex pricing or valuation mechanism (see Plott and Zeiler critiquing the Becker-Degroot-Marshak mechanism; Cason and Plott, 2014). Instead, I presented subjects with a simple exchange of one entitlement for its economically identical counterpart plus a monetary bonus. I verified subjects' comprehension with control questions. My result that people exhibit the status quo bias in the *Base* condition is consistent with Isoni, Loomes and Sugden (2011) who also find that people exhibit a status quo bias after controlling for many confounds that Plott and Zeiler describe.[113]

In addition to attending to Plott and Zeiler's critique of endowment effect studies, I addressed potential confounds specific to my experiment. Risk aversion should not influence subjects' choice of whether to trade or keep their tickets because both tickets have the same probability of losing the same prize.

I designed the *Herding* treatments as a clean test of my claims: first herding can change people's reference point and shift their anticipated regret over losses to favor trading and second, that people refer to the choices of others strategically to improve their outcomes. To isolate people's strategic motivation, I designed my treatments to eliminate other possible motivations for herding, such as the desire (1) to obtain social approval (see generally Cialdini and Trost, 1998; Krupka and Weber, 2009; Thaler and Sunstein, 2008), (2) to benefit from others' expertise (see Thaler and Sunstein, 2008), or (3) to reduce cognitive costs (see Kahneman, 2011). Subjects were aware that they could not obtain approval from others for their decisions because they knew that no other subject in their treatment or the prior treatment could observe or learn about their choices. Subjects also were aware they could not rely on others for expertise or superior knowledge because no other subject could know whether trading or keeping the ticket would lead to a better result. The lottery's outcome was random, with each ticket having an equal probability of winning or losing. My control questions confirmed that subjects understood that nobody could have better knowledge than they did. The decision-making task was simple and subjects grasped it immediately so subjects should not have relied on others to save cognitive effort either. Indeed, the results of *Herding with Focus on Keep* demonstrate that subjects did not simply follow others to reduce cognitive costs. Finally, in *Seek Information*, I imposed an opportunity cost on subjects for accessing information to ensure that subjects did not obtain information either randomly or out of mere curiosity.

B. External Validity

My laboratory experiment places subjects in a stylized situation: Subjects can trade an unusual good, a lottery ticket, for an identical alternative good in strict

[113] For a critique of their results, which does not apply to our experiment, see Plott and Zeiler (2011).

anonymity. It is a one-shot game. While I have shown before that this stylized design enables us to cleanly isolate the behavioral effects I report, I must address whether my results apply to decision-making outside the laboratory.

I decided to use lottery tickets for my exchange experiment instead of physical goods to provide a clean test of the impact of herding on anticipated regret. Participants cannot learn about the value of the goods from each other. By contrast, when trading physical goods participants may believe that other subjects have better knowledge about the true value of the good. Yet even though the good is unusual, I do not expect my subjects to make systematically different choices than they would when deciding over other standard entitlements whose future valuation is uncertain, such as material contract terms, legal settlements, stocks, or physical goods. People anticipate regret when they make a decision where future outcomes could fall short of expectations. Lotteries make the uncertainty of their outcome salient. Therefore, although lotteries may seem to be more likely to trigger regret aversion than usual goods, in fact uncertainty about the future outcomes and value is characteristic of many transactions, such as the settlement of legal claims, the selection of material contract terms, and transactions of real and intellectual property. Anticipated regret can even distort trading over simple consumer goods whose market values may be relatively certain. People seem to resist trading at their true Willingness to Accept value when they anticipate feeling regret over making a bad deal compared to market prices. Expected regret induces people to insist on the assumed market price, even when they may not be able to sell for that price (see Weaver and Frederick, 2012). All these choices can lead to salient losses and therefore trigger anticipated regret, just as the lottery tickets in my experiment. Indeed, evidence that investors hedge and contracting parties include terms to reduce and allocate losses from uncertain future events suggest that potential losses from these transactions are salient and considered by decision makers.

The use of lottery tickets also may have suppressed other driving factors of the status quo bias and the endowment effect, however. For example, endowment with a physical good might trigger a greater sense of attachment than possession of a lottery ticket (Tunçel and Kammitt, 2014). Note that attachment differs from sentimental value. Sentimental value arises from a person's relationship to the good and thus increases both Willingness to Accept and Willingness to Pay (see generally Korobkin, 2014). For example, sentimental value will lead an individual to have a higher valuation for a house owned by her family for generations independently whether she owns the house or finds it owned by others. By contrast, attachment can be caused by endowment alone, and can bias people towards goods they own or possess. Attachment causes an individual to value a house more because she owns it.

Yet even though the nature of the good should affect attachment, and thus the magnitude of the status quo bias, I do not expect attachment to weaken the validity of my results for two reasons. First, people's sense of attachment, and its biasing effect, depends on their reference point. When individuals focus on their endowment and take the status quo as their reference point, the perceived benefits of the good are enhanced. People feel attached and exhibit a status quo bias. By contrast, professional traders who acquire goods to sell them, and thus have a reference point of trade, focus on the benefits of trading and not the benefits of the good (see Novemsky and Kahneman, 2005). They are reported not to feel attached and do not exhibit a status quo bias (see List, 2003). Thus, while it is true that the nature of the good can influence attachment, and affects the magnitude of the status quo bias, people should only experience attachment when they have a reference point of "keep." Herding changes this reference point and enables decision makers to take a reference point of "trade." Herding thus should not only affect regret but also attachment. Second, the results of Weaver and Frederick (2012) seem to suggest that anticipated regret, not attachment, may be a primary driver of the status quo bias for physical goods as well (see Loomes and Sugden, 1982; see generally Korobkin, 2014). For example, Weaver and Frederick eliminate the disparity between subjects' Willingness to Accept and Willingness to Pay for a pen through a manipulation that reduced the seller's expected regret over making a bad deal compared to market prices.

In addition, subjects in my experiment are offered an unusual exchange: to trade one good whose value is purely monetary for an identical good of the exact same value plus a bonus. Subjects in my design, thus, have a strong reason to focus on the others' choices because they faced the same decision and should likely have similar preferences over the goods' only attribute, economic returns. When selecting between choices with more complex attributes, people may not be as confident that other decision makers will value those attributes the way they do. Thus, they might not focus as much on the choices of others as the subjects in my study did. Yet people who are motivated by regret aversion to seek information about others' choices may be able to identify people with similar preferences even when goods have multiple attributes. They can focus on those people who selected goods with the attributes they value as well. For example, automobile owners who place a high value on speed can focus on the choices of those who purchased sports cars; those who value safety can focus on the choices of those who purchased cars with high-safety ratings.

My experimental design that lets subjects exchange perfectly identical goods also makes it salient for subjects that regret aversion is biasing them against the optimal choice. Since the goods are identical, the bonus makes it salient which choice maximizes expected welfare absent regret. When people value an entitlement across multiple attributes, people who are biased by regret towards a suboptimal choice might try to avoid admitting their bias by defending their choice on other grounds. They may select a suboptimal choice to minimize regret, and then rationalize their decision by referring to the attributes that favor their choice. However, my results suggest that this response to the status quo bias is unlikely. People who are biased by regret need not select, and then rationalize, a suboptimal choice in order to minimize regret. I show that they are motivated and able to seek information on others' decisions in order to both minimize regret and improve expected outcomes.

Yet in real environments the decision maker may not realize her bias at all: the reference point can affect both anticipated regret and people's assessments of the expected value of each choice. When people are exchanging goods that are not identical there is a greater likelihood that their biased perception of benefits could cause them to conclude that their choice is optimal when it is not. Yet even when people are not aware of their bias, herding should be effective at reducing anticipated regret over making the optimal choice in markets dominated by professionals and businesses who are not biased against the optimal choice. People should focus on the majority's choice, using this decision as their reference point. When the majority makes an optimal decision (e.g. to trade) herding should bias people towards this choice, even though they otherwise would be biased towards the suboptimal status quo. Nevertheless, people who fail to realize that they are biased may not engage in strategic herding. People will not actively seek information if they believe incorrectly that they make an optimal choice: in their perception, herding cannot improve their expected outcomes. My strategic herding evidence shows that people can determine, in spite of their bias, that the alternative choice provides higher expected monetary welfare. However, when entitlements have multiple attributes, the reference point choice may bias the perceived benefits more.

The design of my herding treatments eliminates all other motivations individuals may have to consider the choices of others in their decision-making process. In my treatments, I asked subjects to decide in strict anonymity, unobserved by others, in order to eliminate the desire for social approval as a motivation to herd (e.g., Asch, 1995). My design also ensures that my subjects cannot learn to make a better choice from the decision the other participants made. Learning can be a strong motivation to herd, either because others have better information or because it enables the decision maker to save the cognitive effort needed to figure out the best choice (see generally Thaler and Sunstein, 2008). Nevertheless, the decision-making situation I put subjects in is not uncommon. People make many important decisions anonymously, free from social approval effects. For example, people decide anonymously when they transact online, through agents, or through shell companies; they also may be able to settle legal disputes in secret. Decision makers also face decisions where others are not better informed about their optimal choice.

On the other hand, there are also many situations, such as the sale of real estate, where people's choices are observable and may be affected by their desire for social approval (see Asch, 1995). People can also have expert knowledge the decision maker does not have. These alternative motivations to herd should not undermine the results of my *Herding* treatments. Indeed, they provide an additional reason to take the majority decision as the reference point choice, enabling the decision maker to make the same choice with less regret. But the motivations can have a downside: They can limit the decision maker's ability to herd strategically by seeking information only when her own choice would otherwise be suboptimal, and not accessing it when she can make an optimal decision in expectation without it. For example, a desire for social approval could lead people to seek information because they want to fit in, even at the cost of giving up an otherwise optimal choice. The benefits of social approval will undermine strategic herding whenever they are so substantial that they exceed the perceived marginal benefit of selecting the optimal choice instead of the majority choice. Similarly, people who herd to obtain information might not block information optimally if they incorrectly conclude that others have additional knowledge about the decision they face. While subjects may seek information optimally in expectation considering their uncertainty in this situation, herding can cause information cascades and multiply decision errors should the majority make a suboptimal decision. Of course, herding will increase welfare if the others indeed know more and make superior decisions. I will discuss the implications of the limits of strategic herding in the policy section.

Finally, my study is a one-shot game which does not give subjects the opportunity to learn from outcomes. In real environments, people are likely to make some decisions, such as trading a specific entitlement, repeatedly. This gives them an opportunity to learn. Thus, while I find a substantial status quo bias in my study, this result might dissipate if subjects were able to repeat the experiment. Yet empirical evidence contradicts this objection. List (2003) shows that learning effects are slim: the status quo bias persists even when people engage in repeat decisions.[114] Repetition alone does not shift people's reference point. Repetition may reduce regret, but only when it leads people to

[114] In an experimental study, Engelmann and Hollard (2010) report that forced trades—that require people to part with the good at the end of the session—also increase trading. While Engelmann and Hollard (2010) attribute the effect to learning about the process of trading, this result also is well by explained by our theory. Forced trades shift subjects' reference point to trade because subjects know that they necessarily will have to part with the good. The reference point shift also explains why subjects immediately changed their behavior in the study. Subjects behaved just as professional traders do.

take a different reference point, as happens with professional traders for example. Thus, I do not expect my results to change with repeat play alone. Note also that the one-shot game is particularly representative of the trading behavior I am interested in, trading by non-professionals, as these are the decision makers most likely to exhibit a status quo bias (List, 2003; see Arlen and Tontrup, 2015).

C. Policy Implications

The literature on behavioral law and economics has tended to focus on policy interventions to reduce the magnitude and negative welfare consequences of regret aversion, and the biases it causes, for example the status quo bias, the endowment effect, the sunk cost fallacy, choice referral, defensive decision-making, and the zero-risk bias. Scholars have proposed interventions such as shifting entitlement allocations and weakening people's rights to their entitlements (e.g., Sunstein, 1986; Jolls, Sunstein and Thaler, 1998; Korobkin, 1998, 2003; McCaffery, Kahneman and Spitzer, 1995; Rachlinski and Jourden, 1998; Coates and Subramanian, 2000; Buccafusco and Sprigman, 2011; see generally Korobkin, 2014).

The goal of my research program is different. I try to identify behavioral strategies that decision makers employ themselves to address regret aversion. These strategies can enhance the efficacy of private ordering. In Arlen and Tontrup (2015), I found that people can self-debias by deciding through institutions, such as agents or voting, that allow them to share decision-making responsibility with others. When they share responsibility, they mute regret aversion. People are willing to pay to use these institutions because they can improve their outcomes and reduce the disutility of regret. In this article, I explore another behavioral strategy, herding. I have shown that, unlike voting and agency institutions, herding does not enable decision makers to make a decision unbiased by regret. Instead, it improves outcomes by shifting expected regret, relieving the preferred choice and burdening the suboptimal choice instead.

My theory and evidence suggest that people may use herding effectively to address regret aversion when deciding in social contexts such as markets. Markets regularly support strategic herding. First, they allow people to observe others' choices in many social environments, either directly or online.[115] For example, property owners can easily access offers or exchanges by other entitlement holders of real and personal property online on sites such as eBay, Edmunds, or Craigslist. Producers of intellectual property have access to trade

[115] I focus on transactions by individuals because businesses are unlikely to experience anticipated regret when deciding to sell their entitlements because as professional traders, they usually obtained them for resale (List 2003), and generally trade through institutions (Arlen and Tontrup 2015).

journals, online forums, and advisors that provide information on transactions by other owners of similar entitlements. Information on existing and proposed contract terms, and their adoption by others, often is available through trade organizations (Bernstein, 2015) and law firm memos. Thus, decision makers operating in today's information-technology-based markets often easily learn the choices others have made.

Markets also enable decision makers to observe others who are less likely to be biased by anticipated regret: e.g., professional and repeat traders (List, 2003) and those deciding through institutions, such as agents (Arlen and Tontrup, 2015). Focusing on the choices of these market participants enables a biased decision maker to set her reference point at the choice she should expect to prefer were she unbiased. My results demonstrate that decision makers can use herding effectively even when markets include both biased and unbiased market participants. I show that people presented with multiple groups of decision makers can strategically focus on the choice of the group that made the better decision. Thus, I expect people to be more likely to focus on the choice of professional traders and those deciding through agents.

Of course, people will not always be able to use herding effectively. Information costs may be too high. People may err in their assessment of the optimal choice and not refer to others when they should. They may also seek information when they should not because they are uncertain and believe others have more information. This may result in informational cascades, if the majority makes a suboptimal decision (Hirshleifer, 1995; Anderson and Holt, 1997). On the other hand, strategic herding shows that decision makers are aware that relying on others' choices can be a double-edged sword: People can make behaviorally rational decisions to block information on how others decided because they anticipate a negative effect on their decision-making.[116] I need additional research to identify the domains where herding is an effective behavioral strategy.

Behavioral strategies, such as herding, warrant particular attention because they may complement, or even be superior to, external intervention. External interventions designed to alter outcomes through mandates or influential defaults have to apply generally and cannot refer to the utility function of a particular individual. Thus, they may push some people toward a suboptimal choice (see Bubb and Pildes, 2014). For example, people who would decide optimally absent intervention may be worse off with a new default if it is optimal on average but not optimal for them. The government-provided default is likely to bias their choices, which could make them worse off. By contrast, herding allows decision makers to use the choice of the majority strategically

[116] Past studies that analyze herding did not give subjects strategic options to block information.

to reduce anticipated regret over selecting the choice that they expect to be optimal for them. They then can use their own preferences and information to determine whether this reference point is their optimal choice. Behavioral strategies may in particular be superior to intervention in dynamic environments. When markets are populated by professionals and businesses who can adapt quickly should the environment change, their decisions are likely better informed than the policymakers who may fail to adjust mandated reference points to new circumstances.

Thus, my results indicate the value of analyzing the adaptive decision-making strategies that people employ to respond strategically to their biases. Understanding the effectiveness and limits of these strategies may enable policymakers to better determine when external intervention is needed and what form it should take. It also may allow policy makers to identify interventions that are designed specifically to complement the behavioral strategies people use. For example, my study suggests the potential benefit of disclosure aimed at increasing market transparency, even when other market participants are not better informed. In specific domains, transparency would allow people to obtain information on others' choices in order that they can use them strategically to change their reference point and reduce regret. Standard disclosure proposals, by contrast, aim to provide people with information about costs and benefits of potential choices. Further research may be able discover additional behavioral strategies and identify complementary interventions that enhance the functioning of private-ordering.

VIII. Conclusion

Regret aversion biases individual decision–making across many domains. The bias can reduce welfare because people experience regret as a source of disutility; it also can deter individuals from making an optimal choice. I find that people can be aware when regret biases them against a better choice. They can respond by employing behavioral strategies to reduce anticipated regret over the choice they want to make. In some contexts, people can use or decide within institutions that reduce the bias, enabling people to make a rational choice (Arlen and Tontrup, 2015). In other contexts, people employ strategies that can enhance welfare, even though they do not debias.

I present evidence that people can use herding as a behavioral rational strategy to overcome the negative impact of regret aversion. My results show that herding allows people to shift their bias by using the choices of others as their reference point. Focusing on the optimal decision of an unbiased majority enables people to make the same choice with less regret. I find that people can herd strategically. They obtain information on others when they otherwise would be biased against their optimal choice. They block information when their own reference point is optimal to avoid the risk of being biased by others toward a suboptimal choice. When people are presented with multiple reference points, for example different groups of market participants make opposing choices, they strategically focus on the group that allows them to make the best choice.

My analysis expands the scope of responses to behavioral biases, opening up paths for both research and policy. My approach of identifying behaviorally rational strategies expands the scope of responses to behavioral biases, suggesting new paths for both research and policy. It differs from “nudging.” Nudges use external intervention and often operate behind the back of the decision maker. While they are designed to shift people’s bias towards a better choice, they have to generalize what this better choice is and generally cannot consider individual preferences or restrictions. By contrast, the strategies I identify enable people to shift their bias themselves towards the choice they personally prefer. People will employ these strategies only when regret aversion reduces their welfare and the strategy would improve their expected outcomes. My approach also differs from government interventions designed to debias. I find that debiasing is not necessary to improve choices. Herding does not eliminate regret aversion; instead, people shift their bias to favor their preferred choice.

Future research should provide more evidence to determine the domains where behavioral strategies can effectively substitute or complement government intervention to overcome behavioral anomalies like regret aversion.

Chapter 6 - Self-Nudging

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Abstract

Nudging interventions typically presume some asymmetry of sophistication and power between the choice architect and the nudged. But the nudged need not be relegated to a passive role. I present evidence that individuals have a capacity to counter their biases, and even to use them to their advantage. This capacity for Behavioral-Self-Management ("BSM") can allow them to act as the choice architects of their future-self.

In my study I provide participants with the autonomy to choose among a variety of loss- and gain-framed contracts that govern the terms under which they perform a real effort task. The results show that subjects strategically harness their own loss aversion to counter their present bias and significantly improve their performance.

The loss-framed contracts give individuals a tool they can use to *self-nudge*. This possibility of *self-nudging* should widen my perspective on biases. Biases can cause cognitive error and dampen motivation, but they can also be a valuable tool for individual decision making. And giving subjects the autonomy to choose their contract adds to the effectiveness of their BSM strategy. I show that subjects experience self-determination utility separate from performance benefits driven by better adjustment of work tasks to subjects' production functions. To demonstrate the policy relevance of my results I expand on an application of BSM strategies to retirement savings-plans, which I suggest may lift participation and savings rates at no additional cost.

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I. Introduction

A "nudge," as Richard Thaler and Cass Sunstein define it, is a change to "any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives." (Thaler and Sunstein 2008). Nudges are not mandates. And yet, nudging is linked to an asymmetry of sophistication and power between the nudger and the nudged. In the private sphere contract drafters impose their behavioral expertise on the nudged to extract desired choices or extra effort provision, whereas in public domains regulation may ideally steer the nudged towards their best interest or the public good (Kőszegi 2014). Either way the picture is one of expert choice architects relegating the nudged to a passive role. Those subject to the nudge are often understood to be neither self-aware enough to employ the behavioral tool without intervention nor sophisticated enough to see through the behavioral strategy or to escape it.

In this Article I advance a contrasting concept of "self-nudging" that aims at empowering decision-makers to manage and utilize their biases. I present evidence that individuals can have the sophistication to utilize their loss aversion to conquer their present bias, committing their future-self to a desired feat. I refer to this capacity as Behavioral-Self-Management (BSM) and it can allow individuals to act as the choice architects of their future-self.

Effective self-nudging requires behavioral sophistication: Individuals must be aware of the present bias or decisional errors they want to counter and they need to anticipate that rearranging their decision-making environment will enlist the bias they want to utilize. Providing for a commitment device by contrast typically only requires one to anticipate that one's future-self will respond to monetary incentives like the penalty or reward provided by a commitment contract (Ayres 2012).

The literature on commitment devices provides indicative evidence for such behavioral sophistication. Augenblick et al., 2015; Kauer 2012, Cadena 2015 suggest that individuals can be aware of their time-inconsistent preferences when facing laborious work tasks or long-term health decisions, i.e., individuals can sometimes anticipate that their future-self will place more weight on effort costs than they do presently.[117] Individuals also can be willing to self-impose commitment devices to conquer their present bias (Bryan et al 2010). In Trope and Fischbach (2000) individuals who must adhere to a sugar free diet to pass a medical test impose penalties on themselves should they fail the test. In Wertenbroich et al. (2002) students voluntarily select early submission deadlines their professor enforced to improve academic success in class. Ayres (2012) describes commitment contracts that require individuals to put money on the line that will be forfeited, should their future-self fail a prescribed task; for example in Gine et al (2010) smokers commit not to smoke for six months. However, the take-up rate of these so-called hard commitment devices—i.e., devices that effectually constrain future choices by providing for penalties or rewards—is very low: in Gine's study only 11% of the invited subjects took up the contract.

Softer commitment devices leave individuals more flexibility for opting out: Kast and Pomeranz (2009) describe a group-savings-mechanism in which individuals announce intended deposits to their peers utilizing social costs for self-commitment. Benartzi and Thaler (2004) leverage inertia, offering employees the opportunity to enroll in a retirement savings plan that automatically allocates future salary gains towards savings; people remain enrolled unless they opt out.[118] While

[117] Additional evidence that individuals anticipate time inconsistency can be found in Wong 2008, Schwartz et al. 2014, 2015, Royer et al. 2015.
[118] The plan avoids present bias by letting employees commit only for the future and uses inertia to keep them enrolled.

the evidence for deliberate self-commitment is still scarce (Laibson 2018) the available studies show that whereas hard commitment devices are often not used and lack the flexibility to respond to external shocks (Amador et al. 2006), softer devices are more readily picked up—more than 60% of the employees enrolled in the retirement savings plan, Benartzis and Thaler had designed. On the other hand, soft commitment devices require more deliberate inhibition of alluring impulses which can drain self-control resources and may fail when individuals' cognitive resources diminish (Baumeister et al.1998; Strack and Deutsch, 2004). Therefore often effectiveness is low, as in Anderberg et al. 2018, who test self-imposed yet not enforced deadlines in higher education.

Self-nudging is distinct from the use of commitment devices: it allows individuals to utilize their own cognitive or motivational biases to counter present bias or other decisional errors; in contrast to soft commitment devices, self-nudging relies on automatic processes: For example, loss aversion once triggered allows for resisting alluring temptations without drawing on deliberate self-control. Self-nudging also preserves individuals' flexibility as they can override the automatic process and discontinue a work task or opt out of a default plan at any time (see my suggestion for a self-nudging retirement savings plan in the policy section).

Empirical evidence of a deliberate strategic use of self-nudging is to my knowledge not yet available. The studies by Gine, Benartzi and Thaler show that subjects enter commitment contracts or savings plans because they expect to benefit from them, yet they do not reveal whether the participating individuals are aware of the nudges that are being employed. One experimental piece refers to self-nudging but their evidence for deliberate self-nudging is unclear. In Torma, et al. (2018) subjects can choose organic food in advance to improve eating habits, yet it appears that subjects employed a commitment device as opposed to employing a bias and an automatic process for self-nudging.

Other literature mentions self-nudging only in passing or, like Reijula and Hertwig (2021), discusses strategies for how individuals *could* self-nudge. These studies do not present experimental or field evidence showing that people engage in self-nudging and are sufficiently sophisticated to do so. Reijula and Hertwig (2021) for example suggest rearranging the refrigerator and other areas of the kitchen to support healthy eating patterns.

In this Article, I focus on contract design and aim to demonstrate the power and effectiveness of self-nudging contracts. Many nudging strategies in contract theory have been built around loss-framing (Hossain and List 2012, Fryer et al. 2012, Brooks et al. 2011, 2017); some of these strategies have also found their way into practical application (Alterbaum et al. 2016). For most people losses loom larger than gains. Work contracts can exploit this effect by promising employees a base-payment if they meet a specified productivity level. If their performance falls short, they face penalties and lose part or all of the payment they were promised.

Given the same monetary performance incentives individuals will then invest more effort in order to avoid losing their entitlement.

Instead of imposing loss-framed contracts on workers, I want to analyze in a set of experiments whether individuals can strategically employ their loss-aversion in nudging contracts to boost their performance. Under a standard behavioral model, a worker should not voluntarily choose a loss-framed over a gain-framed contract. Strictly rational workers should be indifferent. If a worker is loss averse on the other hand, then a gain-framed contract leads her to a higher net outcome, as gain-loss utility below the loss frame's threshold is negative. Empirical findings do not suggest a clear pattern: Luft (1994) concludes from survey evidence that workers resent being exposed to losses and prefer gain-framed contracts. Brooks et al. (2017) find lower acceptance rates for loss-framed contracts when performance thresholds are high, but no difference otherwise. Quidt (2017) and Imas et al. (2017) show results suggesting that workers may prefer loss-framed contracts. Quidt however attributes this to cognitive error, while Imas et al. suggest subjects may use the loss-framed contract for commitment, but their evidence is also consistent with the notion that subjects choose the loss frame because it rewards their work before performance.[119]

None of these earlier studies relates workers' contract choices to their actual performance expectations to rationally explain workers' motivation for choosing a loss-framed contract. Workers may rationally choose a loss frame if they expect it will lead them to a higher net-outcome. This can be the case when they anticipate that their future-self will invest less effort in a work task than they prefer; that is, that their work preferences are *inconsistent* over time. As the loss-framed contract increases the cost their future-self incurs should it fall short of the performance they desire, loss framing can be a rational response to their present bias (Imas et al. 2017). Individuals have a demand for self-nudging if they expect that the benefits from higher performance will outweigh the costs of loss framing (negative gain-loss utility). Then they may voluntarily choose a loss-framed contract.

To analyze my BSM theory that subjects leverage their loss aversion for nudging themselves to a better performance I developed a novel study design. I offer participants a menu of contracts to choose among. All of these contracts offer subjects the opportunity to perform an effort task and promise them a €1 payment for each task they complete. While the payment is piece-rate in all contracts, the framing of the contracts varies.

Three contracts are loss-framed and specify thresholds of 5, 15 or 50 tasks subjects have to complete, and promise accordingly €5, €15, €50 for fulfilling the contract and reaching the threshold. If subjects exceed the threshold, they are paid a bonus of €1 for each task they solve beyond the threshold; conversely, a penalty is deducted from their promised base payment of one €1 for each task they fall short.

[119] In the Imas et al. study subjects are competing over a non-monetary good. Receiving it in advance may thus also have the advantage that subjects can inspect it to find out whether it is worth to raise their effort to win the competition.

One contract is gain-framed and neither specifies a number of tasks that must be completed nor promises a base payment. Importantly, I elicit subjects' performance expectations for each contract they can choose among, such that I can relate their expectations with their contract choices. Also I measure their loss aversion—the bias I expect them to capitalize on.

My results suggest that subjects use a rational self-nudging strategy. Most of the subjects select the contract they expect will nudge their future-self to its optimal performance. Subjects who are more strongly averse to losses can harness their bias more effectively: these subjects choose higher thresholds, expect to increase their productivity significantly more and perform significantly better than participants who are less or not loss-averse. That more strongly loss-averse subjects choose deeper loss frames suggests that subjects are aware of the strength of their bias and consider its impact on their future-self when selecting a contract. For almost all subjects the self-nudging strategy is effective and they comply with the threshold of the contract they selected, making them more productive than subjects who were not given a self-nudging option.

I assume that one key element in the effectiveness of self-nudging is that it preserves the autonomy of the decision-maker in an even stricter sense than nudging. BSM makes the decision-maker her own choice architect to guide her future-self's choices. Externally-imposed nudges are vulnerable to a number of objections that do not apply to self-nudging. First, while nudging is often associated with a preservation of autonomy compared to a mandating intervention, the more effective the nudge, the less will people act upon their own preferences. and the more the choice architect effectively takes over (Bubb and Pildes 2014b). Loss-framing illustrates the risks that externally-imposed nudges may backfire when they override preferences. If the contractor sets the loss frame too deep, then workers derive less utility from the same performance. The loss frame reduces their effort instead of pushing it up (Kahneman and Tversky 1979, Kahneman et al. 1991). A threshold set too low also dampens performance, communicating a social norm of low productivity that workers conform to (Brooks et al. 2017).

BSM by contrast respects the decision-maker's preferences, making the individual her own choice architect to guide her future-self's choices. My first experiment suggests that this pays off. In my second study I want to demonstrate that the autonomy that BSM entails adds to the efficacy of strategies like self-nudging, and opens the possibility that by providing choice, BSM strategies can enhance performance beyond simply matching the task to an individual's preferences.

I want to distinguish two performance-enhancing effects that this autonomy of self-managing may cause. First, autonomy enhances productivity by allowing workers to select or adjust tasks according to their personal preference—I refer to this effect as "preference-matching." Second, the experience of autonomy in exercising a task may also foster intrinsic motivation, further boosting performance. I coin this effect the "Autonomy Premium." To my knowledge this study is the first to establish an Autonomy Premium and separate it from preference-matching. Earlier studies have restricted subjects both in their ability to choose according to own preferences and in their experience of choice autonomy (for example Dickinson et al. 2008, Moller et al. 2006 and Falk et al. 2006a). The design of these studies therefore does not allow the authors to identify what has caused the positive effect on productivity they observe: is it that subjects can select the task best fitting their preferences? Or does the experience of self-determination utility independently raise subjects' effort?

Isolating an Autonomy Premium has important implications for work contracts. While some firms can collect and analyze data that allow them to track employees and learn how to best match their production functions, an Autonomy Premium would elevate performance only when workers are in fact granted choice autonomy and experience self-determination utility. So even if companies adjust performance thresholds optimally, preserving or granting work autonomy could further improve work effort.

To isolate the Autonomy Premium, I have developed a novel identification strategy: In the *Autonomy* treatment subjects can choose among the same three loss-framed contracts I used in my first study. The contracts establish thresholds that require subjects to complete 5, 15 or 50 tasks in order to fulfill the contract. Payment is piece rate and subjects receive bonuses for exceeding and penalties for falling short of the thresholds. In the *Preference Matching* treatment subjects are presented with the same three loss-framed contracts and are incentivized to reveal which of the contracts they prefer. Then subjects have to pick one of the three contracts blindly in a random assignment. Comparing these two treatments allows us to isolate the two major effects of choice autonomy I had predicted: first, subjects perform better when the contract they enter and work under meets their preferences. Second: subjects are significantly more productive when they have the freedom to select the contract they prefer themselves—this is the Autonomy Premium.

I conclude that self-nudging not only improves productivity by enlisting subjects' loss aversion to commit their future-self to a targeted performance and by enabling individuals to select tasks fitting their personal ambitions and capacities. In addition, the experience of self-determination utility also fosters intrinsic motivation when performing a task. My data show that this extra motivation increases participation and acceptance rates significantly beyond the rates reported for the commitment contracts offered by Gine and Ayres or even the soft-enrollment in retirement savings plans suggested by Benartzi and Thaler.

Finally, the economic value of the BSM-enabling contract design I propose is substantial: To incentivize subjects to produce the same performance under a plain vanilla gain-framed incentive contract (i.e., a contract that provides no opportunity for self-nudging) I would have had to pay twice the amount for any given unit of work for subjects to complete. I suggest the comparison of what standard economic incentive would be needed to achieve the same change in behavior as a general approach to rationally judge the behavioral strength of a nudge or a self-nudge.

My findings have broad implications for both the design of private market institutions and for public policy. To demonstrate the practical applicability of my work I will expand in the final section of the paper on one significant policy implication, the design of retirement savings plans. The extent to which individuals can act as managers of their own biases is an open empirical question. BSM strategies require a significant degree of behavioral awareness and sophistication. Future research must explore other domains and report how commonly BSM strategies are employed and what institutional support individuals may need to use them effectively. Earlier work has made first steps in this direction: Arlen and Tontrup (2015a) analyze the self-debiasing of property owners in a principal-agent game. Arlen and Tontrup (2015b) show that owners strategically relocate their bias such that it pushes them to trade rather than keeping them from a beneficial transaction.

I propose BSM as a research program and, as future findings may advise, as a practically valuable instrument in the behavioral toolbox of regulators, to complement and sometimes substitute for externally-imposed interventions.

II. Experiment 1: Utilizing Loss Aversion for Self-Nudging

My first experiment assumes that individuals have the capacity to use loss-framed contracts for self-nudging. I want to show that they are self-aware of their loss aversion and time inconsistent preferences and that they deliberately leverage their loss aversion as a tool to counter their present bias and reach better outcomes.

To reach this goal I let subjects solve an effort task that is tedious but also well incentivized. A subject's current-self should be interested in maximizing earnings, while anticipating that her future-self might discontinue the tedious task early due to present bias. I will explain the effort task below.

The task is governed by a contract and subjects are offered one gain-framed contract and three loss-framed contracts to choose among. Monetary payment is all the same. I expect subjects to pick a loss-framed contract if they anticipate their present bias. The loss frames of the three contracts differ in size, such that subjects can decide the performance level to which they want to push their future-self.

Loss aversion varies between individuals. I use this property to analyze whether subjects are indeed aware of their loss aversion and deliberately exploit it. More strongly loss-averse subjects hold a stronger self-nudging device in their hands. If more strongly loss-averse subjects select deeper loss frames—that is higher thresholds—compared with the less loss-averse subjects, I can conclude that they indeed consider the strength of their loss aversion when they choose a contract and expect it to impact their future-self.

I also want to show that more strongly loss-averse subjects expect their future-self to reach a better performance, and finally that their self-nudging is more effective, leading to the expected higher productivity. I will explain below how I measure subjects' expectation regarding their future performance, and how I incentivize subjects to reveal their true expectation.

A. Experimental Design

Real Effort Task. My experimental design offered subjects the opportunity to enter into a contract obliging them to complete an effort task. The task presented participants with tables containing digits between 1 and 9. Subjects were asked to count how often a specified digit appeared in the table. After each table they completed successfully, they could decide whether to continue with the next table or to stop the task. Subjects must repeat the same task if their answer is incorrect. Participants must wait 15 seconds before they can make a new entry following an incorrect input. As it takes participants around one minute to count a table, the buffer between entries makes guessing time-inefficient. When subjects enter a correct answer, they can decide whether or not they want to continue the task. Participants are free to reject the contract, in which case, they receive no payment. To ensure that subjects focus on the experimental task I implement a time limit. Participants are informed that if they fail to make an input after 180 seconds or if they log out, I exclude them from the experiment without payment.

Contract Design. The terms under which an individual subject performs this effort task are set out in one of several gain- and loss-framed contracts. The Standard contract is gain-framed and does not stipulate a performance threshold. All other contracts are loss-framed and set out a threshold. I offer subjects a choice among the three loss-framed contracts (Low Bar, Stretching and Extreme Effort) and the gain-framed Standard contract.

As a baseline and for attuning the thresholds I first had participants work under the plain vanilla Standard contract that did not express a threshold. Participants were not promised any base payment and received 1€ for each task they finished counting. Subjects completed an average of 10.4 tasks. Based on this performance, I calibrated the three loss-framed contracts. The Low Bar contract offers a threshold of 5 tasks. The Stretching contract expresses a threshold of 15 tasks. Finally, I offered subjects a contract that stipulated an Extreme Effort threshold of 50 tasks, obliging subjects to invest approximately five times the effort they had exerted, on average, under the Standard contract.

To enter one of the loss-framed contracts subjects must pay a fee: 20 cents for the Low Bar contract, 60 cents for the Stretching contract, and €1 for the Extreme Effort contract. Entering the Standard contract, by contrast, is costless. The increasing cost of the loss-framed contracts ensure that the subjects do not pick a contract at random.

Table 1: Overview of Contract Types

Contract Types	*Threshold/ Promised Payment*	*Framing*
Standard Contract	None	Gain
Low Bar	5	Loss
Stretching	15	Loss
Extreme Effort	50	Loss

The threshold divides incentives into a gain frame above the threshold and a loss frame below the threshold. For example, the Low Bar contract set out a threshold of 5 tables. The promised base payment for fulfilling the contract is 5€. For exceeding the threshold subjects are paid a bonus of 1€ for each table they complete above the threshold and a penalty for each table they fall short of the threshold. To install the loss frame and induce loss aversion, subjects are promised the base payment the contract stipulates by setting out the threshold. A similar combined loss-gain framing with promised payment is also used in Armantier and Boley (2015) and Brooks et al. 2017. DellaVigna et al. (2018), Hannan et al. (2005) use a pure loss framing (no incentives above thresholds). All studies report significant performance gains under the loss frame.

The framing of incentives as penalties (losses) and bonuses may directly induce a reference point that leads subjects to experience outcomes below the reference point as losses and outcomes above the reference points as gains. According to Köszegi and Rabin 2006 the reference point may also be expectation-based. If the contract's framing induces subjects to form the expectation that they will meet the threshold, they will experience falling short of this expectation as a loss. Camerer et al. (1997) report the example of New York cab drivers, who form outcome expectations for a given work day, with the result that they work longer when that expectation is higher (for example, on high cab usage days like Halloween) and less when their expectation is lower. Abeler et al. (2011) demonstrate expectation-based reference dependence by randomly manipulating subjects' expectations in a work task. For my study the difference is only marginal. I elicit subjects work expectation and they barely differ from the stipulated threshold.

I chose the loss-gain contract design because it offers three advantages. First, it allowed us to hold monetary incentives constant for all contracts. That is, the contracts vary the performance thresholds (5, 15, 50), but the payment for the same performance remains the same no matter what threshold is defined. Subjects received the same net payment of €1 per table completed under all contract types. For example, in the Low Bar contract subjects were promised a payment of €5, while in the Extreme Effort treatment they were promised €50. Participants who fell short of the threshold faced a penalty of €1 for each task they fell short. When they exceeded the threshold, they received a bonus of €1 per task. The effective net payment, therefore, was identical under each contract for the same level of performance. For example, a participant who completed 12 tasks was promised under the Low Bar contract a base payment of €5 and received a bonus of €7; under the Extreme Effort contract she was promised a base payment of €50, but had to pay a penalty of €38 for falling short of the threshold.

The second advantage of the design is that it prevents a situation, created by pure loss-framing, wherein workers have high-powered incentives below the contractual threshold but only weak or no incentives to perform above. Using a loss-gain contract design avoids this disincentive to performance above the threshold and leads to a much stronger performance overall (Brooks et al. 2017).

Finally, the design allows us to rule out alternative explanations for my findings, as some behavioral effects could also push performance up yet only to the chosen threshold not beyond. I will explain this in detail in Experiment 2.

Performance Expectations. I elicit subjects' performance expectations for each of the four contracts. The order in which the contracts are presented when participants indicate the number of tasks they expect to complete under each is randomized. The design incentivizes subjects to reveal their true expectations. Because the incentives may affect performance, I apply them only to a randomly selected group of one in five subjects. Participants are instructed that the selected subjects will be randomly assigned one of the four contracts and that they are paid according to their actual performance under the assigned contract, plus a bonus of 10 cents for each table they complete on top of meeting their performance expectations. If they fall short of their expectations, they get no additional payment. Participants learn whether they are selected or not only after they have indicated their expectations.

This design gives all subjects a monetary incentive to reveal their true performance expectations, while it does not affect the actual performance incentives of the subjects who are not selected and who are part of my sample. All participants who are not selected can choose between the four contracts and perform the task under the contract they prefer; they are paid for their performance, but they are not paid for the expectation measure. Not to confound my analysis I exclude all selected subjects who were randomly assigned a contract and received the bonus pay if they met the expectations they indicated.

Loss Aversion Measure. The claim I analyze in the Self-Nudging experiment is that people improve their productivity by utilizing their loss aversion as a debiasing and self-nudging device. To support this claim, I exploit that loss aversion varies across subjects. People can be more or less loss-averse; some might be neutral. Subjects who are more strongly averse to losses should be better equipped to utilize their loss aversion than less loss-averse subjects; the more loss-averse subjects simply have available to them a stronger self-nudging instrument. To test this claim I distinguish groups of subjects in terms of their relative loss aversion. This allows us to compare contract choices, performance expectations, and productivity across these groups and attribute differences to their relative propensity of loss aversion (Kahn, Luce and Nowlis 2006).

To estimate subjects' loss aversion, I use a separate incentivized tool that has been used and published in diverse fields such as economics, psychology and the law. The measure was developed by Fehr and Götte (2007) and then taken up by other authors, sometimes with small modifications depending on task and research question like adapting gains and losses of the lotteries or adding a third lottery (Abeler et al, 2011; Bibby and Ferguson, 2011; Brooks et al. 2012, 2017; Fehr et. al. 2013; Gächter et al. 2021; Mrkva et al., 2020). In its original version that I use, the measure gives subjects the opportunity to participate in two lotteries. The first lottery offers subjects a 0.5 chance of winning €8 and the same probability of losing €5. The second lottery presents the exact same payoffs, but the lottery is repeated six times and subjects are paid the average outcome of the six draws. Therefore, subjects in the second lottery face a lower risk of suffering an overall loss. A strictly rational player should accept both the first and the second lottery, as the expected net outcome of both is positive. A loss-averse player may reject the first lottery or even both lotteries.

For non-parametric testing I classify participants into two categories: I collapse subjects who accepted either one or both of the lotteries into one category (1) the "loss-averse" and distinguish them from my category (2), the "less loss-averse or not loss-averse" subjects who participate in both lotteries. I use this classification because data show that many subjects, even though they understand that in both lotteries, chances are higher they will increase their earnings when participating, are nonetheless averse to losing all their potential earnings; therefore they decide to accept only one lottery but not the other. This strategy differs in respect to loss aversion from subjects who are willing to accept both lotteries to improve their earnings. For my regressions I use all information the measure provides and implement a three-step classification reflecting increasing loss aversion (subjects who accept both lotteries=0; subjects who accept one lottery=1; subjects who accept no lottery=2).

Note, it is entirely possible that subjects who accept both lotteries nonetheless manifest some degree of loss aversion. For them loss-gain utility may simply count for less than the 3€ difference between the losses and gains the lotteries offer. This needs to be considered for understanding my results. To support the robustness of my findings I will also test whether results vary when I employ a differently designed measure that offers subjects six mixed gain-loss lotteries that hold a gain of €6 constant in each while increasing the potential loss in steps of €1 up to €7. Gains and losses occur with chance 0.5.

B. Procedure

Recruitment. Participants are either current students at or graduates of the University of Münster in Germany. Participants are studying or have studied in the full range of academic departments across the university. About 30% of the sample has graduated and is employed outside of the University. I recruited participants via the University's central email server. I sent an invitation email containing a link to the study. The link became inactive once used, ensuring that participants could not complete the study more than once. The invitation informed potential participants about the time it would require them to complete the experiment, to minimize the number of subjects who discontinue participation due to an underestimation of the time they would need to finish the study.

Online Implementation. I implemented the experiment online to impose real opportunity costs on the participants—a feature central to my experimental design. Conducting the study online means that subjects are free to discontinue the effort task whenever their opportunity costs exceed their expected benefit from performance, without being influenced by (or influencing) the behavior of other subjects. In contrast, if I conducted the study in a laboratory and participants were permitted to leave after stopping their performance, the decision of a subject to discontinue would likely affect the effort and motivation of the subjects still working on the task. On the other hand, if participants would have to wait in the laboratory until all subjects finished the task or decided not to continue, they would have barely any opportunity costs; they may finish all tasks as the best use of the time they were obliged to be in the lab, making the study insensitive for differences between subjects' levels of motivation.

In addition, the online platform reduces participation costs unrelated to task performance and thereby allows us to sensitively measure contract acceptance rates. In a laboratory study by contrast, subjects will likely feel constrained to enter the contract in order to recover their travel and participation costs.

Control Questions. I use a set of control questions (the control questions are reported in the Supplemental Materials in Chapter 7) to test participants' understanding of the task and of the incentives provided by the contracts. Subjects are asked to calculate their payoff for different performance levels under all contracts they are offered. Participants are permitted to proceed with the study only if they answer the control questions correctly. All but two subjects passed this hurdle; in particular, the control questions demonstrate that subjects understand that the alternative thresholds of the contracts are a framing device and do not affect their monetary payment per task.

Demographics. I obtained age and gender information. No age or gender differences were predicted and I did not find any correlations to other variables in my regressions. Demographics are presented with the regression results.

C. Theory and Predictions

1. Loss and Norm Framing

Loss framing exploits the fact that payments can be offered as gains or losses, at equivalent monetary costs. According to Prospect Theory, individuals assess utility relative to a reference point (Kahneman and Tversky 1979, Kahneman et al. 1991; Kőszegi and Rabin 2006). Outcomes below this reference point are experienced as losses and outcomes above as gains. To account for this reference dependence, I assume in my study that subjects derive independent gain-loss utility from their performance. While consumption utility remains constant, gain-loss utility is assumed to be negative below the reference point and positive above.

Loss framing can increase effort because reference-dependence is asymmetric: i.e., for most people, losses loom larger than gains. Assume an individual either falls short of the reference point or exceeds the reference point by the same margin, then the negative gain-loss utility from not reaching the reference point is larger than the positive benefit from exceeding it. Thus, loss framing increases the costs of falling short of a performance norm and induces loss-averse individuals to work harder. As a result, by framing payments as losses, firms may push worker motivation and performance without spending more on financial incentives (Hossain and List 2012).

Empirical evidence supports this effect. Hossain and List present field evidence from Chinese factory workers, who lift their productivity under loss-framed contracts. Fryer et al. (2012) vary the incentives for teachers, and find that loss-framed contracts increase teaching performance. Armantier et al. (2015), and Brooks, Stremitzer and Tontrup (2011, 2017) present laboratory evidence showing that loss-framed contracts can push performance higher. In the experiments reported in this article, my contract treatments are designed to shift the reference point; by varying performance thresholds and base-payment they establish loss frames of different sizes: the *Low Bar* treatment suggests a reference point of five tasks, the *Stretching* contract one of 15 tasks, and the *Extreme Effort* contract one of 50 tasks.

But loss framing is risky. It can depress productivity if thresholds are not well-adjusted (Brooks, Stremitzer, Tontrup 2017). The more distant perceived losses are from people's reference points, the less gain-loss utility they carry (Kahneman and Tversky 1979, Kahneman et al. 1991). And the less value people place on gains and losses, the smaller the motivating effect of performance incentives on effort choices. Thus, when contract drafters push thresholds too high, the loss frame may reduce, rather than spur work effort. A threshold set too low is likely also to dampen performance, because it communicates the (prescriptive or social) norm that matching the threshold is a sufficient performance. When social desirability of continuing performance diminishes, workers who reach the threshold are more likely to stop their performance compared to when no norm was specified. That is, workers conform to the perceived social norm by lowering their productivity (Brooks, Stremitzer, Tontrup 2017). A contract drafter's attempt to exploit loss aversion may thus backfire, which may explain why loss-framed contracts have not entered business practices on a larger scale (as suggested by Hossain and List 2012) even though they promise better performance without extra cost.

2. Why Choose a Loss-Framed Contract?

As a benchmark for my BSM-enabling contract experiments I conducted treatments in which subjects were presented with just one of the basic contracts without giving them a choice. So I conducted a *Standard* contract treatment, as well as *Low Bar*, *Stretching* and *Extreme Effort* treatments, and measured performance and contract acceptance as estimates of productivity under each of these.

In contrast, my first two experiments provide subjects with a choice between these basic contracts. Experiment One analyzes subjects' motivation to choose a loss-framed contract by linking subjects' contract choices to their performance expectations.

I want to show that a BSM-enabling contract that preserves autonomy and offers workers a choice between loss-framed and gain-framed contracts will often be more effective than a contract design that exogenously tries to nudge workers to an optimal performance.

The BSM contract allows workers to improve their performance both by taking advantage of their own loss aversion, and by deriving self-determination utility from an autonomous performance. It also eliminates the risks of a bad match of the working task the contract specifies and the workers' production functions. But why would workers deliberately select a loss-framed contract? Standard economic theory suggests that workers should if anything prefer a gain-framed over a loss-framed contract. While a strictly rational individual should be indifferent about the framing, a loss-averse individual earns a lower net outcome under a loss-framed contract. As gain-loss utility is negative below the threshold, gain-loss utility increases loss-averse workers' net-consumption utility only once their performance exceeds the threshold (O'Donoghue et al. 1999, Imas et al. 2017). In contrast, under a gain-framed contract, assuming that workers take the status quo as their reference point, gain-loss utility is positive from the beginning of performance. Accordingly, individuals should choose the flattest loss frame (if loss-averse) or be indifferent (if fully rational), when presented with a choice between contracts offering differently-sized loss frames. For loss-averse workers, the deeper the loss frame, the less gain-loss utility and net-consumption utility they will earn.

I propose instead that loss-framed contracts may be attractive to loss-averse workers as a self-nudging device if those workers expect that their effort preferences are likely to be inconsistent over time. To see how loss framing can serve as a self-nudging device, consider a subject who would like to earn a payoff of €15 before she begins to work. She expects her future-self—i.e., herself when actually completing the effort task—to place a relatively higher weight on the cost of effort, leading her future-self to choose a lower level of performance worth only €10. By entering a loss-framed Stretching contract she can make it costlier for her future-self to fall short of her performance goal of €15. As negative gain-loss utility looms larger, the future-self will derive a higher marginal benefit from the effort she invests below the threshold. The future-self's optimal effort and expected monetary payoff increase accordingly. Thus, when subjects expect their future-self to choose a productivity level below their goal, placing a higher weight on effort costs, then entering a loss-framed contract is a sophisticated response. On the other hand, individuals who expect their preferences to be consistent at t0 when entering the contract and at t1 when their future-self works, have no need for a commitment device; they are (all else equal) better off with a gain-framed contract.

I can also specify the conditions under which subjects should expect to produce their best performance under a loss-framed contract. First, the worker anticipates that she will have time-inconsistent preferences. Second, the subject must anticipate that she will be sufficiently loss-averse to benefit from contract framing. And finally, she must expect the loss frame to improve performance and payoff by so much that net consumption utility is positive; that is, the payoff increase due to an improved performance must outweigh the costs of loss framing (the subject's negative gain-loss utility plus the fee for entering the loss-framed contract). This is most likely the case if subjects expect to satisfy the loss-framed contract, in order to avoid the experience of a loss. Notice that the optimal level of self-commitment is conditioned on the production function of the future-self. If subjects overstretch their future-self, they will suffer a loss. If the performance gain does not make up in payoff for the costs of loss framing, then the loss-framed contract effectively lowers net consumption utility.

I therefore assume participants will typically optimize their net consumption utility by choosing a contract they expect to comply with. I measure this compliance directly by comparing subjects' actual performance with the contract threshold they chose and I find that 84.8% of the subjects indeed choose a contract they comply with. Also subjects choose the threshold contracts to improve their productivity: under the loss frame they expect to reach a performance that exceeds their expectation for the alternative gain-framed contract by 9.9 tasks, which should compensate them for negative gain-loss utility. Indeed, as most subjects entering a loss-framed contract comply with the chosen contract, they manage to match the expected gain by solving on average even half a task more than they expected.

Now I want to demonstrate that subjects achieve these performance gains by deliberately utilizing their loss aversion for self-nudging. For this purpose, I will exploit the variation in levels of loss aversion across subjects.

3. Hypotheses

I assume that subjects with some degree of loss aversion and a demand for self-commitment can capitalize on their bias through self-nudging and improve their performance by choosing a loss-framed contract. Recall that subjects who wish to select a loss-framed contract must pay a fee and will earn a lower net-outcome for investing equal effort: this ensures that they will choose a loss-framed contract only when they expect to benefit from this choice. Since the more strongly loss-averse subjects should be better prepared to utilize their loss aversion for self-nudging, I hypothesize that the group of more profoundly loss-averse subjects will be significantly more likely to select a loss-framed contract than the participants I classified as being less or not loss-averse ($H_{1.1a}$). I assume that subjects anticipate the strength of their loss aversion such that the more strongly loss-averse subjects can push their future-self's effort provision more effectively. I therefore expect them to choose contracts with higher thresholds that expand deeper loss frames than the less or not loss-averse subjects ($H_{1.1b}$).

Performance expectations should follow the same pattern. I assume that the subjects anticipate the strength of their loss aversion as a self-nudging tool and therefore hypothesize that the more strongly loss-averse subjects expect to push their future-self harder to reach a higher level of productivity than the group of less or not loss-averse participants ($H_{1.2}$).

Further, I assume that subjects use self-nudging successfully, leading to better outcomes for those better equipped to utilize their loss aversion as a self-nudging tool. I predict in line with subjects' expectations that the more strongly loss-averse subjects will perform significantly better relative to subjects who are less or not loss-averse ($H_{1.3}$).

The *Autonomy* and *Preference-Matching* treatments offer subjects a choice between the three loss-framed contracts as well. Therefore, I expect that the more strongly loss-averse subjects should more effectively take advantage of loss framing and perform better than the group of subjects less or not loss-averse in these two treatments as well ($H_{1.4a,\,b}$).

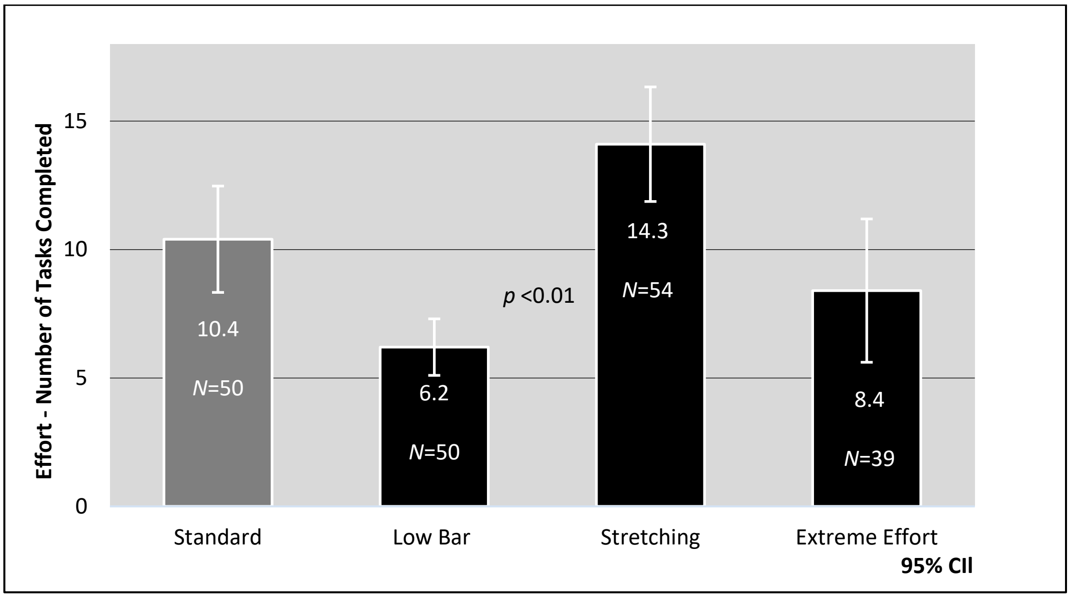
Finally, as subjects can choose a performance threshold and the size of loss frame that matches their personal level of loss aversion, I assume self-nudging to be a more effective than the exogenously assigned loss-framed contracts, which suppress performance if loss frames are either set too low or too high. I therefore expect subjects in *Self-Nudging* to outperform participants in all single threshold contract treatments ($H_{1.5}$).

D. Results Self-Nudging

First, I want to provide a benchmark for the effectiveness of my Self-Nudging experiment. As a benchmark I use subjects' performance when they are offered only one contract—i.e., the gain-framed *Standard* contract or one of the three loss-framed contracts. I find that the performance results under the basic contracts are U-shaped (see Figure 1): Under the *Low Bar* contract, subjects solve 6.2 tasks; under the *Stretching* contract performance rises to 14.3 tasks; while under the *Extreme Effort* contract production declines to a mean result of only 8.4 tasks. This outcome reflects the two risks of loss framing—diminishing sensitivity and norm framing—such that performance under both the *Low Bar* and the *Extreme Effort* loss-framed contracts is significantly lower than under the plain gain-framed *Standard* contract with 10.4 tasks. Only the *Stretching* contract outperforms the *Standard* contract; it also leads to a significantly higher performance than *Low Bar* and *Extreme Effort* contracts (for all comparisons

Mann-Whitney *p*-value <0.01). The results remain the same when I account for participants who reject the contract they are offered, treating these subjects as having exerted zero effort (compare Appendix A Figures A1 and A2).

Figure 1: Risks of loss framing



The data demonstrate that exogenously imposing loss frames on workers creates a substantial risk; if the loss frame is not well adjusted, loss framing can be harmful and lead to lower productivity than a plain (gain-framed) *Standard* contract (see Brooks, Stremitzer, Tontrup et al. 2017).

1. Contract Choices

Now, I analyze the data of my Self-Nudging experiment. First, I provide some descriptive data to better illustrate subjects' contract choices. I observe that contrary to economic theory's prediction a significant majority of subjects opts deliberately for a loss frame, in spite of the extra fee subjects had to pay for making that choice (77.5%; 117 of 151; Binomial test p-value <0.01). In total, 35 subjects choose the *Low Bar* contract (23.2%), 60 subjects pick a Stretching contract (39.7%) and 33 choose the even more costly *Extreme Effort* contract (21.9%). 34 subjects avoid the fee picking a *Standard* contract (22.5%).

For non-parametric testing I classify subjects in regard to their loss-aversion into two types, individuals who are more averse to losses (i.e. subjects who rejected either both or one lottery) and individuals less or not loss-averse (subjects who accepted both lotteries). In line with my prediction in $H_{1.1a}$, the more strongly loss-averse subjects were significantly more likely to select a loss-framed contract than the less or not loss-averse group of subjects (89.3%;

67/75 subjects versus 67.1%; 25/76; Fisher 2x2 *p*-value <0.01). Next, I distinguish the contract choices by contract type (*Standard*, *Low Bar*, *Stretching* or *Extreme Effort*) and again compare the two groups of more strongly loss-averse versus less or not loss-averse subjects. I had hypothesized in $H_{1.1b}$ that the more strongly loss-averse subjects, aware of the strength of their personal self-nudging tool, would also choose higher threshold contracts and indeed, my data support this result: I find that the more strongly loss-averse participants choose significantly deeper loss-framed contracts (Fisher test 2x4 *p*-value <0.01).

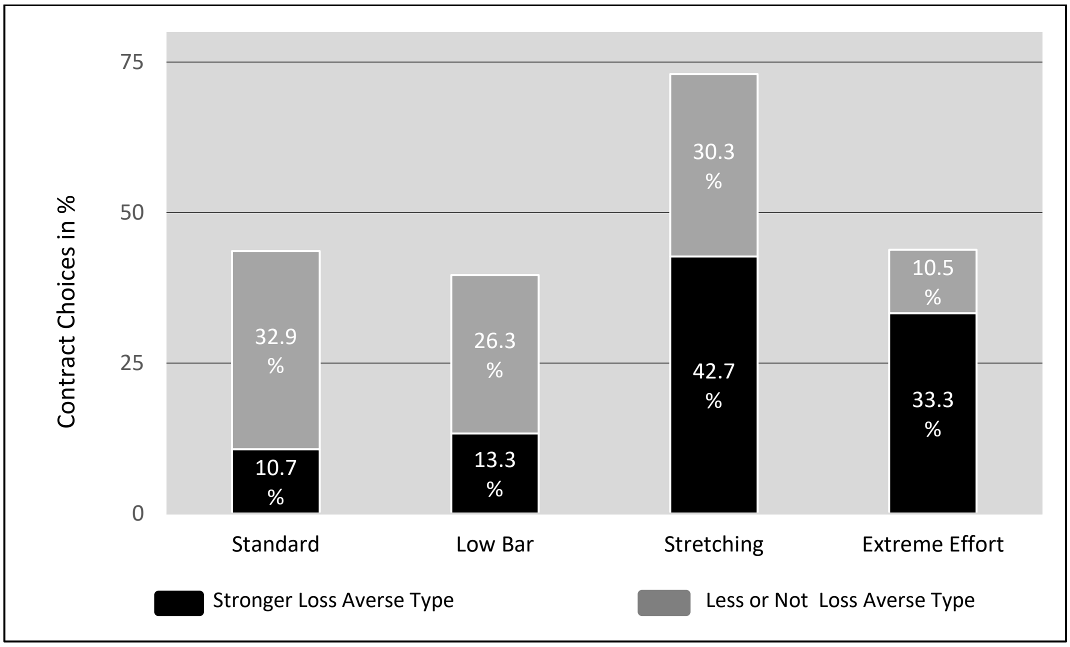
To confirm the robustness of my findings I use a three-step type classification that captures increasing loss aversion (subjects who accept both lotteries=0; subjects who reject one lottery=1; subjects who reject both lotteries=2). I first perform a logistic regression with chosen contract type as dependent variable, coding contracts as either being loss- or gain-framed. I include variables for demographics, contract acceptance and for estimating the main effect loss aversion type. The results show that as their loss aversion increases subjects are increasingly more likely to enter a loss-framed contract than the gain-framed contract (reg beta=0.815 *p*-value <0.01; reported as Logistic_1 in Table 2).

Second, I conduct an ordered logistic regression to consider all four contract types in my dependent variable. I include the same variables as in the binary logistic regression reported above, in particular I use the 3-step loss aversion measure. Results show that the more strongly loss-averse the subjects are, the more likely are they to choose a deeper loss-framed contract (reg beta=1.387 *p*-value <0.01). When I estimate the marginal effects for each contract category I find that with an increase in loss aversion by one unit, the subjects get (1) less likely to choose a *Standard* contract (dy/dx-0.110; p-value <0.01), (2) and get less likely to choose a *Low Bar* contract (dy/dx -0.060; *p*-value <0.01), while they get (3) more likely to choose a *Stretching* contract (dy/dx 0.055; *p*-value =0.02) and more likely to choose an *Extreme Effort* contract (dy/dx 0.115; *p*-value <0.01).

The results fit my theory: the *Standard* contract does not provide a self-nudging option and the Low Bar contract with its low threshold is not a useful tool for self-nudging and improving one's productivity. For both the *Stretching* and the *Extreme Effort* contract this is different and these contracts I see subject choose more often with increasing loss aversion; i.e., the stronger their nudge, the harder can and do subjects push their future-self. The results underline, that subjects appear to take the expected strength of their nudge into account when choosing a contract.

The data show also that many of the subjects who accepted both lotteries choose one of the loss-framed contracts. This might seem inconsistent with my theory that subjects choose loss-framed contracts to utilize their loss aversion for nudging themselves to a better performance. Yet, it is important to recognize that the loss aversion measure does not distinguish whether subjects who reject both lotteries are simply less loss-averse than the participants who accept one or two lotteries or are not averse to the losses at stake at all. For my research question it is not necessary to identify subjects who are not loss-averse and distinguish them from subjects who are (if that was possible). Those who are more profoundly loss-averse, I hypothesize, should be better equipped to utilize their loss aversion as a self-nudging tool. To provide this evidence it is only necessary to show that the subjects who are more strongly averse to losses also stronger engage in self-nudging, choose deeper loss-framed contracts, expect to reach a higher productivity, and in fact perform better than subjects with a lower degree of loss aversion. Therefore, I elicit and compare only relative levels of loss aversion, but do not identify subjects who are not at all averse to losses. As a consequence, participants who accept both lotteries in my study may still manifest and utilize their loss aversion, just not as effectively.

Figure 2: Contract choices by contract and loss aversion type



Some of the subjects may also leverage the moral costs they would sustain from contract breach, feeling guilty over letting down their contractual partner (Ederer and Stremitzer 2017; Vanberg 2008). While I discuss this possibility in the discussion section in detail, note that it does not confound my findings. Guilt aversion or the feeling of moral bond cannot explain why more strongly loss-averse subjects opt for deeper loss-framed contracts, expect to perform better, or raise their productivity higher, unless these moral motivations would be correlated with loss aversion. But there is no suggestion in the literature and it does not seem plausible that the mental mechanisms behind loss aversion and guilt aversion are related.

2. Performance Expectations

I have asked subjects to indicate the effort they expect their future-self to exert under each of the four contracts they are offered (*Low Bar*, *Stretching*, *Extreme Effort* and *Standard*). Before I analyze my hypothesis, I provide some descriptive data about subjects' performance expectations. A majority of subjects (75.9%; N=44; p-value <0.01) expect to perform better under a threshold contract, while a quarter (14/56; 24.1%) of the subjects expect to perform equally good or better under the gain-framed Standard contract. As I have seen above, the average expected performance gain is striking: participants who choose a loss-framed contract expect to almost double their performance by reaching a mean productivity of 23.9 tables under their chosen threshold contract compared to the 11.6 tasks they expect to count under the Standard contract (N=128; Wilcoxon p-value <0.01).

Turning to my hypothesis the extent of the performance gain should depend on the strength of subjects' loss aversion. If subjects are indeed aware of the intensity of their loss aversion and anticipate its effectiveness as a self-nudging tool as I hypothesize, the group of more strongly loss-averse subjects should expect to benefit more from choosing a loss-framed contract than the participants who are less or not loss-averse ($H_{1.2}$). The results support my hypothesis: I find that the more loss-averse subjects expect to reach a significantly higher productivity of 26.7 tasks under the contract they chose, compared to the less or not loss-averse subjects who expect to solve only 17.7 tasks (Mann-Whitney p-value <0.01). I find the same pattern, when I ask by how much subjects expect to increase their performance relative to the Standard contract. Again, the more strongly loss-averse subjects raise their performance expectation by 14.7 (N=68) tasks, while the less or not loss-averse subjects expect to lift productivity by only 8.5 tasks under their preferred loss-framed contract (N=60; Wilcoxon p-value =0.01).

To confirm this result, I perform an OLS regression with control variables for chosen contract types and contract acceptance, variables for age and gender and the three-category dummy variable for loss aversion estimating the main effect. The result confirms that as subjects' degree of loss aversion rises, they expect to perform better (reg beta=2.240 p-value =0.02; reported as OLS_3 in Table 2).

I can express the expected performance gain also relative to the *Standard* contract. Here, the group of more profoundly loss-averse subjects expect to raise their productivity with 12.2 (*N*=75) tasks significantly higher relative to the expectation they have assuming they work under the *Standard* contract, compared to the less or not loss-averse subjects who expect to raise their productivity only by 6.6 tasks (*N*=76; Mann-Whitney *p*-value =0.01). The result is supported in an OLS regression including the same variables as in OLS_3 reported above (reg beta=1.572 *p*-value =0.03).

Table 2: Regressions for the Self-Nuding Experiment

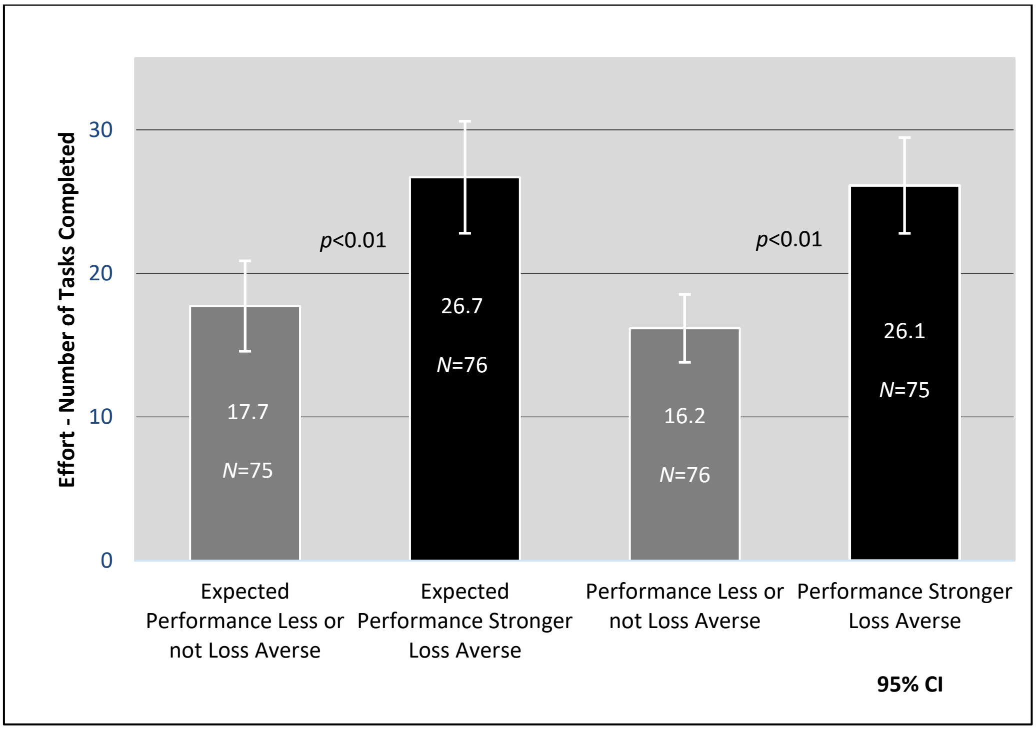
Dependent Variable Effort Provision	Logistic (1) Contract Choice	Ordered Logistic (2) Contract Choice	OLS (3) Expectations	OLS (4) Performance
Loss-Aversion	0.815** (0.306)	0.712*** (0.191)	2.240* (0.962)	3.022** (1.104)
Low Bar Contract			-4.095* (2.038)	-5.228 (2.341)
Stretching Contract			0.564* (0.889)	0.629 (1.021)
Extreme Effort Contract			28.778*** (2.027)	16.724*** (2.327)
Contract Acceptance	-0.620 (1.124)	-1.001 (0.785)	7.609* (3.652)	27.246*** (4.246)
Age	0.021 (0.038)	-0.039 (0.025)	-0.115 (0.136)	-0.013 (0.156)
Sex	0.555 (0.406)	0.374 (0.299)	-1.048 (1.514)	0.746 (1.739)
Constant	0.566 (1.582)		10.933* (5.494)	-10.736 (6.308)
Observations	158	158	158	158
R^2	Nagelk 0.113	Pseudo R^2 0.050	0.695	0.490

∗ $p < 0.05$, ∗∗ $p < 0.01$, ∗∗∗ $p < 0.001$.

3. Actual Performance

Now I analyze participants' actual performance to estimate whether their self-nudging strategy is effective. As before I hypothesize that the more profoundly loss-averse subjects should be better prepared to utilize loss-framed contracts for self-nudging and should therefore reach a higher productivity ($H_{1.3}$). The data support this hypothesis with the more loss-averse participants performing significantly better (26.1 tasks; *N*=75) compared to the less or not loss averse subjects (16.2 tasks; *N*=76; Mann-Whitney *p*-value <0.01). OLS analysis confirms this outcome: I include variables for the chosen contract types and contract acceptance, variables for age and gender. The main effect is estimated with the three-category loss aversion dummy. Results confirm that subjects with increasing loss aversion are significantly more productive (reg beta=3.022 *p*-value <0.01; reported as OLS_4 in Table 2).

Figure 3: Expected and actual performance by loss aversion type



4. Self-Nudging in the Autonomy and Preference Matching Treatments

If the more loss-averse subjects leverage their loss aversion more effectively, I should find a performance effect also in the *Autonomy* and *Preference Matching* treatments ($H_{1.4a, b}$). Indeed, in the *Autonomy* treatment I find a significantly higher performance of 27.2 tasks for the more strongly loss-averse subjects (N=80) compared to 21.4 tasks for the less or not loss-averse subjects (N=73; Mann-Whitney p-value=0.01; $H_{1.4a}$). An OLS regression confirms this result with control variables for chosen contract type, contract acceptance and demographics. The three-category dummy shows the significant main effect of loss aversion (reg beta=5.015 p-value <0.01; reported as OLS_5 in Table 2).

The result is the same for the *Preference Matching* treatment. When I consider the subjects who received the contract they favored, I find a performance of 19.6 tasks for the more strongly loss-averse subjects compared to 14.4 tasks for the less or not loss-averse subjects (Mann-Whitney p-value =0.02; $H_{1.4b}$). An OLS regression confirms this result with control variables for contract type and acceptance and demographics. A three-category dummy shows the significant main effect of loss aversion (reg beta=3.076 p-value<0.01; reported as OLS_6 in Table 2). The consistency of loss aversion's strong impact on productivity across experiments shows the effectiveness of the self-nudging contracts I propose. By contrast, loss aversion is *not* correlated to performance in the *Standard* contracts, which present no loss frame for subjects to leverage their own loss aversion as a spur to higher performance.

5. Self-Nudging is more Effective than Exogenous Loss Framing

As self-nudging allows subjects to consider their degree of loss aversion and their net-effort costs when choosing a threshold contract I expect subjects' productivity in the *Self-Nudging* experiment to exceed the performance of participants in each of the single contract treatments ($H_{1.5}$). As expected, productivity in *Self-Nudging* (21.1 tasks; *N*=151) is significantly higher compared to performance under the most productive single contract treatment—i.e., the *Stretching* contract (14.3 tasks; *N*=54; Mann-Whitney *p*-value <0.01; with a control for loss aversion reg beta=4.092 *p*-value <0.01). The same holds obviously also for a comparison to the less productive loss-framed contracts Low Bar (6.2 tasks; *N*=50; Mann-Whitney *p*-value <0.01; reg beta=4.246 *p*-value <0.01) and Extreme Effort (8.4 tasks; *N*=39; Mann-Whitney *p*-value <0.01; reg beta=1.922 *p*-value <0.01). Finally, the productivity is also significantly higher compared to the gain-framed Standard contract (10.4 tasks; *N*=50; Mann-Whitney *p*-value <0.01; reg beta=4.921 *p*-value <0.01).

6. Robustness Check

To check the robustness of my findings I used a second loss aversion measure with an altered design. This measure was designed and tested by Gächter, et al (2021). The task offers subjects the opportunity to participate in six mixed gain-loss lotteries. For each game subjects decide whether they want to participate or not. While the winning price is fixed at €6 the potential loss is increased with each lottery from €2-7. Which lottery is paid for is determined randomly after the end of the experiment. This measure reduces the difference between gains and losses in the lotteries to 1€ compared to the 3€ difference between gains and losses in the measure of Fehr and Götte. It allows us to identify subjects who are more lightly averse to losses and to show that they also may have leveraged their loss aversion. Indeed, overall in my sample, 88.8% (*N*=87 of 98) of the subjects manifest evidence of loss aversion; that is, they decide not to enter a lottery game even though it offers positive expected value. The result suggests that a vast majority of the subjects in my sample would principally have the option to leverage their loss aversion.

Tabel 3: Robustness Check Second Loss Aversion Measure

Dependent Variable Effort Provision	Logistic (1) Contract Choice	Ordered Logistic (2) Contract Choice	OLS (3) Expectations	OLS (4) Performance
Loss-Aversion II	0.542*	0.485***	1.456*	2.323***
	(0.215)	(0.135)	(0.635)	(0.613)
Low Bar Contract			-5.405	-7.008
			(2.456)	(2.372)
Stretching Contract			-0.119	-0.359
			(0.928)	(0.897)
Extreme Effort Contract			26.883***	16.161***
			(2.625)	(2.536)
Contract Acceptance	0.010	-0.738	4.029	29.186***
	(1.239)	(0.897)	(4.247)	(4.102)
Age	0.060	0.039	-0.115	0.005
	(0.052)	(0.028)	(0.150)	(0.145)
Sex	1.612	0.147	-1.315	1.355
	(0.622)	(0.406)	(2.005)	(1.936)
Constant	-2.271		14.950*	-15.434
	(2.174)		(6.706)	(6.477)
Observations	98	98	98	98
R^2	NagelK 0.275	Pseudo R^2 0.065	0.714	0.621

$*$ $p < 0.05$, $**$ $p < 0.01$, $***$ $p < 0.001$.

7. Summary of Main Results

In sum, we find consistent evidence for our theory that participants possess the sophistication to anticipate their own time-inconsistent preferences and are prepared to use their own loss-aversion as a nudge for improving their performance (see the summary of all results on Self-Nudging in Appendix B Tabel B1):

(1) Our data show that the more strongly loss-averse subjects choose contracts with higher thresholds and deeper loss frames, suggesting that subjects anticipate the strength of their loss aversion and consider it when they decide how deeply loss-framed the contract they choose should be.

(2) The more strongly loss-averse subjects also expect to perform significantly better, which again suggests that they consider in their expectations the degree of their loss aversion and thus the power of their self-nudge.

(3) The more strongly loss-averse subjects raise their actual productivity to a significantly higher level compared to less or not loss-averse subjects, showing that the more strongly loss-averse can indeed utilize their loss aversion more effectively.

(4) The robustness of my findings is shown in the *Autonomy* and *Preference Matching* treatments where more strongly loss-averse subjects also perform significantly better than the less or not loss-averse subjects.

(5) Compared to the single-contract treatments subjects can improve their performance substantially by entering one of the self-nudging contracts we offer them. We will later see also that subjects' take-up rate of the self-nudging contract is much higher.

III. Experiment Two: Why Autonomy Improves Performance

To analyze the impact that a BSM-enabling contract has on performance, we aim to separate two effects that we expect to improve productivity: (1) access to private information to facilitate a better match of the work task with a worker's preferences and abilities and (2) the experience of autonomy itself, which we refer to as the "Autonomy Premium."

An Autonomy Premium would provide our BSM contract design with a comparative advantage over contracts that exogenously try to nudge workers to a particular performance. Employers may learn how to match their employees' preferences and abilities. But if contract choice creates an Autonomy Premium, then even employers with a full understanding of their workers' production functions would benefit from granting autonomy.

A. Experimental Design

Design Concept. We aim to identify and isolate the Autonomy Premium by comparing two treatments: *Autonomy* and *Preference Matching*. In the *Autonomy* treatment subjects choose and obtain the contract they want directly, i.e. they receive their favored contract because they choose it. In the control, the *Preference Matching* treatment, subjects will also receive the contract they favor, however, not because they choose it, but because of a random process. We assume subjects in the *Autonomy* treatment experience self-determination utility, therefore have a stronger motivation for the task and perform better, compared to subjects in *Preference Matching* who we assume do not experience this extra motivation.

To test this hypothesis, we offer participants in the *Autonomy* treatment a choice among the three loss-framed contracts we presented above: *Low Bar*, *Stretching* and *Extreme Effort*. The control treatment, *Preference Matching*, also presents subjects with the same three loss-framed contracts and participants are asked which contract of the three they prefer. To incentivize the subjects to reveal their true contract preference we use a similar technique as in the Self-Nudging experiment: We instructed participants that with a 20% chance they would be directly offered the contract they prefer; if they were not directly offered their favored contract, they had to blindly pick one of the loss contracts (including the one they prefer) which then was offered to them. Thus, if subjects indicated their true preferences, they were more likely to perform under the contract they in fact favored.

To isolate the Autonomy Premium, we compare all subjects in the *Preference Matching* treatment who randomly received their preferred contract with the Autonomy treatment where subjects chose their preferred contract themselves. The difference between the two samples we assume isolates the effect that the experience of self-determination has on performance: while in both (sub-)samples subjects perform under the contract they favor, only in *Autonomy* did they receive the contract because of their own determination.

Additionally, the *Preference Matching* treatment allows us to estimate the performance gain of a better match of subjects' production functions with the contract's threshold, as half of the subjects received their preferred contract, while the other half did not. We compare the productivity of participants who all have a preference for the same contract; one half of these subjects were also assigned this contract while the other half was assigned an alternative contract they did not favor.

Random Contract Assignment. It is crucial for our design that subjects in the *Preference Matching* treatment do not suspect that they may have received their preferred contract automatically upon indicating they had a preference for it: if they did, the distinction between the *Preference Matching* and *Autonomy* treatments would be lost. To make the randomness of the contract assignment in *Preference Matching* salient, we prepared two sets of photographs picturing buildings in New York. The buildings' entryways were marked with a street number corresponding to the three contract thresholds—one of 5, 15 or 50. The numbers were pixelated. We first presented participants with two buildings whose street numbers did not correspond with the threshold the subjects had indicated to favor. After subjects chose, they were presented in the second stage with a new building matching the street number they had selected and a building with the street number that matched the contract threshold they had indicated to prefer. Subjects pick again and are assigned the contract with the threshold matching the street number of the building they chose. We provide a link to Google Maps for each building. The link allowed subjects to check that the building indeed has the street number we tell them.

With subjects picking their contract themselves and being able to check the Google links, the random assignment is fully transparent. Since subjects have a 50% chance to receive their preferred contract, this protocol provides us also with about 150 observations for all intended group comparisons: *Au-*

tonomy versus *Preference Matching* (only subjects assigned their favored contract) and subjects in *Preference Matching* who received their preferred contract compared to the subjects who did not.

Ruling out Reciprocity. Finally, as a robustness check we implemented a Random Autonomy treatment, in which it is determined randomly whether subjects can choose one of the three loss-framed contracts themselves, or are assigned one of the three contracts by chance. Since the subjects experience autonomy as a random event in this treatment and not as an intentional act of kindness granted by the offeror of the contract, the subjects have no reason to lift their performance because they wish to reciprocate a kind action. Therefore, if the performance-gain we observe in the *Autonomy* treatment was not driven by self-determination utility but by positive reciprocity instead, then subjects in the *Random Autonomy* should be less productive than those in the Autonomy treatment. For the random assignment of the contracts, we use the same protocol we employed in the *Preference Matching* treatment.

Ruling out Alternative Choice Mechanisms. We designed our contracts to provide subjects with incentives below and also above the threshold. This design not only improves performance, as subjects are rewarded for continuing their performance once the contractual obligation is met, but allows us also to separate the Autonomy Premium from alternative choice effects that can push subjects to reach the threshold, but not to perform beyond—such as guilt aversion over a breach of contract, or cognitive dissonance when the performance does not meet what was promised.

B. Theory and Predictions

The empirical evidence in the literature mingles the assumed productivity effect of autonomy with the benefits of preference matching. Deci and Ryan (2017, 2000), Frey (1997), Dickinson (2008), and Falk et al (2006a), Fehr et al (2013), and Neri et al (2017), among others, have shown in experimental and field work that individuals have a higher intrinsic motivation and persistence in the performance of a task, when they are granted autonomy in choosing it. The authors' experimental manipulations, however, not only affect the subjects' experience of autonomy, but also restrict them in choosing according to their true preferences. The studies all share the same basic design. They grant choice autonomy in one treatment and restrict choices in some way in the control condition; they measure how the restriction affects work effort. The choice restrictions vary in severity from factually limiting subjects' choice sets or action space, to offering monetary incentives, to applying mere social pressure. An example of a restriction imposing social pressure is Moller et al. (2006). In this study's *Autonomy* treatment subjects can freely choose their preferred effort task, while in the control condition the experimenter pressures the subjects to select a particular task explaining that they would help the study succeed choosing this task as experimenters had already collected enough observations for the other treatment groups. The authors measure higher work effort in the treatment group that grants the autonomy, but the design does not allow them to separate what has caused the advantage: it may either be caused by subjects being able to select the task that best matches their preferences or by subjects reacting positively to their perceived decision authority and work autonomy in itself. Dickinson et al. (2008) restrict autonomy with incentives and monitoring in a principal and agent game. Results depend on the intensity of monitoring: if monitoring exceeds some limit, subjects feel their autonomy being restricted and performance declines heavily, relative to a treatment where principals have no control option. Moderate monitoring, however, improves productivity and reduces shirking. Falk et al. (2006a) effectively limit subjects' choice set. In their principal-agent game principals can exercise control by requiring work thresholds; when they do, mean performance declines relative to a treatment that does not give principals a control option. Both studies may involve the two effects we aim to disentangle: the interference with the experience of autonomy and second a loss of motivation that results from not being able to choose what task or productivity level best fits own preferences.

To provide evidence for an independent Autonomy Premium driven by self-determination utility, we require a new type of experimental design. For separating the Autonomy Premium from any impact of a good match of preferences, abilities and work tasks we need to compare subjects who have the same preferences and then manipulate only their autonomy to choose a specific contract. Such a design allows us to compare performance under three conditions: (a) when subjects have no choice autonomy and are offered a contract that does not fit their preferences, (b) when subjects have no choice autonomy but are offered the contract they would have chosen themselves and (c) when subjects have choice autonomy and accordingly are able to themselves choose the contract they prefer.

1. Preference Matching

All things equal, an individual should perform better when her preferences and abilities are well-matched to the contract's threshold. To see this, consider that a subject's optimal effort is given by the difference of marginal effort costs and the marginal utility they derive from performance. Whether a loss frame increases or dampens subjects' optimal effort depends on the distance between their optimal effort choice and the threshold. A threshold above but close enough to their future-self's otherwise optimal effort can elevate their performance, because gain-loss utility increases with proximity to the reference point, providing a strong performance incentive. By contrast when subjects are randomly assigned a loss-framed contract and the threshold falls well below their otherwise optimal effort or is set well above, gain-loss utility is small and productivity shrinks.

This allows us to formulate two hypotheses: The performance of subjects who have a preference for contract γ and randomly received contract γ should exceed the effort level of participants who have the same preference for contract γ but were randomly assigned contract δ ($H_{1.1}$). Unlike in this comparison however, when employers impose contracts on workers without knowing or considering their production functions, some employees will nevertheless see their preferences matched while others will not. Therefore, as a robustness check we test the effect of preference matching against a more realistic benchmark: We compare the subjects who received their preferred contract γ in the *Preference Matching* treatment with the pooled loss contract treatments *Low Bar*, *Stretching* and *Extreme Effort*. The pooled treatments include both subjects who have a preference for contract γ and received contract γ and subjects who had a preference for γ but were assigned δ. We expect subjects in *Preference Matching* to perform better, given the larger probability of preference adjustment in that condition ($H_{1.2}$).

2. Autonomy Premium

We hypothesize that subjects derive separate self-determination utility completing the task when they are permitted to choose their preferred contract themselves. This utility should lift their productivity independently of, and possibly beyond, the loss-frame's threshold for the contract they have chosen, as we expect them to be less likely to discontinue their work once the threshold is met due to a stronger intrinsic motivation. For example, a subject may complete 18 tasks under a *Stretching* contract without being granted autonomy, but if she was given autonomy and chose that contract, the self-determination utility she derives may push her optimal effort further beyond the threshold, perhaps up to 25 tasks. To isolate this Autonomy Premium, we compare the productivity of subjects in the *Autonomy* treatment who chose contract γ with the productivity of subjects in the *Preference Matching* treatment who also favored contract γ and who were randomly assigned that contract. We expect subjects in the *Autonomy* treatment to be significantly more productive versus the subjects in *Preference Matching* ($H_{2.1}$).

3. Ruling Out Alternative Choice Effects

The concept of an intrinsic Autonomy Premium is behaviorally different from other effects of choice that also can raise performance and that lead to commitment. One mechanism driving commitment effects is cognitive dissonance (Festinger 1959). If subjects choose a contract threshold and then fail to comply with it, they may experience cognitive dissonance between their choice and their behavior, suggesting that they either made a bad choice, or that they failed to meet a performance norm they validated with their choice (see Leotti et al. 2010). To avoid this dissonance subjects may raise their performance to comply with the threshold contract they have chosen.

Guilt aversion is another mechanism that can commit workers to comply with the contract threshold (Charness and Dufwenberg 2006). Subjects may believe that by selecting a particular contract threshold they have induced a performance expectation in their partner. The subject anticipates experiencing guilt over disappointing this expectation and to avoid the resulting guilt the subject matches the threshold.

Choice may also induce regret costs. Regret can occur over the forgone benefits of an alternative decision path not taken. For example, a worker chooses one job, rather than another. The worker anticipates that she will experience regret should the job she took turn out to be inferior to the alternative she dismissed. Now she invests more effort into the chosen job confirming that she made the right choice in order to avoid future regret costs (Loomes and Sugden 1982; Bell 1982; Sugden 1985; Bartling et al. 2014 connecting regret costs with autonomy; Sjötröm et al. 2017). Regret costs are positive as long as the option actually taken is superior to the dismissed alternative. They are negative if the alternative path suggests a better outcome. Therefore, regret costs can both push and dampen effort in our study if subjects face some uncertainty about what may turn out to be their optimal effort (for example subjects' effort costs may vary across production time). Regret costs should push effort if the subject realizes she could have gained more net-consumption utility under a lower threshold contract she dismissed. Now the subject may raise her effort level to meet the higher contract threshold she did select to avoid regret costs. On the other hand, regret costs may dampen effort when a subject realizes that effort costs are lower than expected and her performance reaches the point at which the contract she selected becomes inferior to a more demanding contract she dismissed. Now the subject may reduce effort to avoid regret costs.

We do not rule out that commitment effects of some sort may have an impact on our results, but we seek to show that only the Autonomy Premium could have caused the results we observe. The central characteristic of self-determination utility that separates it from choice effects that induce a form of commitment is that self-determination utility can lift subjects' effort above the contract threshold they have selected. This leads us to the hypothesis that we expect comparatively more subjects to exceed contract thresholds in the Autonomy treatment and solve more tasks above the thresholds versus the *Preference Matching* treatment ($H_{2.2}$).

Finally, we want to rule out an effect that is only indirectly associated with choice. If workers perceive being granted the autonomy to choose a contract as an act of kindness, they may respond with positive reciprocity and lift their performance (see for example Hannan et al. 2005; Falk et al. 2006b; Fehr et al. 2007).[120] Therefore, we aim to mute positive reciprocity in the *Random Autonomy* treatment. The treatment does not induce positive reciprocity, because subjects are not deliberately granted autonomy; instead, autonomy is explicitly randomly assigned. We expect the Autonomy Premium to prevail when positive reciprocity is muted ($H_{2.3}$).

C. Results

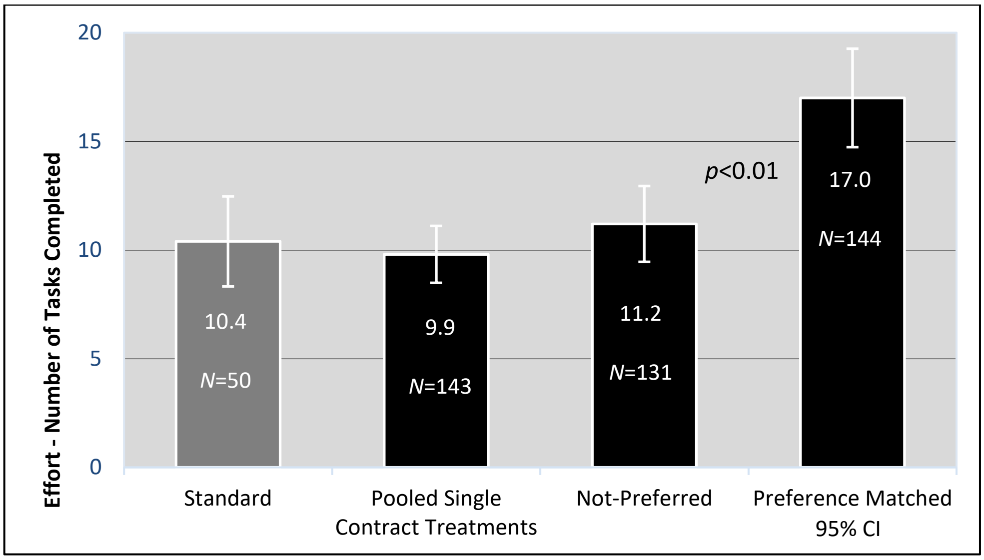
1. Preference Matching

We have seen that the *Self-Nudging* experiment has almost doubled subjects' performance compared to the *Standard* contract that provides only monetary incentives but no self-nudging option. Now we want to identify the share that the distinct effects of choice have in this large performance gain. We focus our analysis first on preference matching. We predicted subjects should improve their performance because choice allows them to select a contract that matches their production function ($H_{2.1}$).

Three hundred subjects participated in the *Preference Matching* treatment, of which 275 accepted a contract. We elicited the subjects' contract preferences: 105 indicated that they prefer a *Low Bar* contract, 118 favored a *Stretching* contract, and 77 an *Extreme Effort* contract. To isolate the effect of preference matching on performance we compare the effort level of the participants who received the loss-framed contract they indicated to prefer with those subjects who did not receive their favored loss-framed contract. The results support our hypothesis, as *Figure* 3 illustrates: Across the three loss-framed contracts, subjects who accepted the contract and whose preferences were matched, reach a mean productivity of 17.0 tasks (N=144), while participants who were randomly assigned a loss-framed contract, they would not have chosen, complete only a mean 11.2 tasks (N=131; Mann-Whitney p-value <0.01). Results hold when we account for the 25 subjects who rejected the agreement (16.0 versus 10.0 tasks; Mann-Whitney p-value <0.01).

[120] Note that we chose to make experimenters the contractual partners of the participants to make effects of positive reciprocity or guilt aversion unlikely to occur in the first place. With other participants as counterparts and their payoffs on the line, those effects would probably have had a much stronger impact on our results.

Figure 4: The effect of preference matching on performance



To confirm this result, we conduct an OLS regression with dummy variables for contract types and contract acceptance, variables for demographics, age and gender and for loss-aversion; another dummy variable that distinguishes whether subjects received their preferred contract or not estimates the main effect. The result holds and supports $H_{1.1}$: preference matching substantially improves performance (reg beta=5.600 *p*-value <0.01; reported as OLS_1 in Table 4).

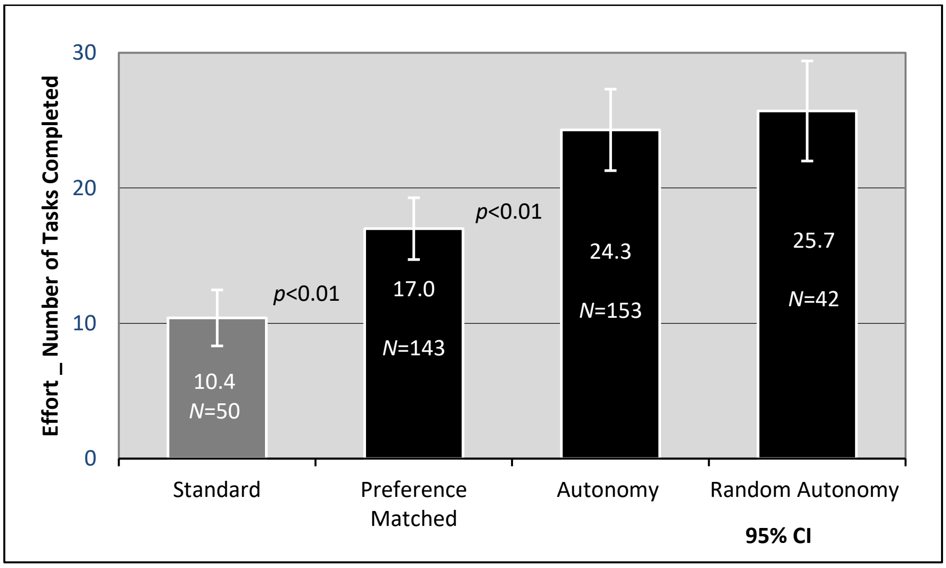
Another way to measure the performance advantage preference-matching generates is by comparing the effort provision of subjects who received their favored contract in the *Preference Matching* treatment with the productivity we observe when we pool the basic contract treatments *Low Bar*, *Stretching*, and *Extreme Effort*. With this comparison we account for the fact that whenever employers impose loss-framed contracts on workers without considering or knowing their actual preferences, some workers will receive, by chance, their preferred contract. As expected ($H_{1.2}$), the participants in the single threshold contract treatments reach only a significantly lower productivity (9.9 vs. 17.0 tasks; Mann-Whitney, *p*-value <0.01). An OLS regression including the same control variables as above and with a dummy variable that distinguishes the compared subject groups to estimate the main effect confirms that preference matching substantially raised productivity (reg beta=0.666 *p*-value <0.01; reported as OLS_2 in Table 4).

2. Autonomy Premium

We isolate the Autonomy Premium by comparing participants in Autonomy with the subjects in *Preference Matching* who are assigned their favored contract. We expect self-determination utility to improve subjects' effort provision under all three loss-framed contracts ($H_{2.1}$).

Indeed, as illustrated in Figure 4, mean performance of subjects in Autonomy (24.4 tasks *N*=153) is higher than subjects' productivity in the *Preference Matching* treatment across all contracts (*N*=144; 17.0 tasks; Mann-Whitney *p*-value <0.01). The difference holds when we include the 27 subjects (*N*=25 in *Preference Matching*; *N*=2 in *Autonomy*) who rejected the contract (24.1 tasks vs. 16.0; Mann-Whitney *p*-value <0.01).

Figure 5: Autonomy premium



We perform an OLS regression with the same subject sample used for the non-parametric tests and control variables for contract type, contract acceptance, demographics and loss aversion and a dummy variable distinguishing the treatments estimating the main effect. The regression confirms a significant autonomy effect (reg beta=7.180 *p*-value <0.01; reported as OLS_3 in Table 4).

Performance increases in the *Autonomy* treatment irrespective of the contract subjects enter: participants who select a *Low Bar* contract (*N*=54; 16.4 tasks) significantly exceed the performance of subjects in *Preference Matching* (*N*=53; 8.2 tasks) whose preference for the *Low Bar* contract was matched randomly (Mann-Whitney *p*-value <0.01). The same holds for participants who elect a *Stretching* contract, reaching 21.3 tasks (*N*=67), while participants in *Preference Matching* complete only 16.5 tasks (*N*=56; Mann-Whitney *p*-value

<0.01) under the same contract. Finally, participants who choose the *Extreme Effort* contract (N=32) achieve a mean effort level of 44.5 tasks, while subjects in *Preference Matching* who work under this contract complete only 31.3 tasks (N=35; Mann-Whitney p-value <0.01).

The results are supported in separate OLS regressions each with control variables for contract type, contract acceptance, demographics and loss aversion; a treatment dummy estimates the difference between the relevant subject groups of the *Autonomy* and *Preference Matching* treatments.

Table 4: Regressions for the Autonomy Premium Experiment

Dependent Variable Effort Provision	OLS (1)	OLS (2) Vs 14	OLS (3) 9vs. 12	Logistic (4) Subjects Exceeding Threshold	OLS (5) Effort Above Threshold	OLS (6)
Preferred Contract	5.600*** (1.261)					
Treatment		0.666*** (0.095)	7.180*** (1.376)	1.651*** (0.331)	4.148*** (1.035)	0.776 (1.033)
Stretching Contract	4.703*** (1.455)	6.240*** (1.214)	5.170*** (1.376)	-0.193*** (0.318)	-3.008* (1.176)	3.693 (1.904)
Extreme Effort Contract	10.374*** (1.613)	9.782*** (1.265)	22.889*** (1.780)	-5.819*** (1.058)	-7.172*** (1.339)	25.948*** (2.374)
Contract Acceptance	12.745*** (2.310)	12.535*** (1.438)	21.713*** (3.711)		2.447 (2.792)	39.204*** (6.182)
Loss-Aversion	2.828*** (0.788)	2.653*** (0.665)	3.653*** (0.845)	0.568* (0.027)	2.968*** (0.636)	3.777*** (1.054)
Age	-0.071 (0.161)		-0.101 (0.192)	(-0.210 (0.042)	-0.112 (0.145)	-0.449 (0.237)
Sex	0.068 (1.264)		0.946 (1.411)	-0.111 (0.320)	0.943 (1.062)	-0.546 (1.766)
Constant	-6.020 (4.837)	-10.296*** (1.604)	98.555 (17.301)	-18.563*** (4.105)	46.403 (13.013)	-22.685 (17.261)
Observations	300	342	308	308	308	199
R^2	0.288	0.393	0.463	NagelK 0.564	0.186	0.464

We find a significant Autonomy Premium on subjects' performance under all contracts, the *Low Bar* contract (reg beta=7.742 p-value <0.01), the *Stretching* (reg beta=4.461 p-value <0.01) and the *Extreme Effort* contract (reg beta=14.756 p-value 0.04).

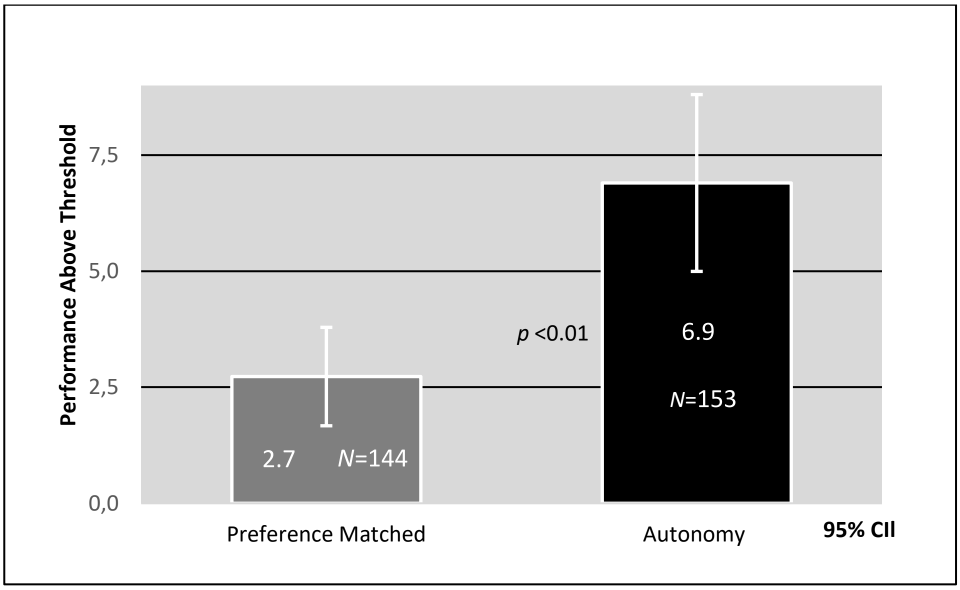
3. Ruling out Alternative Choice Mechanisms

The data show a significant performance raise above the threshold in the *Autonomy* treatment, supporting an independent effect of the Autonomy Premium that cannot be explained by alternative behavioral mechanisms such as regret costs, cognitive dissonance or guilt aversion, as they cannot push productivity beyond the threshold. In support of $H_{2.2}$ we find significantly more subjects in the *Autonomy* treatment exceed the contract threshold (69.3%; 105 of 153), than in *Preference Matching* (45.8%; 66 of 144; Fisher test *p*-value <0.01). A logistic regression, with control variables for contract types, contract acceptance, demographics and loss aversion and a dummy variable measuring the treatment difference confirms this effect (reg beta=1.651 *p*-value <0.01; fully reported as Logistic_4 in Table 4).

For further support, we measure the productivity difference across treatments above the threshold. As expected, subjects in Autonomy raise their performance and solve with an average of 6.9 tasks versus 2.7 tasks (i.e. 1056 vs. 325) significantly more and three times the number of tasks above the threshold than the subjects in Preference Matching with their favored contract assigned (Mann-Whitney *p*-value <0.01).

The result is confirmed in an OLS regression with the same control variables as included above and a treatment variable to estimate the main effect (reg beta=4.148 *p*-value <0.01; reported as OLS_5 in Table 4).

Figure 6: Performance above thresholds by treatment



4. Ruling out Positive Reciprocity

Finally, the data suggest that positive reciprocity had no significant impact on productivity. If the subjects would have responded with positive reciprocity to being granted autonomy, we should have seen them being more productive in the Autonomy treatment than the participants in the Random Autonomy treatment who experience autonomy only as a random event ($H_{2.3}$). However, as Figure 5 shows, performance in *Random Autonomy* (25.7 tables; *N*=42) is indistinguishable from or if anything higher than the performance of subjects in the Autonomy treatment (24.4 tasks; *p*-value 0.07 Mann-Whitney).

To confirm this result, we conduct an OLS regression with the same control variables as above and a dummy variable for comparing the treatments estimating the main effect (reg beta=-0.776 *p*-value 0.454; fully reported as OLS_6 in Table 4).

In summary, the results of Experiment 2 support our theory that individuals derive substantial self-determination utility from their performance when they experience autonomy (see the summary of the Autonomy results in Appendix B in Table B2). We separate this Autonomy Premium from performance effects associated with individuals' preferences being accommodated. Our data show that most of the performance gain of the Autonomy Premium is extra-obligatory and occurs above the thresholds. This rules out that the result we observe is driven by commitment effects caused by cognitive dissonance, guilt or regret aversion or alike that should not push performance beyond the thresholds. This extra-obligatory performance is characteristic for the intrinsic nature of self-determination utility and underscores the policy importance of the Autonomy Premium.

IV. Experiment Three: The Economic Value of BSM-Enabling Contracts

For contract drafters to use behavioral nudges as enhancement or even to replace incentives like penalties and rewards, it is important to assess the effectiveness of the behavioral tools in comparison to traditional economic levers. We therefore aim to estimate the economic value of self-nudging and our BSM-enabling contract design. To do so, we compare the performance of the subjects in *Self-Nudging* and *Autonomy* with productivity under the plain Standard contract where no other factors influence performance except the incentives. Increasing incentives under the *Standard* contract until the subjects reach the same productivity as in the *Self-Nudging* and *Autonomy* experiments should provide us with an estimate of the monetary equivalent for the behavioral effects of self-determination and nudging compared to standard incentive contracts (a similar idea for estimating the effectiveness of public behavioral policies is used by Benartzi et al. 2017).

The second variable for measuring the effectiveness of the BSM-contract design is contract acceptance rates. If supply of qualified workers is scarce, the efficiency of contracts does not rest only on the productivity of the parties who accept the contract. In a tight labor market, it matters also how likely workers are to enter a contract in the first place. That net consumption utility under a loss-framed contract is lower for loss-averse individuals can make it less attractive to pick up such a contract. Therefore, standard theory suggests that loss framing should reduce contract acceptance rates.

However, if sophisticated workers who understand their own demand for self-commitment are prepared to enlist their loss aversion to improve their productivity, then a contract that enables self-nudging can be more attractive than a gain-framed contract. Additionally, we expect both the deliberate element of self-nudging and the fact that subjects are offered a set of differently-framed contracts to choose among to add an Autonomy Premium on top of subjects' net consumption utility. Therefore, payoffs should be more likely to exceed production costs in the *Autonomy* and *Self-Nudging* treatments and we expect contract acceptance rates to be higher, compared to any single-contract treatment where subjects are not given an explicit choice.

A. Methods and Results – Estimating the Economic Equivalent

We first observed subjects' performance in the Self-Nudging and Autonomy experiments and learned that their performance exceeded productivity under the Standard contract by a factor of about 2.0 and 2.3 respectively. As our real effort task is neither cognitively nor physically demanding, at least within the relevant productivity range of up to 50 tasks, we assume a linear relationship between payment and performance for the plain Standard contract, thus increasing the payment from €1 per task to €2 per task we expect to approximately double subjects' performance and about equal the productivity we observed under *Autonomy* and *Self-Nudging*.

We presented subjects with a plain Standard contract, but this time paying €2 for each solved task instead of €1. As expected, doubling the performance incentives also about doubled mean productivity, lifting it from 10.4 to 21.6 tasks. Thus, performance in our effort task is almost a linear function of payment. Strikingly, productivity under this double-incentive Standard contract still falls short of the performance in the Autonomy treatment (Mann-Whitney p-value 0.04) and almost exactly matches productivity in Self-Nudging (Mann-Whitney p-value 0.56). Assuming the linear relationship between incentives and productivity, we can approximate that payment would have to be set at €2.29 per task to make subjects reach the same productivity as in the Autonomy treatment and at €1.95 to reach the productivity of subjects in the Self-Nudging experiment. Put in absolute amounts, to encourage the same mean level of performance under a gain-framed contract that we observe under the Autonomy treatment may cost €55.8 in monetary incentives instead of only €24.4; for the Self-Nudging experiment we find incentive costs of €41.2 versus €21.1. The result shows the enormous economic value of sophisticated behavioral contract design and captures monetarily the extensive value people place on their (work) autonomy.

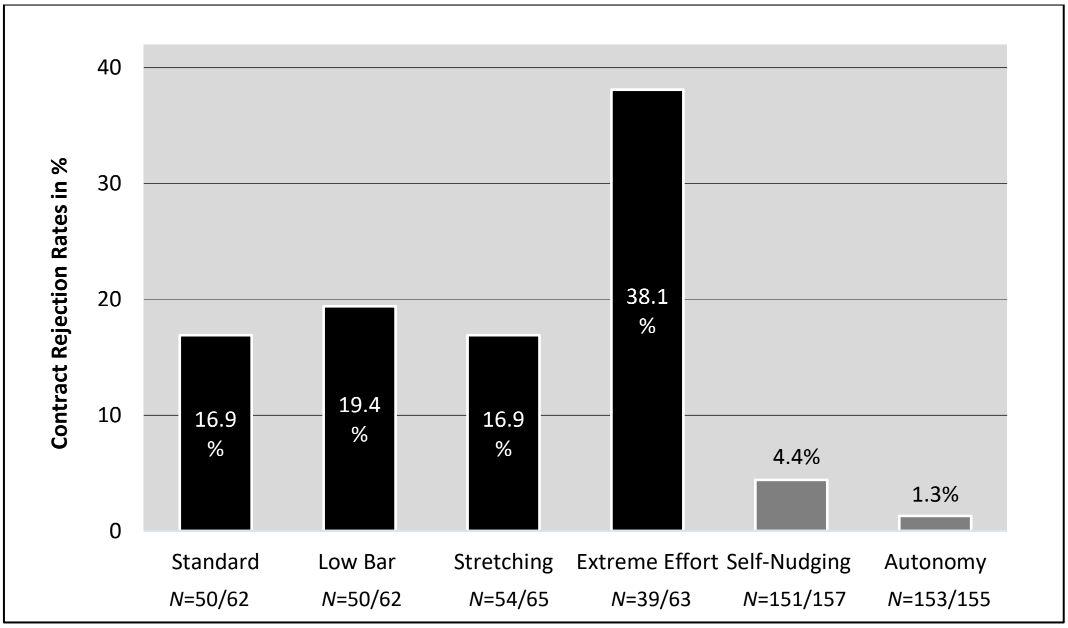
B. Methods and Results - Contract Acceptance

We used an endogenous contract design, such that all subjects had the option to turn down any of the offered contracts. Conducting the study online prevented subjects from experiencing sunk costs when turning down the contract, while they still could complete other parts of the study to receive an earning.

As you can see in Figure 6, only 2.9% (9 of 313) of the participants across Autonomy (N=2/155), and Self-Nudging (N=7/158) dismissed the contract offer. This rejection rate is considerably lower compared to all basic contracts that did not offer a choice including the Standard contract (for all comparisons Fisher test p-value <0.01).

We can reject that self-selection into a less demanding contract explains the high acceptance rate of the nudging contracts, as the acceptance rates for all basic loss-framed contracts, including the *Low Bar* and also the *Standard* contract which does not prescribe any performance target, are higher than the rates we observe in the *Autonomy* and *Self-Nudging* treatments (see these results reported in detail in Appendix B Table B3; also an overview of relative performance in all treatments is given in Appendix B Figures B1 & B2). Thus, subjects do not simply choose lower-threshold contracts to avoid demanding performance expectations.

Figure 7: Contract rejection rates compared to basic contracts



The opposite seems to explain the result: We have seen above that with a contract design that provides a self-nudging option and autonomy, subjects expect to reach significantly better outcomes. A contract that is expected to pay off better is more likely to be taken up and acceptance rates appear to rise accordingly.

The result emphasizes the attractiveness of self-nudging: apparently, it does not suffer from the low take-up rate typical for hard commitment devices (Ayres 2012, Gine 2010). The self-nudging approach provides individuals with flexibility: they can opt out and discontinue performance at any time, while the bias they enlist effectively pushes the effort provision of their future-self in an automatic process that requires little self-control resources once active.

III. Discussion and Conclusions

My findings suggest a variety of policy applications and directions for future research, not only within contract law and design, but also for many regulatory tasks at the intersection of private and public law. [121]

A. External Validity

To make the work task I present my subjects with as realistic as possible, their payment matches what students would earn in temporary jobs at a German university or in business. I selected a number-counting task because I wanted to keep subjects' intrinsic motivation low for methodological reasons. First, I want to identify an effect of intrinsically-valued autonomy on performance. Any creative task may become in itself more interesting when I grant subjects the autonomy to handle it by themselves (see Moller et al. 2006).[122] A mundane task allows us to conclude that subjects lift their effort because they place value on their autonomy, and not because my manipulation allowed them to tweak the task in a way making it more stimulating for them.

Second, I wanted to use a minimal manipulation of autonomy to show that self-determination utility is not only experienced when meaningful life choices are taken, but will affect performance when the choice in question is more mundane, as is often true in the workplace. And indeed, I find a strong Autonomy Premium even though subjects are only exercising choice over the contract under which they perform a simple number counting task. The fact that autonomy invigorates performance in this context underscores the value that people place on it.

Third, I wanted the task to focus on effort rather than knowledge or skills acquired before my study. An effort task allows subjects to improve their

[121] Note that I discuss further policy applications for self-nudging in Appendix A. These include the regulation of Car companies who have to meet emission and fuel consumption goals across the fleet they sell (see EU regulation No 443/2009 and the US CAFÉ standards). and German waste law.
[122] Consider a web-designer who either has to copy a website, or who is given some autonomy for improving the website in function and appearance.

performance by self-nudging; by contrast if I had asked subjects to guess random numbers, a self-nudging strategy would have been useless. The effort task also eliminates any skill or knowledge-related variance between subjects, which would have added noise to my data.[123]

However, other factors might limit the ecological validity of my study. My subjects are more educated than average, which might increase their sensitivity and sophistication in self-managing their behavior, so my results may not apply equally well to a more representative sample of the population. Further, the simplicity of the task might have made it easier for subjects to better estimate how present bias may affect their productivity and what threshold they should be choosing to exploit their loss aversion effectively. On the other hand, in real workplaces repetition and past experience might give workers a similar good understanding of their tasks and the potential of their present bias to make them less productive than they want to be, motivating them to foster their commitment. Experience might also compensate for a lack of sophistication. Self-nudging may not always require a clear understanding of the mental mechanisms involved. Individuals may not understand that they utilize their own loss aversion when they decide for a loss-framed contract. They may choose the contract, because they remember being productive when working under that contract (Laibson 2020). Also, the relatively small incentives I offer in my study may not limit my results, as people might plausibly invest more effort in self-managing if the stakes are higher.

Another important question is whether the effects of loss-framing and autonomy are robust in repeated work situations. Here the economic and psychological literature provides insights. Benartzi and Thaler (1995) analyzed trading behavior with bonds and stocks facing losses; over hundreds of repetitions traders preferred the safer bonds, even though they realized that the returns of stocks had been systematically higher. Pope et al. (2011) showed evidence for loss-aversion in professional golf. The golfers invested more effort in a putt that avoided exceeding par than a putt that would have secured a birdie, even though their success depended only on their overall score. Camerer et al. (1997) observed NYC cabdrivers and suggested that they formed a reference point of expected earnings for a given work-day. They drove longer hours on bad days (i.e., on days with nice weather, where fewer people take a taxi) to avoid a loss relative to that reference, while they would have maximized their hourly income working longer on good taxi days (cold and rainy) instead. In none of these studies did the workers' professional experience and the fact that they had repeatedly performed their work tasks weaken the impact loss aversion had on their behavior. Instead, it seems more

[123] Chaudhry et al. (2016) for example fail to find an effect of contract choice, plausibly because they use a slider task which requires skill and accuracy.

likely that subjects learn with experience that their loss aversion is robust and therefore is a reliable commitment device. For that reason BSM might not become weaker but rather more effective with time and experience.[124]

There are also indications in the business literature that autonomy's effects on work productivity may be persistent: survey data shows that long-time job satisfaction and identification increase with the autonomy employees have (for example, Wheatley 2017).

Extra-contract communication and work culture may weaken the effects of contract choice if an employee realizes that co-workers follow informal rules different from the norms the contract suggests. Also conflicting interests at the workplace may dampen the effectiveness of the contract design. But work culture may also reinforce my findings when people work together and observe and learn from each other's behavior. To make behavioral contracting successful, employers must ensure that the autonomy that the contract grants workers is not undermined by informal practices at the workplace.

B. Internal Validity

Normative Force of Contracts. In the Self-Nudging experiment, I show that subjects utilize their loss aversion to improve their productivity and payoff. But subjects may also use the contract itself as a commitment device. As gains and losses differ only in framing but not in the incentives they offer, the penalty for breach that the contracts stipulate is equal to the piece rate payment for each unit. Therefore, no matter whether the subjects comply with the contract or breach it, they will always earn a payment that reflects merely the number of tasks they in fact solve. With no extra penalty for breach being provided for, subjects could not exploit the enforcement of the contract as a commitment device. They would have to utilize their moral motivation for obeying the contract. Among others Vanberg (2008) and Wilkinson-Ryan & Baron (2009) suggest that people may have a per se preference for keeping contracts, Ederer and Stremitzer (2015) show subjects are guilt averse over letting their contractual partner down.

However, the possibility that subjects may utilize their moral motivation as a commitment device in my study could not explain my findings that more strongly loss-averse subjects choose more deeply loss-framed contracts, have higher performance expectations, and are more productive—unless

[124] Financial incentives can sometimes be so high-powered that they may crowd out the effects on internal work motivation that I report (Frey 1997). In our study, I adjusted the payment to match typical student jobs. If I would have paid either one cent or $10 per table completed instead, subjects may not have been sensitive to our autonomy manipulation of the contract terms. On the other hand, the influence of incentives is also limited, when performance is difficult to monitor or attribute, when workers have indispensable qualifications or job security. In these cases I expect self-determination utility and intrinsic motivation to affect performance even stronger.

these moral motivations were actually strongly correlated with loss aversion. Only then, while subjects may in fact utilize their guilt aversion for self-nudging, I might nonetheless find stronger loss aversion to be predictive of better performance. However, no such relationship has been reported in the literature and it does not appear to be a plausible supposition—there is no evidence that the mental mechanisms behind loss aversion and guilt aversion are linked. But while promise-keeping or guilt aversion do not explain my findings, my data do not rule out that subjects exploit their guilt aversion over contract breach for self-commitment. This is an interesting question to address in future research.

Contrast Effects and Extreme-Avoidance. Biases like contrast effects or extreme-avoidance may also explain subjects' contract choices. Most subjects favored a Stretching contract, but rather than aiming for selecting a commitment device, subjects may have chosen this contract because it stood out being presented alongside less attractive options. A similar explanation might be that people, when offered three or more options to choose among, often avoid the extremes, which, in the context of my experiments, could lead them to choose the *Stretching* contract.

Note however, my findings compare differences between subject groups, and the more loss-averse subjects are, the more likely are they compared to the less or not loss-averse participants to choose *Stretching* or *Extreme Effort* contracts. If contrast effects or extreme-avoidance were present, those effects should have shaped the choices of both groups of subjects equally; they therefore do not explain why the contract choices of the two groups differ.

Goal Setting. Behavior driven by goal setting (for example Locke and Latham 2006; in economics see Goerg and Kube 2012) might also explain why subjects choose ambitious thresholds and improve their performance in my study. Goals can advance work commitment when they are challenging, important for the worker, and achievable. Self-chosen goals tend to have these properties in the chooser's perspective and goal setting theory predicts that workers will be more faithfully committed to a goal they have set relative to standards imposed on them.

Yet, in my *Self-Nudging* experiment, all subjects work under a self-chosen threshold, while loss averse subjects choose higher thresholds, have higher outcome expectations and perform better. Thus, while present in my study, goal setting does not provide an explanation of my findings.

C. Towards a Theory of Bias-Self-Management

Most behaviorally-informed regulation seeks to identify potential biases and then designs interventions to prevent them from negatively affecting behavior. These interventions tend to assume that decision-makers are either not aware of their biases or at least are unlikely to manage them without intervention. In my study individuals utilize their own loss-aversion as a self-nudging device to improve their productivity. By choosing a loss-framed contract they rearrange their decision environment without changing their monetary incentives and counter present bias with their loss aversion. Zamir and Teichman (2018) refer to this strategy as "indirect debiasing" as the consequences of subjects' present bias are contained.

Apparently, subjects' intervention fits the account that Thaler and Sunstein give of nudging (Thaler and Sunstein 2008), with the distinction that the subjects act as their own choice architects. This phenomenon of self-nudging should widen my perspective on biases. Biases can cause cognitive error and dampen motivation, but they can also be a valuable tool for individual decision making.

Self-nudging is just one strategy of BSM. BSM rests on the assumption that individuals can act to improve their decision-making outcomes by deliberately employing the same strategies that otherwise law-makers tend to use: (1) they self-debias; (2) they self-insulate their biases from their actions; and (3) they engage in self-nudging (see the analogies for state interventions in Jolls and Sunstein 2006). To do so, individuals may access and use some institution or social mechanism that facilitates their BSM strategy. I have provided an example of one such institution in my experiment—a loss-framed contract serving the decision-maker as a tool for self-nudging.

There are also BSM analogues for debiasing and bias-insulating interventions. Arlen and Tontrup (2015a) focus on the BSM equivalent to debiasing. Their study shows that property owners often are aware that ownership creates a bias and deliberately involve agents when selling their property. The responsibility that the agent assumes for making the trade mutes the owners' regret costs over losing their property. This allows owners to give agents unbiased instructions for the sale. There is also a BSM analogy to an insulating intervention. Arlen and Tontrup (2015b) show that owners strategically access information to learn whether other market participants have traded. If most of them have traded, owners take their decision as a reference point. Instead of feeling regret over selling, they anticipate experiencing regret over not following their example and finding out later they made the wrong choice. By accessing the market information, owners do not directly debias, but rather they strategically relocate their biases.

My BSM account is policy relevant, because it refers to decision-making strategies that I can show people in fact use to manage their biases. By contrast many studies in psychology and business economics focus on constructing optimal debiasing strategies and test their performance relative to rational benchmarks. Roese et al. (2012) for example advise decision-makers to suppress hindsight bias by constructing an alternative chain of causal events that would have led to a different outcome. Studies of this sort do not ask whether people are in fact using the debiasing or the self-nudging strategy they analyze. For legal policy this is a crucial elision. Legal policy is interested in the gap between actual and socially optimal behavior; an intervention becomes sensible if the gap between the two reveals sufficient room for improvement. If people use (or would use in the right institutional framework) BSM strategies effectively, then an externally-imposed intervention may not improve decision quality and payoffs by enough relative to the BSM strategy to outweigh regulatory costs. This may be true even if the BSM strategy is in itself not optimal.

To support environments for BSM strategies to be used effectively, has advantages over external, public-policy-based interventions. BSM imposes costs only on those who are in fact biased and choose to debias, insulate their bias, or to utilize it for self-nudging. In my study, with subjects choosing the contract that personally benefits them, the costs of loss-framing are only carried by those who benefit from self-nudging, while those without a demand for self-commitment remain unburdened. In contrast, external policy interventions often cannot identify and address biased individuals separately from those who are not biased (it may not even be able to identify those who are biased); the intervention therefore imposes the regulatory costs on all people subject to the regulation (see Korobkin and Ulen 2000, Rizzo and Whitman 2009, Arlen and Tontrup 2015a).

BSM might also help save indirect costs of regulation. Interventions that restrict people's choices often trigger rejection and reactance (see Arad and Rubinstein 2018). The relatively higher rates of contract rejection in the single-contract treatments (i.e., where I imposed a particular loss-framed contract), might be an example of such dampened motivation.

Policy makers and contract drafters can support and encourage BSM by providing decision makers with institutions or decision-making infrastructure permitting them to debias, insulate or harness their biases implicated in a particular domain to their advantage. The contract design that allows workers to turn their loss aversion into a self-nudging device is not the only example of such institutional support. Other examples are found in Arlen and Tontrup (2015a): owners rely on agents to debias their trading and in Arlen and

Tontrup (2015b): the owners strategically access market information to avoid regret costs.

BSM however, has limits and cannot generally replace direct legal intervention. BSM requires sophistication: decision makers must be aware of their bias and must have the capacity to apply a countervailing strategy. Some biases are likely to elude notice by even the most self-aware individuals. Awareness of the hindsight bias, for example, in a normal population is rare, as it effectively changes the decision-maker's memory (Hoffrage et al. 2000). Future research must identify the legal and social contexts in which people can effectively manage their biases and what institutional underpinnings should be provided to allow them to do so effectively in particular decision-making contexts. In this sense I propose BSM as a research program and a complementary instrument in the behavioral toolbox of regulators.

D. Policy Applications and Directions for Future Research

Possible applications of BSM in the private law context are obvious. However, while recently loss-framing has become more common to address principal/agent problems typical for service or construction contracts (see Alterbaum 2016 referring to the construction of the Oresund bridge), general business contracts have rarely been loss-framed in the past (Baker et al. 1988, Lazear 1991). Contractors may have believed that trying to take advantage of loss aversion will provoke the disapproval of counterparties and deter workers or potential business partners from entering an agreement (see Luft 1994). Or employers may have expected that they would have to pay a premium to entice workers to accept a loss-framed contract (Imas et al. 2017). I have also seen that loss-framing can be risky if the employer has little knowledge about workers' production function. Setting thresholds too high or too low can severely dampen performance below the level workers would reach under a plain gain-framed contract (Brooks, Stremitzer and Tontrup 2017). Thus, in the eyes of employers and contractors the productivity advantage of loss framing may not have outweighed these extra costs and risks.

My new contract design avoids these informational risks and can be a helpful tool where an asymmetry of information or expertise between the parties causes a monitoring problem. But even where parties could exercise authority and might gather sufficient information to set effective thresholds my data suggest, relying on incentives (or penalties) rather than a BSM-enabling contract might be more costly. In my experimental setting, the performance gain due to self-nudging was substantial. Achieving the same work effort with monetary incentives alone required doubling workers' payment.

Finally, I want to analyze more thoroughly one innovative application of BSM, the design of privately-sponsored retirement savings plans, to demonstrate the practical policy relevance of my findings beyond their obvious implications for work contracts. Many Americans appear to save less for their retirement than is optimal. In a survey study by Choi et al. (2006) two-thirds of respondents indicated that rationally they should save more. Policy interventions have therefore attempted to increase savings by providing tax exemptions, both to incentivize employers to sponsor retirement saving plans for their employees, and to entice employees to save more. The positive effect of the tax incentives on actual savings however, has remained small (see Madrian and Shea, 2001, Chetty et al. 2014). In particular the tax incentives do not reach the type of present-biased worker the regulation was supposed to target.

The reason for this failure seems obvious: People saving for retirement have time-inconsistent preferences; they would like to save more until the time comes for them to contribute to their plan, at which point their preferences shift due to their present-bias (i.e., they prefer to have money available now, rather than later). But these plans are designed such that workers must save money first, before employers match the workers' savings with a contribution ex post. As the plans promise rewards only for the future, present-biased workers react to these incentive plans only weakly. Ironically, the workers' present-bias is both the reason why the regulation was created and the reason why it fails.

My findings suggest that a loss-framed retirement savings plan design should be more effective. Such a plan may allow workers to choose between different savings targets adjusted to their income (for a description of such smart defaults, see Batchelder 2018). Their choice should entitle workers immediately to the employer's contribution that matches the savings goal they select. In contrast to current plan designs, employees should receive this contribution (which is frozen and may only be used for retirement savings) upon selecting their savings goal before actually building up savings, not after. By allocating workers the matching contributions immediately, this plan design counters their present-bias. However, workers would lose this up-front employer benefit, in whole or in part, should they fail to meet the savings goals they have chosen. This activates the worker's loss aversion and since negative gain-loss utility looms larger, workers have a much stronger incentive to meet their savings goal than under the current plan design. Finally, fostering the experience of autonomy by providing choice among different savings goals — which are really loss frames of different depths—may lead workers to select and comply with a savings goal that is more ambitious, compared to any legally-mandated savings rates that may in the future be politically achievable in the U.S.

To create the necessary framework, regulation could either directly mandate a retirement plan design with advance contributions and socially-optimal defaults or incentivize employers to offer such plans voluntarily. my results suggest that a loss-framed plan design which provides choice within the realm of smart defaults may increase actual savings rates substantially.

V. Appendix A

In the following Appendix A I provide additional information on the experimental design and methods and additional applications that can add to the understanding of my project, but which I felt would distract from focus and flow of the main paper if placed there.

A. Methods

1. Procedures

I conducted my study online using the Limeservice platform and the open-source software Limesurvey (see http://www.limesurvey.org/). I invited the subjects via email to the experiment, drawing from a large subject pool (approx. 1500 subjects) that I established at the University of Münster. The e-mail invitation did not describe the purpose of the study in order to avoid participants self-selecting into an experiment in which they were interested. To make sure that subjects do not have to discontinue the study because completing the study takes longer than they expected, I inform them in the invitation that completing the study will take them between 15 and 50 minutes and that the time they invest and their payment will depend on their choices.

To ensure that subjects focus on the experimental task I implement a time limit. Participants are informed that if they fail to make an input after 180 seconds or if they log out, I exclude them from the experiment without payment. The conditional payment and the easy-to-handle real effort task result in a low drop-out rate (for an online study) of ~10% of participants who began the study.

For calibrating the payoffs, I wanted to assure that incentives are comparable to a normal working environment in a regular student job in and outside of the university. A student job in Münster would offer approximately €8 per hour. In my real-effort task, participants needed easily less than 1 minute to complete a table. Reading the instructions took around 5 minutes, such that the subjects earned an hourly wage of at least €55. Note however, I randomly selected every fifth participant for payment. Subjects' expectation therefore was to earn at least €11 an hour, slightly more than in a student job. In addition, subjects could earn the payoff from the loss aversion measure which in expectations increased their payoff by another €3.

For the random selection of the subjects to be paid, I asked subjects to predict the last two digits of the numbers drawn in an upcoming public lottery, the Eurojackpot. The chance of getting one digit right was 1in ten, providing them with a chance of being paid of 20%. I provided subjects with the link to the Eurojackpot website to ensure transparency of the random selection process allowing them to check themselves whether they have won or not. Stochastic payouts like the one I implemented are routinely used in experimental economics. Evidence suggests that larger amounts paid at a lower probability can simulate higher stakes decisions: that is, while actual payoffs in expectations are equivalent the incentives are more effective than if smaller amounts are paid with higher probability (Laury 2005 and Laury et al. 2008). I also attribute my relatively low dropout rate to participants' loss aversion and the good experimental earnings (see for a comparison the meta-study on online experiments by Musch 2000).

2. Treatment Designs

The following table summarizes my treatment design.

T able 5. Overview of the Treatments

Treatments	*Descriptions*
Applies to all Treatments	Subjects complete only ONE treatment Subjects can reject contract, forgoing payment Treatments are comprised of one or more of the contracts that I described above: *Standard*, *Low Bar*, *Stretching*, and *Extreme Effort* contracts
Self-Nudging *N*=151 (exclusive 7 contract rejections)	Choice between all gain-framed (*Standard*) and loss-framed contracts (*Low Bar*, *Stretching*, or *Extreme Effort*) Loss-framed contracts require payment of extra fee rising with threshold Subjects indicate how they expect to perform under each of the contracts I compare actual performance under chosen contract with expectation
Autonomy *N*=153 (exclusive 2 contract rejections)	Choice between all loss-framed contracts (*Low Bar*, *Stretching*. or *Extreme Effort*)
Preference Matching	Subjects indicate which of the loss-framed contracts they prefer (either *Low Bar*, *Stretching*, or *Extreme Effort*)

N=275 (exclusive 25 contract rejections)	With a chance 20% subjects directly receive the contract they preferred With chance 80% subjects are randomly assigned one of the three contracts Only subjects who were randomly assigned a contract are considered in my results
Random Autonomy *N*=42 (exclusive 2 contract rejection)	Random assignment of EITHER: Choice between all loss-framed contracts (*Low Bar*, *Stretching*, or *Extreme Effort*) OR Random assignment of one of the three loss-framed contracts (*Low Bar*, *Stretching*, or *Extreme Effort*)
Pooled Threshold Contract Treatments *N*=143 (exclusive 45 contract rejections)	Subjects are presented with one threshold contract (either *Low Bar*, *Stretching*, or *Extreme Effort*)

I did not induce contract acceptance, because I wanted to measure whether granting autonomy and facilitating the use of loss-framing as a commitment device would increase subjects' willingness to enter a loss-framed contract. In the regression analysis of my data, I used a dummy variable to control for contract acceptance. For the non-parametric tests, I analyzed the data both including the observations that rejected the contract and excluding them. When including the subjects who rejected the contract, I assumed that they exerted zero effort. Note that both sets of results—i.e., the one in which I account for contract rejections, and the other in which I exclude them from the sample—reflect a situation principals care about. If the supply of workers is unlimited, a principal may consider only how agents perform under the offered contract and is indifferent to the possibility that the contract terms may deter workers from acceptance. Considering only those subjects in my analysis who accept the terms reflects this situation. On the other hand, the supply of qualified workers might be scarce, such that unattractive terms that deter many workers from acceptance may severely harm production. I capture this scenario by including all participants in my analysis.

C. Results - Self-Nudging Increases Performance Quality

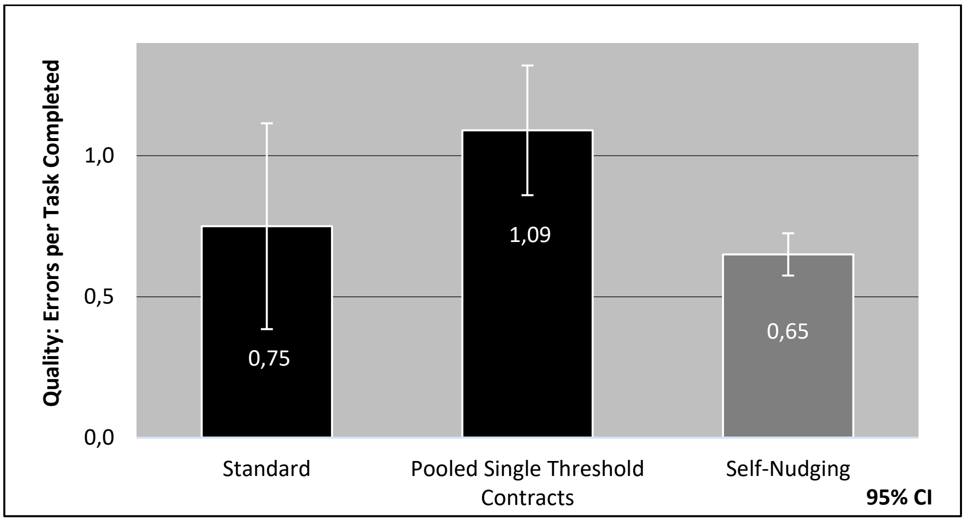
I did not induce contract acceptance, because I wanted to measure whether granting autonomy and facilitating the use of loss-framing as a commitment device would increase subjects' willingness to enter a loss-framed contract. In the regression analysis of my data, I used a dummy variable to control for contract acceptance. For the non-parametric tests, I analyzed the data both including the observations that rejected the contract and excluding them. When including the subjects who rejected the contract, I assumed that they exerted zero effort. Note that both sets of results—i.e., the one in which I account for contract rejections, and the other in which I exclude them from the sample—reflect a situation principals care about. If the supply of workers is unlimited, a principal may consider only how agents perform under the offered contract and is indifferent to the possibility that the contract terms may deter workers from acceptance. Considering only those subjects in my analysis who accept the terms reflects this situation. On the other hand, the supply of qualified workers might be scarce, such that unattractive terms that deter many workers from acceptance may severely harm production. I capture this scenario by including all participants in my analysis.

My study design allows us also to measure performance quality. I estimate quality by how much subjects' entries deviate from the true value (i.e., the true number of appearances of the specified digit in the given table). I consider failed and successful attempts; successful attempts can still vary by quality, as an input within a range of +/-2 of the true value permits subjects to proceed to the next table. I calculate deviations per table completed successfully and refer to the outcome as a "quality score". The experimental software did not enable us to record subjects` exact entry for failed inputs, so I assume in my analysis the minimal deviation of 3 from the true value for all trials that failed. The design makes it very difficult for subjects to deliberately trade off quantity and quality. As the quality threshold required for moving to the next table permits only for a small margin of error subjects are discouraged from attempting to shirk on quality to reach a higher quantity result. This allows us to attribute changes in performance quality directly to subjects' motivation and attention to the real effort task.

The single contract treatments, *Low Bar, Stretching and Extreme Effort,* are less likely to match the subjects' individual preferences and production functions with the thresholds they offer. A poor match to most subjects` actual preferences as in the low and the extremely high threshold treatments, leads to a low-quality performance. In both the *Low Bar* (1.23) and *Extreme Effort* (1.45) treatments, performance quality is significantly worse than under the *Stretching* (0.68) and the *Linear* contract (0.75) in *Baseline* (for all four treatment comparisons Mann-Whitney p-value <0.01).

The data show that self-nudging and contract choice effectively increase performance quality: In the Self-Nudging treatment I find a score of 0.64, significantly better-quality performance than in the pooled single-threshold treatments (1.08; Mann-Whitney p-value <0.01). I perform OLS regressions with dummy variables for contract type, contract acceptance and loss-aversion; another dummy variable estimates the difference between the Autonomy and the single threshold treatments I am interested in. The difference is clearly significant (Self-Nudging reg beta=0.145 *p*-value <0.01).

Figure 7: Mean Quality Score Across Treatments



Importantly, I can conclude from the data that the strong increase in productivity driven by self-nudging is not achieved at the expense of performance quality. Indeed, the data rather suggests the opposite: enabling self-nudging and contract choice appear to motivate both more and better effort.

D. Implications

1. Internal and External Validity

In real work environments the factors that drive motivation will vary across types of jobs, departments or branches, and individuals. Some individuals may only work for the money, some will be intrinsically motivated by enjoyment of their tasks, some may have strong internal standards or desire to adhere to professional norms. My task does not induce intrinsic motivation, and that permits us to cleanly separate the autonomy premium. It seems plausible, however, that the demand for self-commitment declines the more intrinsic a worker's motivation. Thus, in environments where intrinsic motivation plays a dominant role, self-commitment might be less effective in improving performance (although it may be effective as a means to finish work). On the other hand, the autonomy premium is likely more readily experienced with tasks that are driven by intrinsic motivation as I explained in the main text.

2. Policy Implications and Directions for Future Research

An additional example for a public policy application that I did not spell out in the main paper is waste management law. Many countries try to reduce waste production, but regulation that relies solely on monetary incentives faces a tradeoff: on the one hand increasing prices for waste disposal would incentivize industry and households to reduce their waste production, on the other hand, when prices are pushed too high, the rate of illegal waste disposal increases dramatically (Engel 2002). Therefore, regulation often compromises, providing only moderate financial incentives for waste reduction.

A behavioral design that uses my findings could help attenuate this conflict of objectives at no extra cost. Communities can design a loss-framed schedule for the fees they charge. Households would first have to pay a standard tax that applies to everyone. The tax corresponds with a threshold for disposal. Households would then choose their preferred disposal plan. The plan entitles them to an up-front payment if their disposal plan is below the threshold. By contrast for selecting a plan above the threshold the household has to pay a penalty. If a household does not comply with the chosen plan – i.e., if they dispose of more waste than they have specified -- they lose all or part of the up-front payment. Putting the endowment at stake elicits loss aversion and increases the cost of not meeting the individual waste management goal. Note that my experiments suggest that contractually promised payments may already induce a reference point or an outcome expectation that individuals process as a reference point (see above Köszegi and Rabin 2006), which makes an implementation even easier.

The design has two main advantages: it should generate an autonomy effect, elevating people's intrinsic motivation to meet the goal (or at least reduce resistance to the regulation), and importantly, communities might be able to offer more ambitious plans without driving up illegal waste disposal, as people are more willing to accept demanding thresholds when they are given a choice. Those who choose a plan above the threshold still have an incentive to cut down their disposal, as they can regain their tax in part.

I offer another example. Car companies must meet emission and fuel consumption goals across the fleet they sell (see EU regulation No 443/2009 and the US CAFÉ standards). Instead of paying fines when they fail to reach the norm, the companies could pay a standard tax and choose an emission target within limits prescribed. Depending on the target they choose, the companies would receive a tax bonus. If they fall short of their self-selected goal, they lose all or a portion of that bonus.

This example raises the interesting question whether my findings may apply to corporate actors. Brokers acting as agents for large investment firms can be motivated by loss aversion (Odean 1998, Shefrin and Statment 1985); they often receive bonuses tied to their company's profit or they may be compensated with shares. A slightly different take is that corporate agents might engage in defensive decision-making and therefore act as if they were loss-averse (see Gigerenzer 2014). This may be true especially if realized losses may be more easily perceived as a management error relative to potential gains not pursued.

VI. Appendix B

Table B1: Summary of Effects on Self-Nudging

Choice Contract, Expectation & Performance by Loss Aversion Type

Self-Nudging	*N*	*p-value*	*(p-values are two-tailed Mann-Whitney* Stronger Loss Averse	Less or not Loss Averse
Choice of Loss or Gain Frame	*N*=151	$p < 0.01$ ($Z = 10.9$)	*26.13*	*16.18*
Performance Expectations	*N*=151	$p < 0.01$ ($Z = 3.72$)	*27.24*	*17.46*
Actual Performance	*N*=151	$p < 0.01$ ($Z = 4.56$)	*26.13*	*16.18*

Table B2: Summy of Effects on Autonomy and Preference Matching

			Comparison Group *(p-values are two-tailed Mann-Whitney)*			
Comparison Group	Treatments	Mean Effort	Pooled Single Threshold	Preference Not-Matched	Prefe-rence Matched	Autonomy
Pooled Single Threshold Treatments (*N*=143)	Low Bar; Stretching; Extreme Effort	9.9	–	$p = 0.14$ ($Z = -1.46$)	$p < 0.01$ ($Z = -5.29$)	$p < 0.01$ ($Z = -8.79$)
Preference Not-Matched (*N*=131)	Preference Matching	11.2		–	$p < 0.01$ ($Z = -4.25$)	$p < 0.01$ ($Z = -7.94$)
Preference Matched (*N*=144)	Preference Matching	17.0				$p < 0.01$ ($Z = -4.44$)
Autonomy (*N*=153)	Restricted Choice	24.4				–

*Only Subjects Who Entered a Contract

Table B3: Summary Rejection Rates Across Treatments

Treatment	Threshold	Rejection Rate	Standard	Low Bar	Stretching	Extreme Effort	Self-Nudging
			Treatment *(p-values are two-tailed Chi² tests)*				
Standard	– (*N*=50)	19.4%		*p* = 1 (χ^2=0.00)	*p* = 0.72 (χ^2=0.12)	*p* = 0.02 (χ^2=5.35)	*p* < 0.01 (χ^2=12.5)
Low Bar	5 (*N*=50)	19.4%		–	*p* = 0.72 (χ^2=0.12)	*p* =0.02 (χ^2=5.35)	*p* < 0.01 (χ^2=12.5)
Stretching	15 (*N*=50)	16.9%			–	*p* < 0.01 (χ^2=7.21)	*p* < 0.01 (χ^2=9.68)
Extreme Effort	50 (*N*=39)	38.1%				–	*p* < 0.01 (χ^2=42.3)
Self-Nudging	–;5; 15; 50 (*N*=151)	1.3%					–

Figure B1*:* Effort across treatments **(ONLY Subjects Who Entered a Contract)**

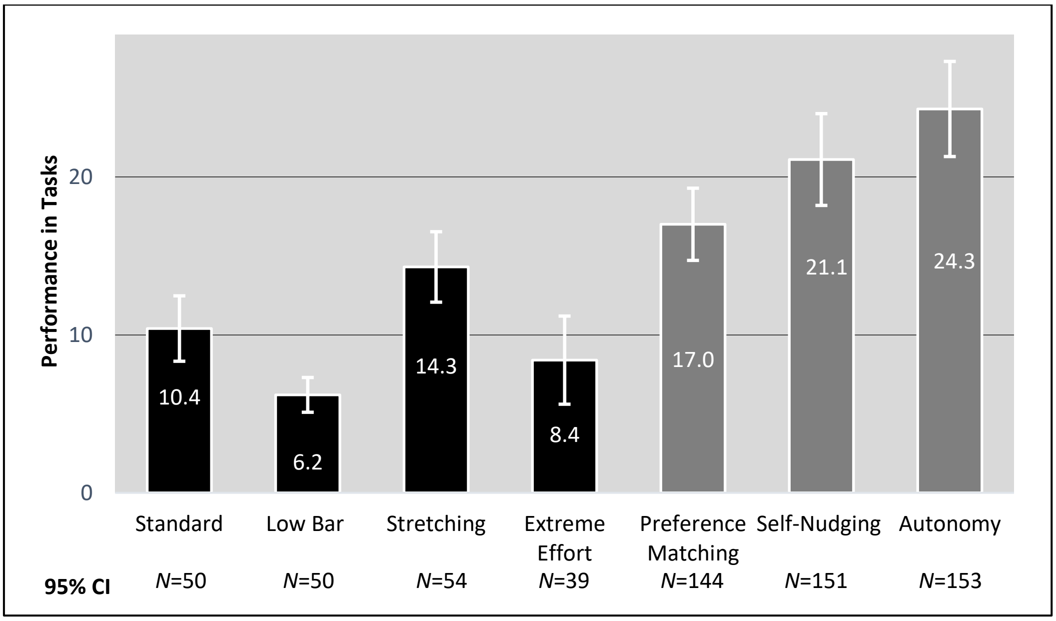
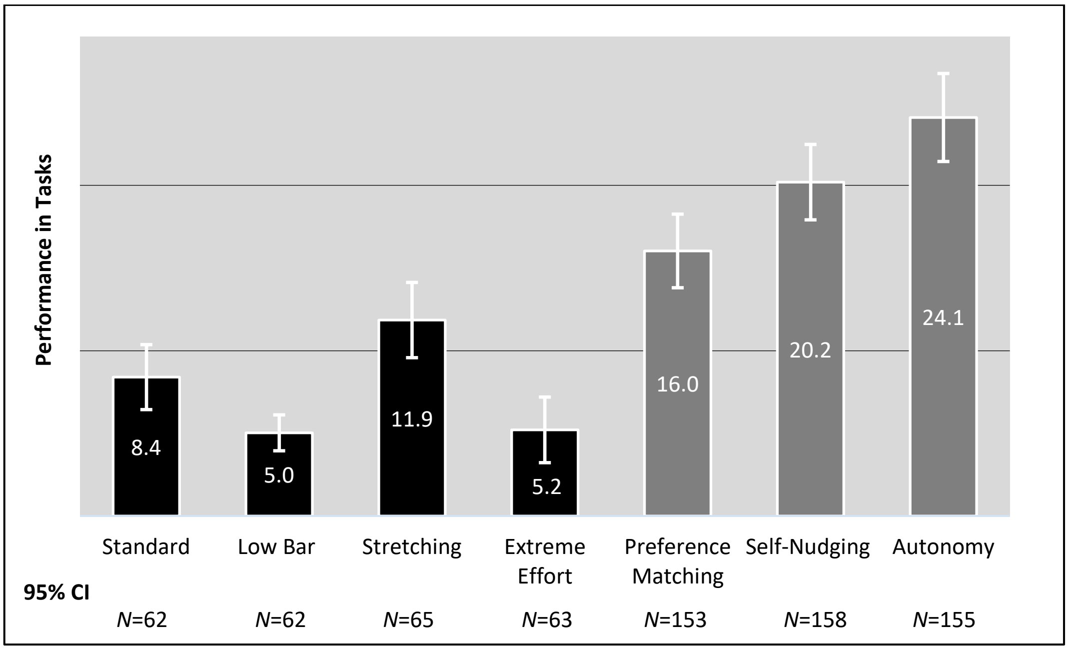


Figure B2: Effort across treatments (INCLUDING subjects who did not accept a contract)



Chapter 7 - Supplemental Materials

In the following I will provide the instructions used to carry out the studies. The presentation follows the structure of the chapters.

I. Instructions for Chapter 2 - Debiasing through Markets

Note, all treatments have an Endowment and a No-Endowment condition. In the main text I will always report the instructions of the No-Endowment condition. The manipulation that is the text that differs between the No-Endowment and Endowment conditions is reported in italics. You find the corresponding text for the Endowment condition in squared brackets directly after the text of the manipulation. The instructions for the Endowment condition are also set in italics. Only when it seems necessary, I present the whole text of the two conditions in separate sections.

I first (A) report the instructions for the two True-Valuation conditions; second (B) I present the two conditions of the Strategy treatment; under (C) you find the instructions for the Interaction treatment. Finally, under (D) I present the instructions for the last treatment Agency&Markets. At the end, under (E), I report the instructions for the regret and loss aversion measures.

A. Instructions for the TRUE VALUATION Treatment

1. GENERAL

Thank you very much for participating in this experiment! All necessary instructions will be presented on the screen. Note that you cannot use your login key more than once. If you logout before you have fully completed the experiment you will not be able to finish and receive a payment. You will be able to earn a sum of money, depending on the decisions you make. It is therefore very important that you read these instructions carefully.

1.1. Anonymity

In the study you will be given the opportunity to trade an online lottery ticket in a market. The receipt of the lottery ticket is sent to you before the experiment begins. It states all winnings numbers. Note however, you do not own the ticket. The receipt does not establish ownership. For claiming any winnings you need an authentication code. In the course of the experiment you will be presented with choice options. *If you choose the ticket you will be send the au-*

*thentication code. [**Endowment Condition**: Additionally, you are send an authentication code, which identifies you as the owner of the ticket and allows you to claim any winnings of the ticket.].* All data will be anonymous. Therefore, in order to receive the lottery ticket you need an anonymous e-mail account. Please go to this website and create an account for this study: https://securemail.hidemyass.com/

You do not have to provide any personal information. When prompted to state your "real" e-mail address make one up. Please use an alias for your new e-mail address. The account is created within 30 seconds. Please keep the new account open, until you have completed this study.

Please register here with the e-mail address of your new account:

@Hmamail.com

Once I have received your message I will send you *the receipt of the [**Endowment Condition**: your]* lottery ticket to the new e-mail address.

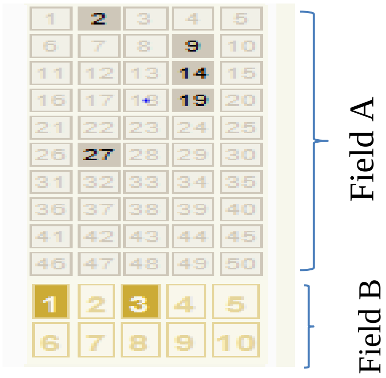
1.2. Payment

Your income is calculated in €. Your payment is going to be anonymous. You can either choose a pick up or payment via PayPal. You can pick up your earnings in the main office of the AStA in the University castle of Münster. You will be asked to make up a 5 digit code for identification. Your earnings will be put in an envelope marked with your code and deposited for pick up under this code. The employee who will had you the envelope was not involved in the study and does not know what content is in the envelope. Alternatively, you can be paid using PayPal if you register your @Hmamail.com mail address at PayPal. If you choose PayPal, your earnings will be sent to your Hmamail account.

2. LOTTERY

2.1. Scratchcards

In this study you can *receive [**Endow Condition**: trade]* a real lottery ticket in a market transaction. The lottery ticket was sent to your anonymous e-mail account. The header of the mail is: Lottery Ticket. Please open the e-mail. *The ticket receipt [**Endow Condition**: Your ticket]* looks like this:



The lottery ticket that you can receive in the market was selected randomly.

[***Endow Condition****: Your lottery ticket was assigned randomly to you]. I purchased the lottery tickets for the targeted number of experimental participants in advance. Then I randomly drew the tickets and numbered them following the order of the draw. The ticket selected first is sent to the first subject that starts with the experiment, the second is sent to the second participant and so forth.*

In the lottery, 5 numbers for field A ranging from 1 to 50 and 2 numbers in field B from 1 to 10 are drawn (see the picture). The rule of the game is therefore: "5 out of 50 plus 2 out of 10."

*As explained above, you do not own the ticket. The receipt does not establish ownership. For claiming any winnings you need to provide the authentication code. Owner is who can provide the code. In the course of the experiment you will be presented with choice options. If you choose the ticket you will be sent the authentication code [****Endow Condition****: Here is your authentication code for the lottery ticket. The code identifies you as the owners of the lottery ticket and allows you to collect any prize the ticket might win:*

Order Number: AN 54283402

The winning numbers are marked on the ticket. I used a computer program to randomly select the winning numbers for each ticket.

The lottery distributes several millions of Euro amongst the victors. The winning numbers are publicized on the lotteries` website; I will send you the link to the website of the lottery at the end of the study. Your lottery ticket is for this week`s draw.

2.2. Prizes

There are 12 different winning classes in the lottery. For a prize in the lowest winning class, 3 correct numbers (from both fields A & B) are required. For the top prize, all numbers have to be correct (five from field A and two from field

B). The table below shows the probability of winning and the expected winnings in the 12 classes (for one ticket).

Please confirm that you received and read the E-mail with *the receipt of the [**Endow Condition**: your]* lottery ticket. Please state below the first three winning numbers of the ticket you were sent. You will be paid for the experiment only if the numbers you submit are correct.

2	9	14

Rank	Number of Correct Numbers Required for a Winning	Probability of Winning	Profit Share*
Rank 1	(5 correct + 2 Euro numbers) -- TOP	1 : 59,325,280	34,000,000.00 EUR
Rank 2	(5 correct + 1 Euro numbers)	1 : 4,943,773	567,839.40 EUR
Rank 3	(5 correct + 0 Euro numbers)	1 : 3,955,019	54,756.30 EUR
Rank 4	(4 correct + 2 Euro numbers)	1 : 263,668	3,683.20 EUR
Rank 5	(4 correct + 1 Euro numbers)	1 : 21,972	242.90 EUR
Rank 6	(4 correct + 0 Euro numbers)	1 : 17,578	113.70 EUR
Rank 7	(3 correct + 2 Euro numbers)	1 : 5,992	56.10 EUR
Rank 8	(3 correct + 1 Euro numbers)	1 : 499	21.80 EUR
Rank 9	(2 correct + 2 Euro numbers)	1 : 418	15.00 EUR
Rank 10	(3 correct + 0 Euro numbers)	1 : 399	13.10 EUR
Rank 11	(1 correct + 2 Euro numbers)	1 : 80	9.70 EUR
Rank 12	(2 correct + 1 Euro numbers)	1 : 35	8.50 EUR

* The top-prize may be split.

3. THE MARKET

3.1. The Transaction

You are given the opportunity to *choose between an amount of money that is offered to you and the lottery ticket [**Endowment Condition**: trade your lottery ticket]* in a market. If you receive the ticket, then the experimenter will immediately sent you the authentication code making you the ticket owner.

The outcome of the market, does not depend on your decision alone, it also depends on a random price that will be drawn publicly. Contingent on your decision you will either *receive [**Endow Condition**: keep]* the lottery ticket or you will obtain an amount of money. Please note this is not a hypothetical transaction. Your pricing decisions are final and binding!

3.2. The Rules of the Market

You will be presented with a list of all prices between €0.25 and €10.00. [0.25; 0.50; 0.75…9.75; 10.00] You have to decide for each price whether you prefer to *obtain the money or the lottery ticket [**Endow Condition:** to sell or keep your lottery ticket].*

At a price of ...

€ 10.00:	○ I would choose the money	○ I would choose the lot. ticket
€ 9.75:	○ I would choose the money	○ I would choose the lot. ticket
...		
€ 5.50:	○ I would choose the money	○ I would choose the lot. ticket
€ 0.25:	○ I would choose the money	○ I would choose the lot. ticket

*[**For the Endow Condition**]*

At a price of ...

€ 10.00:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 9.75:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
...		
€ 0.50:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 0.25:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*

Your decisions will be compared with a random price that will be drawn in a public lottery after the experiment is over. You will be able to check the outcome online. If you stated that for the random price and all higher prices you prefer *to obtain the money [**Endow Condition**: to sell your lottery ticket]*, then you receive the random price. If you stated by contrast that you prefer *to receive [**Endow Condition**: to keep]* the lottery ticket at the drawn price and at all lower prices, then you *receive [**Endow Condition**: keep]* the lottery ticket *and the experimenter will send you right after the Eurojackpot draw the authentication code for the ticket* [***Endow Condition**: no text*]

Here are two examples:

(1) Assume the random price is 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price

At a price of ...

€ 10.00:	● I would choose the money	○ I would choose the lot. ticket
€ 9.75:	● I would choose the money	○ I would choose the lot. ticket
...		
€ 4.00:	○ I would choose the money	● I would choose the lot. ticket
€ 3.75:	○ I would choose the money	● I would choose the lot. ticket

...

The markings show that for the random price and for all lower prices, you want to obtain the lottery ticket. So the experimenter would send you the authentication code for the lottery ticket.

(2) Now assume that the random price would have been 4.25 Euro.

At a price of …

€ 10.00:	● I would choose the money	○ I would choose the lot. ticket
€ 9.75:	● I would choose the money	○ I would choose the lot. ticket
…		
€ 4.25:	● I would choose the money	○ I would choose the lot. ticket
€ 4.00:	○ I would choose the money	● I would choose the lot. ticket

In this case the list shows that for the random price and all higher prices, you want to obtain the money. So you would receive the random price and the experimenter pays you 4.25 Euro.

[For the Endow Condition]

Here are two examples:

(1) Assume the random price is 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price:

At a price of …

€ 10.00:	● *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 9.75:	● *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
…		
€ 4.00:	○ *I would sell the lot. ticket*	● *I would keep the lot. ticket*
€ 3.75:	○ *I would sell the lot. ticket*	● *I would keep the lot. ticket*
…		

The markings show that for the random price and all higher prices, you want to keep the lottery ticket.

(2) Now assume that the random price would have been 4.25 Euro.

At a price of …

€ 10.00:	● *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 9.75:	● *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
…		
€ 4.25:	● *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 4.00:	○ *I would sell the lot. ticket*	● *I would keep the lot. ticket*

In this case the markings show that for the random price and for all higher prices, you want to obtain the money. So you would sell your lottery ticket and would receive the random price in return; the experimenter pays you 4.25 Euro.

3.3. Determining the Random Prize

I use the last two numbers that are drawn in the online lottery to determine the random price. The last two numbers are all one-digit numbers, except the "10"; I transform "10" into a zero, so the numbers that can be drawn are:

0.1.2.3.4.5.6.7.8.9. I take the first number as the first digit of the price, so it determines the Euro amount; the second number serves as the second digit and determines the decimal place of Euro Cent; I add a “0” for the single Cent. Finally, I round up the price to the next full 25 Cent for instance 1.20 > €1.25 or 4.32 > €4.50.

Here are two examples: Assume the drawn numbers are: First Number: 9; Second Number: 8. Then the drawn price would be 9.80 Euro. Rounding up to the next full 25 Cent gives us a random price of exactly 10 Euro. Here is a second example: First Number: 10; Second Number: 6. This leads to a price of 0.60 Euro, transforming “10” into “0”. Since I round up to the next full 25 Cent I get a random price of 0.75 Euro. The random price can range from 0.25 Cent up to 10 Euro with each price having an equal chance to be drawn.

Please confirm by checking the box that you agree with the rules of the market.

4. CONTROL QUESTIONS

To ensure that you fully understand the rules of the market, I will present you with a set of short scenarios. Please select the correct answer (notice, you can only proceed with the experiment, if you answer the questions correctly):

(1) The random price drawn in the lottery is €4.00. Max has stated that for €3.00 and all higher prices he prefers to obtain the money *[**Endow Condition**: wants to sell his ticket]*. **What happens?**

- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*
- ○ Max receives the lot. ticket. *[**Endow Condition**: Max keeps the ticket.]*
- ○ Max receives €3.00. *[**Endow Condition**: Max sells his ticket for €3.00.]*

(2) The random price drawn in the lottery is €4.00. Max has stated that for €4.50 and all higher prices he prefers *to obtain the money [**Endow Condition**: to sell his ticket]*. **What happens?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(3) The random price drawn in the lottery is €4.00. Assume now that Max has overstated the price. He indicated that for €4.50 and all higher prices he prefers *to obtain the money [**Endow Condition**: to sell his ticket]*. In truth however, he prefers *to obtain the money [**Endow Condition**: wants to sell his ticket]* already at a price of €4.00 and all higher prices. **What happens?**

- ○ Max receives €4.50 [***Endow Condition****: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(4) **What happens if Max states his true preference of €4.00?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(5) **What happens if €5.00 is drawn as the random price?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(6) The random price drawn in the lottery is €4.00. Assume now that Max has understated the price. He indicated that for €4.00 and all higher prices he prefers *to obtain the money [**Endow Condition**: wants to sell his ticket]*. In truth however, he prefers *to obtain the money [**Endow Condition**: to sell his ticket]* only at a price of 4.50 and all higher prices. What happens?

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(7) What happens if Max would have stated his true preference of €4.50?

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(8) What happens if €5.00 was drawn as the random price?

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

5. DECISION

At what price do you prefer the money over the ticket?

Please state for **each** of the listed prices whether you would like to receive the money or obtain the lottery ticket. Your decisions are binding!

At a price of …

€ 10.00: ○ I would choose the money ○ I would choose the lot. ticket
€ 9.75: ○ I would choose the money ○ I would choose the lot. ticket
€ 0.25: ○ I would choose the money ○ I would choose the lot. ticket

[For the Endow Condition]:

At what price do you want to sell your ticket?

*Please indicate for **each** of the listed prices whether you would like to sell the lottery ticket or keep the ticket. Your decisions are binding!*
***At a price of**…*
€ 10.00: ○ I would sell the lot. ticket ○ I would keep the lot. ticket
€ 9.75: ○ I would sell the lot. ticket ○ I would keep the lot. ticket
…
€ 0.25: ○ I would sell the lot. ticket ○ I would keep the lot. ticket

6. SECOND MARKET

Please assume that you can *choose between the ticket and the money [**Endow Condition**: sell your ticket]* in a market just as you did before. A random price is drawn and you *receive the money [**Endow Condition**: sell your ticket*], if you have preferred the money over the ticket for this price. The rules of the market are the same as in the first market you just participated in, except for one rule: In this market, when you receive the money *[**Endow Condition**: sell your ticket]*, then you are paid the lowest price for which you *prefer the money [**Endow Condition**: are willing to sell],* not the random price.

Assume for example the lowest price for which you *prefer the money over the ticket [**Endow Condition:** are willing to sell your ticket]* is 4€ and a random price of 5€ is drawn. In this case you would receive 4€. If *the lowest price for which you prefer the money would be 6 € [**Endow Condition:** would demand 6€ for selling your ticket]* - one Euro more than the random price - then you would keep your ticket. Let`s assume now that you overstate your valuation and indicate that you prefer the money over the ticket at a price of 5€ *[**Endow Condition**: ask for 5€ for selling your* ticket], while your actual valuation is only 4€. In this case you would receive 5€. However, if you would indicate to prefer money over the ticket at 6€ *[**Endow Condition:** ask for 6€ for selling your ticket],* while your true valuation is in fact 5€, then you would *receive the ticket [**Endow Condition:** have to keep your ticket],* even though you would have preferred the money over the ticket at *[**Endow Condition:** to sell the ticket for]* the random price of 5€.

Just as in the first market above I ask you now
at what price do you prefer the money over the ticket?
Please state for **each** of the listed prices below whether you would like to receive the money or obtain the lottery ticket instead. Your decisions are binding!

At a price of …
€ 10.00: ○ I would choose the money ○ I would choose the ticket
€ 9.75: ○ I would choose the money ○ I would choose the ticket

…..
€ 0.25: ○ I would choose the money ○ I would choose the ticket

[For the Endow Condition]:
At what price do you want to sell your ticket?
*Please indicate for **each** of the listed prices whether you would like to sell or keep the lottery ticket. Your decisions are binding!*
***At a price of**…*
€ 10.00: ○ I would sell the lot. Ticket ○ I would keep the lot. ticket
€ 9.75: ○ I would sell the lot. Ticket ○ I would keep the lot. ticket
….
€ 0.25: ○ I would sell the lot. Ticket ○ I would keep the lot. ticket

B. Instructions for the STRATEGY Treatment - (Endowment & No-Endowment)

Note that I only report the section about "The Rules of the Market". All other passages of the instructions do not differ from the True-Valuation treatment that I presented above.

3.2. The Rules of the Market

You will be presented with a list of all prices between €0.25 and €10.00. [0.25; 0.50; 0.75...9.75; 10.00] You have to decide for each price whether you prefer *to obtain the money or the lottery ticket [**Endow Condition:** to sell or keep your lottery ticket].*

At a price of ...

€ 10.00: ○ I would choose the money ○ I would choose the lot. ticket

€ 9.75: ○ I would choose the money ○ I would choose the lot. ticket

...

*[**For the Endow Condition**]*

***At a price of** ...*

€ 10.00: ○ I would sell the lot. ticket ○ I would keep the lot. ticket

€ 9.75: ○ I would sell the lot. ticket ○ I would keep the lot, ticket

Your decisions will be compared with a random price that will be drawn in a public lottery after the experiment is over. If you stated that for the random price you prefer *to obtain the money [**Endow Condition**: to sell your lottery ticket]*, then you are paid the lowest price for which you *prefer the money [**Endow Condition**: are willing to sell]*. If you stated by contrast that you prefer *to receive [**Endow Condition**: to keep]* the lottery ticket at the drawn price and at all lower prices, then you *receive [**Endow Condition**: keep]* the lottery ticket *and the experimenter will send you the authentication code for the ticket* [***Endow Condition**: no text*].

Here are two examples:

(1) Assume the random price is 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price:

At a price of ...

€ 10.00: ● I would choose the money ○ I would choose the ticket

€ 9.75: ● I would choose the money ○ I would choose the ticket

...

€ 4.00: ○ I would choose the money ● I would choose the ticket

€ 3.75: ○ I would choose the money ● I would choose the ticket

The list shows that for the random price and all lower prices, you want to obtain the lottery ticket. So the experimenter would send you the authentication code for the lottery ticket.

(2) Now assume that the random price would have been 4.50 Euro.

At a price of …

€ 10.00:	● I would choose the money	○ I would choose the ticket
€ 9.75:	● I would choose the money	○ I would choose the ticket
….		
€ 4.50:	● I would choose the money	○ I would choose the ticket
€ 4.25:	● I would choose the money	○ I would choose the ticket
€ 4.00:	○ I would choose the money	● I would choose the ticket

In this case the list shows that for the random price of €4.50, you want to obtain the money. So you would receive the lowest price for which you in fact prefer to obtain the money; the experimenter pays you 4.25 Euro.

[For the Endow Condition]

(1) Assume the random price is 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price:

***At a price of** …*

€ 10.00:	● *I would sell the lot. ticket*	○ *I would keep the ticket*
€ 9.75:	● *I would sell the lot. ticket*	○ *I would keep the ticket*
…		
€ 4.00:	○ *I would sell the lot. ticket*	● *I would keep the ticket*
€ 3.75:	○ *I would sell the lot. ticket*	● *I would keep the ticket*

The list shows that for the random price and all lower prices, you want to keep your lottery ticket.

(2) Now assume that the random price would have been 4.50 Euro.

***At a price of** …*

€ 10.00:	● *I would sell the lot. ticket*	○ *I would keep the ticket*
€ 9.75:	● *I would sell the lot. ticket*	○ *I would keep the ticket*
….		
€ 4.50:	● *I would sell the lot. ticket*	○ *I would keep the ticket*
€ 4.25:	● *I would sell the ticket*	○ *I would keep the ticket*
€ 4.00:	○ *I would sell the ticket*	● *I would keep the ticket*

In this case the list shows that for the random price of €4.50, you want to sell your lottery ticket. So you would receive the lowest price for which you in fact are willing to sell the ticket; the experimenter pays you 4.25 Euro.

4. CONTROL QUESTIONS

To ensure that you fully understand the rules of the market, I will present you with a set of short scenarios. Please select the correct answer (notice, you can only proceed with the experiment, if you answer the questions correctly):

(1) The random price drawn in the lottery is €4.00. Max has stated that for €3.00 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. **What happens?**

- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €3.00. *[**Endow Condition**: Max sells his ticket for €3.00.]*

(2) The random price drawn in the lottery is €4.00. Max has stated that for €4.50 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. **What happens?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(3) The random price drawn in the lottery is €4.00. Assume now that Max has overstated the price. He indicated that for €4.50 and all higher prices he prefers *to obtain the money [**Endow Condition**: to sell his ticket]*. In truth however, he prefers to *obtain the money [**Endow Condition**: wants to sell his ticket]* already for a price of 4.00 and all higher prices. **What happens?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(4) **What happens if Max would have stated his true preference of €4.00?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(5) **What happens if €5.00 was drawn as the random price?**

- ● Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(6) The random price drawn in the lottery is €4.00. Assume now that Max has understated the price. He indicated that for €4.00 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. In truth

however, he prefers to *obtain the money [**Endow Condition**: to sell his ticket]* only at a price of 4.50 and all higher prices. **What happens?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(7) **What happens if Max would stated his true preference of €4.50?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(8) **What happens if €5.00 was drawn as the random price?**

- ○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
- ○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
- ● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

5. DECISION

At what price do you prefer the money over the ticket? Please state for **each** of the listed prices whether you would like to receive the money or obtain the lottery ticket. Your decisions are binding!

At a price of …

€ 10.00:	○ I would choose the money	○ I would choose the lot. ticket
€ 9.75:	○ I would choose the money	○ I would choose the lot. ticket
…		
€ 0.25:	○ I would choose the money	○ I would choose the lot. ticket

*[**For the Endowment Condition**]:*
At what price do you want to sell the ticket?
*Please indicate for **each** of the listed prices whether you would like to sell the lot. ticket or keep the ticket. Your decisions are binding!*
At a price of…

€ 10.00:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
€ 9.75:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*
…		
€ 0.25:	○ *I would sell the lot. ticket*	○ *I would keep the lot. ticket*

6. SECOND MARKET

Please assume that you can *choose between the ticket and the money [**Endow Condition**: sell your ticket]* in a market just as you did before. A random price is drawn and you *receive the money [**Endow Condition**: sell your ticket*], if you have preferred the money over the ticket *[**Endow Condition**: to sell your ticket*] for this price. The rules of the market are the same as in the first market you just participated in, except for one rule: In this market, when you receive the money *[**Endow Condition**: sell your ticket]*, then you are paid the random price not the lowest price for which you *prefer the money [**Endow Condition**: are willing to sell]*.

Assume for example the lowest price for which you *prefer the money over the ticket [**Endow Condition:** are willing to sell your ticket for]* is 4€ and a random price of 5€ is drawn. In this case you would receive 5€. If *the lowest price for which you prefer the money would be 6 € [**Endow Condition:*** you *would demand 6€ for selling your ticket]* - one Euro more than the random price - then you would keep your ticket. Let`s assume now that you overstate your valuation and indicate that you prefer the money over the ticket at a price of 5€ *[**Endow Condition**: ask for 5€ for selling your* ticket], while your actual valuation is only 4€. In this case you would still receive the random price of 5€. However, if you would indicate to prefer money over the ticket at 6€ *[**Endow Condition:** ask for 6€ for selling your ticket],* while your true valuation is in fact 5€, then you would *receive the ticket [**Endow Condition:** have to keep your ticket],* even though you would have preferred the money over the ticket at *[**Endow Condition:** to sell the ticket for]* the random price of 5€.

Just as in the market before I ask you now:
At what price do you prefer the money over the ticket?
Please state for **each** of the listed prices whether you would like to receive the money or obtain the lottery ticket. Your decisions are binding!
At a price of …

€ 10.00:	○ I would choose the money	○ I would choose the lot. ticket
€ 9.75:	○ I would choose the money	○ I would choose the lot. ticket
…		
€ 0.25:	○ I would choose the money	○ I would choose the lot. ticket

*[**For the Endowment Condition**]:*
At what price do you want to sell your ticket?
*Please indicate for **each** of the listed prices whether you would like to sell the lottery ticket or keep the ticket. Your decisions are binding!*
At a price of

€ 10.00:	○ *I would sell the ticket*	○ *I would keep the ticket*
€ 9.75:	○ *I would sell the ticket*	○ *I would keep the ticket*
…		
€ 0.25:	○ *I would sell the ticket*	○ *I would keep the ticket*

C. Instructions for the INTERACTION Treatment
[Endowment & No-Endowment]

Note that I only report the sections about the "Market Transaction" and "The Rules of the Market". All other parts of the instructions do not differ from the True-Valuation treatment that I presented above (under A).

(1) INSTRUCTIONS FOR THE MAIN SUBJECTS

3. The Market

3.1. Market Transaction

You are given the opportunity to *choose between an amount of money and the lottery ticket [**Endow Condition**: to trade your lottery ticket]* in a market. *If you receive the ticket, then the experimenter will immediately send you the authentication code making you the owner of the ticket [**Endow Condition**: no text]*.

The outcome of the market does not depend on your decision alone, it also depends on a second participant who is randomly selected and assigned to you. Contingent on your decision and the decision of your partner you will either *receive [**Endow Condition**: keep]* the lottery ticket or you will obtain an amount of money. Please note this is not a hypothetical transaction. Your pricing decisions and the decisions of your partner are final and binding!

3.2. The Rules of the Market

You will be presented with a list of all prices between €0.25 and €10.00. [0.25; 0.50; 0.75…9.75; 10.00] You have to decide for each price whether you prefer to *obtain the money or the lottery ticket [**Endow Condition:** to sell or keep your lottery ticket].*

At a price of …

€ 10.00:	○ I would choose the money	○ I would choose the ticket
€ 9.75:	○ I would choose the money	○ I would choose the ticket

[For the Endowment Condition]

At a price of *…*

€ 10.00:	○ *I would sell the ticket*	○ *I would keep the ticket*
€ 9.75:	○ *I would sell the ticket*	○ *I would keep the ticket*

…

Your decisions will be compared with the highest price that your partner is willing to pay for the ticket. I refer to this price as the **offer** of your partner. If you stated that for your partner`s offer price you prefer to *obtain the money [**Endow Condition**: to sell your lottery ticket]*, then you are paid the lowest price for which you prefer the money. If you stated by contrast that you prefer to *receive [**Endow Condition**: to keep]* the lottery ticket at the price that your partner offers, then you *receive [**Endow Condition**: keep]* the lottery ticket *and the experimenter will send you authentication code for the ticket [**Endow Condition**: no text.*]

Here are two examples:

(1) Assume your partner has offered a price of 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price:

At a price of …

€ 10.00:	●	I would choose the money	○	I would choose the ticket
€ 9.75:	●	I would choose the money	○	I would choose the ticket
…				
€ 4.00:	○	I would choose the money	●	I would choose the ticket
€ 3.75:	○	I would choose the money	●	I would choose the ticket
…				

The list shows that for the price of €4.00 that your partner offers, you want to obtain the lottery ticket. So the experimenter would send you the authentication code for the lottery ticket.

(2) Now assume that your partner has offered 4.50 Euro.

At a price of …

€ 10.00:	●	I would choose the money	○	I would choose the ticket
€ 9.75:	●	I would choose the money	○	I would choose the ticket
….				
€ 4.50:	●	I would choose the money	○	I would choose the ticket
€ 4.25:	●	I would choose the money	○	I would choose the ticket
€ 4.00:	○	I would choose the money	●	I would choose the ticket

In this case the list shows that for the price of €4.50 that your partner offers, you want to obtain the money. So you would receive the lowest price for which you in fact prefer to obtain the money; the experimenter pays you 4.25 Euro.

[For the Endowment Condition]

(1) Assume your partner has offered a price of 4 Euro. Then the experimenter will check in your price list, what decision you have made for this price:

***At a price of** …*

€ 10.00:	●	*I would sell the ticket*	○	*I would keep the ticket*
€ 9.75:	●	*I would sell the ticket*	○	*I would keep the ticket*
…				
€ 4.00:	○	*I would sell the ticket*	●	*I would keep the ticket*
€ 3.75:	○	*I would sell the ticket*	●	*I would keep the ticket*
…				

The list shows that for the price of €4.00 that your partner offers, you want to obtain the lottery ticket. So the experimenter would send you the authentication code for the lottery ticket.
(2) Now assume that your partner has offered 4.50 Euro.
***At a price of** …*

€ 10.00:	●	*I would sell the ticket*	○	*I would keep the ticket*
€ 9.75:	●	*I would sell the ticket*	○	*I would keep the ticket*
….				
€ 4.50:	●	*I would sell the ticket*	○	*I would keep the ticket*
€ 4.25:	●	*I would sell the ticket*	○	*I would keep the ticket*
€ 4.00:	○	*I would sell the ticket*	●	*I would keep the ticket*

In this case the list shows that for the price of €4.50 that your partner offers, you want to sell your lottery ticket. So you would receive the lowest price for which you in fact are willing to sell the ticket; the experimenter pays you 4.25 Eur

4. CONTROL QUESTIONS

To ensure that you fully understand the rules of the market, I will present you with a set of short scenarios. Please select the correct answer (notice, you can only proceed with the experiment, if you answer the questions correctly):

(1) Max stated that for €3.00 and all higher prices he prefers to *obtain the money* *[**Endow Condition**: to sell his ticket]*. His partner has offered a price of €4.00. **What happens?**

○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*
○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
● Max receives €3.00. *[**Endow Condition**: Max sells his ticket for €3.00.]*

(2) Max has stated that for €4.50 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. His partner has offered a price of €4.00. **What happens?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(3) Assume now that Max has overstated the price. He indicated that for €4.50 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. In truth however, he prefers to *obtain the money [**Endow Condition**: wants to sell his ticket]* already for a price of 4.00 and all higher prices. His partner has offered a price of €4.00. **What happens?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(4) **What happens if Max would have stated his true preference of €4.00?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(5) **What happens if €5.00 was drawn as the random price?**

● Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(6) Assume now that Max has understated the price. He indicated that for €4.00 and all higher prices he prefers to *obtain the money [**Endow Condition**: to sell his ticket]*. In truth however, he prefers to *obtain the money [**Endow Condition**: to sell his ticket]* only at a price of 4.50 and all higher prices. His partner has offered a price of €4.00. **What happens?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(7) **What happens if Max would have stated his true preference of €4.50?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
● Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

(8) **What happens if €5.00 was drawn as the random price?**

○ Max receives €4.50 *[**Endow Condition**: Max sells his ticket for €4.50.]*
○ Max receives the ticket *[**Endow Condition**: Max keeps the ticket.]*.
● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*

5. DECISION

At what price do you prefer the money over the ticket?
Please state for **each** of the listed prices whether you would like to receive the money or obtain the lottery ticket. Your decisions are binding!
At a price of …

€ 10.00:	○ I would choose the money	○ I would choose the ticket
€ 9.75:	○ I would choose the money	○ I would choose the ticket
…		
€ 0.25:	○ I would choose the money	○ I would choose the ticket

[For the Endowment Condition]:
At what price do you want to sell the ticket?
*Please indicate for **each** of the listed prices whether you would like to sell the lottery ticket or keep the ticket. Your decisions are binding!*
At a price of...
€ 10.00: ○ I would sell the lot. ticket ○ I would keep the lot. ticket
€ 9.75: ○ I would sell the lot. ticket ○ I would keep the lot. ticket
...
€ 0.25: ○ I would sell the lot. ticket ○ I would keep the lot. ticket

6. SECOND MARKET

Please assume that you can *choose between the ticket and the money [**Endow Condition**: sell your ticket]* in a market just as you did before. However, this time you are not matched with a real partner. Instead of a real partner making an offer, a random price is drawn from an urn. Each price between 0.25, 050, 0.75 ...9.75, 10 is represented once in the urn. The rice you indicate is compared with the random price. You *receive the money [**Endow Condition**: sell your ticket*], if you have preferred the money over the ticket *[**Endow Condition**: to sell your ticket*] for this random price. When you *receive the money [**Endow Condition**: sell your ticket]*, then you are paid the random price.

Assume for example the lowest price for which you *prefer the money over the ticket [**Endow Condition:** are willing to sell your ticket for]* is 4€ and a random price of 5€ is drawn. In this case you would receive 5€. If *the lowest price for which you prefer the money would be 6 € [**Endow Condition:*** you *would demand 6€ for selling your ticket]* - one Euro more than the random price - then you would keep your ticket. Let`s assume now that you overstate your valuation and indicate that you *prefer the money over the ticket at a price of 5€ [**Endow Condition**: ask for 5€ for selling your* ticket], while your actual valuation is only 4€. In this case you would still receive the random price of 5€. However, if you would *indicate to prefer money over the ticket at 6€ [**Endow Condition:** ask for 6€ for selling your ticket],* while your true valuation is in fact 5€, then you would *receive the ticket [**EndowCondition:** have to keep your ticket],* even though you would have preferred *the money over the ticket at [**Endow Condition:** to sell the ticket for]* the random price of 5€.
Just as in the first market before I ask you now

At what price do you prefer the money over the ticket?
Please state for **each** of the listed prices whether you would like to receive the money or obtain the lottery ticket. Your decisions are binding!
At a price of ...
€ 10.00: ○ I would choose the money ○ I would choose the ticket
€ 9.75: ○ I would choose the money ○ I would choose the ticket

...

€ 0.25: ○ I would choose the money ○ I would choose the ticket

*[**For the Endowment Condition**]:*

At what price do you want to sell your ticket?

*Please indicate for **each** of the listed prices whether you would like to sell the lottery ticket or keep the ticket. Your decisions are binding!*

***At a price of**...*

€ 10.00: ○ I would sell the lot. ticket ○ I would keep the ticket

€ 9.75: ○ I would sell the lot. ticket ○ I would keep the ticket

...

€ 0.25: ○ I would sell the lot. ticket ○ I would keep the ticket

(2) Instructions Main Subjects Receive about their Partners

In the following I present the information Subjects were given about their Partner`s Instructions in the Interaction Treatment. The instructions are identical for both conditions Endowment and No-Endowment.

1. Your Partner`s Instructions

Your partner is asked at what price he would buy the lottery ticket. He received an endowment of 10 Euro. He can spend any portion of his endowment on buying the ticket; he keeps the rest of the endowment that he does not spend.
He will be presented with a list of all prices between €0.25 and €10.00 and has to decide for each price whether he wants to buy the lottery ticket or not.

2. Your Partner`s Payoff

Your partner buys the lottery ticket, if he states that he values the ticket at a higher price or at the same price as you. As any buyer in the real world, your partner keeps the money that he does not spend on buying the ticket. Thus he earns the more the less he spends on the ticket. However, if he offers a price for the ticket that is lower than the price you indicate then he does not get the ticket.

(3) Instructions for the Interaction Partner

The instructions are identical for all conditions. I only report the relevant sections that differ from the instructions that the subjects received.

3.2. The Rules of the Market

You are given an endowment of 10 Euro for which you can buy a lottery ticket in a real market. You can spend any portion of your endowment on buying the ticket; you keep the rest of the endowment that you did not spend on the transaction.

You will be presented with a list of all prices between €0.25 and €10.00. [0.25; 0.50; 0.75…9.75; 10.00] You have to decide for each price whether you want to buy the lottery ticket or not.

At a price of …

€ 10.00:	○	I would buy the ticket	○	I would keep the money
€ 9.75:	○	I would buy the ticket	○	I would keep the money
…				
€ 5.50:	○	I would buy the ticket	○	I would keep the money
€ 0.25:	○	I would buy the ticket	○	I would keep the money

3.3. Your Payoff

You buy the lottery ticket, if you state a higher price for the ticket as your partner or the same price. Otherwise no transaction takes place and you will keep the money, but will not get a lottery ticket.

Here are examples:

Assume you have stated to value the lottery ticket at a price of 4€.

(1) Your partner indicates that he prefers to receive the money over the ticket at a price of 3.75€. Then you get the ticket and keep a rest of 6 Euro of your endowment. Your partner is paid €3.75.

(2) Your partner indicates that he prefers to receive the money over the ticket at a price of 4.25€. Then the transaction fails and you keep your full endowment. Your partner gets the lottery ticket.

D. Instructions for the Agency&Markets Treatment

[Endow & No-Endowment]

Note that I only report the sections about the “Market Transaction”. All other passages of the instructions do not differ from the Strategy treatment that I presented above (A).

(1) Instructions for the Main Subjects

3.1. Market Transaction

You are given the opportunity to *choose between an amount of money that is offered to you and the lottery ticket [**Endow Condition**: trade your lottery ticket]* in a market. If you receive the ticket, then the experimenter will immediately sent you the authentication code making you the ticket owner.

You are assigned an agent, who is acting for you in the market. You instruct the agent with the pricing decision you make. The agent`s payment is conditioned on following your instruction. When implementing your pricing decision the agent will be paid €2 otherwise he receives nothing. Other than this payment, the agent does not have any own monetary interest in the transaction. However, the agent is not bound; he alone decides for what price *to choose the money over the ticket [**Endow Condition**: to sell the ticket]* and his decision is final. The price you select is the instruction for the agent. The outcome of the market, does not depend on the agent`s decision alone, it also depends on a random price that will be drawn publicly. Contingent on the agent`s decision you will either *receive [**Endow Condition**: keep]* the lottery ticket or you will obtain an amount of money. Please note this is not a hypothetical transaction. The agent`s decisions are final and binding!
[…..]

The agent`s decisions will be compared with a random price that will be drawn in a public lottery after the experiment is over. If the agent stated that for the random price and all higher prices you prefer *to obtain the money [**Endow Condition**: to sell your lottery ticket]*, then you receive the random price. If the agent stated by contrast that you prefer *to receive [**Endow Condition**: to keep]* the lottery ticket at the drawn price and at all lower prices, then you *receive [**Endow Condition**: keep]* the lottery ticket *and the experimenter will send you right after the Eurojackpot draw the authentication code for the ticket* [***Endow Condition**: no text*]

Here are two examples:
(1) Assume the random price is 4 Euro. Then the experimenter will check, what decision the agent has made for this price:
At a price of …

€ 10.00:	● I would choose the money	○ I would choose the lot. ticket
€ 9.75:	● I would choose the money	○ I would choose the lot. ticket
…		
€ 4.00:	○ I would choose the money	● I would choose the lot. ticket
€ 3.75:	○ I would choose the money	● I would choose the lot. ticket
…		

The list shows that for the random price and for all lower prices, you want to obtain the lottery ticket. If the agent follows your instructions, the experimenter would send you the authentication code for the lottery ticket.

4. CONTROL QUESTIONS

To ensure that you fully understand the rules of the market, I will present you with a set of short scenarios. Please select the correct answer (notice, you can only proceed with the experiment, if you answer the questions correctly):

(1) The random price drawn in the lottery is €4.00. Max has stated that for €3.75 and all higher prices he prefers to obtain the money [Endow Condition: wants to sell his ticket]. The agent follows his instructions. What happens?

● Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*
○ Max receives the lottery ticket. *[**Endow Condition**: Max keeps the ticket.]*
○ Max receives €3.00. *[**Endow Condition**: Max sells his ticket for €3.00.]*

(2) What happens if the agent would have stated that Max prefers €4.25 instead of the €3.75 he instructed the agent to indicate?

○ Max receives €4.00. *[**Endow Condition**: Max sells his ticket for €4.00.]*
● Max receives the lottery ticket. *[**Endow Condition**: Max keeps the ticket.]*
○ Max receives €3.00. *[**Endow Condition**: Max sells his ticket for €3.00.]*

6. SECOND MARKET

Please assume that you can *choose between the ticket and the money [**Endow Condition**: sell your ticket]* in a market just as you did before. Your agent is still acting for you; he is paid €2 only, if he follows your instructions; otherwise he receives nothing. Other than this payment, he does not have any own monetary interest in the transaction. A random price is drawn and you *receive the money [**Endow Condition**: sell your ticket*], if you have indicated to prefer the money over the ticket *[**Endow Condition**: to sell your ticket*] for this price and the agent implemented your instructions. The rules of the market are the same as in the first market you just participated in, except for one rule: In this market, when you receive the money *[**Endow Condition**: sell your ticket]*, then you are paid the random price not the lowest price for which you *prefer the money [**Endow Condition**: are willing to sell]*.

Notice, the agent`s decision is final; whether he follows or does not follow your instructions you will receive money or ticket according to his decision. In the following examples I assume that the agent implements your instructions. Assume for example the lowest price for which you *prefer the money* *over the ticket [**Endow Condition:** are willing to sell your ticket]* is 4€ and a random price of 5€ is drawn. In this case you would receive 4€. If

….

(2) Instructions for the Agent

I report only the main sections of the instructions that differ from the instructions of the other subjects.

1. LOTTERY

In this study you will act for a different participant. This participant can *receive [**Endow Condition**: trade]* a real lottery ticket in a market transaction depending on the decisions you make. You will be informed about the other participant`s instructions. When you follow the participant's instructions you are paid €2, if not, you do not receive any payment. However, you are not bound by the participant's instructions; your decision is final and according to your decision the participant will receive either money or ticket.

3.1. Market Transaction

You are supposed to choose in a market between an amount of money that the other participant is offered and the lottery ticket he will receive [**Endow Condition**: trade your lottery ticket].

The outcome of the market, does not depend on your decision alone, it also depends on a random price that will be drawn publicly. Contingent on your decision the other participant will either receive [**Endow Condition**: keep] the lottery ticket or he will obtain an amount of money. Please note this is not a hypothetical transaction. Your pricing decisions are final and binding!

3.2. The Rules of the Market

You will be presented with a list of all prices between €0.25 and €10.00. [0.25; 0.50; 0.75…9.75; 10.00] You have to decide for each price whether the participant shall obtain the money or the lottery ticket [**Endow Condition**: to sell or keep your lottery ticket].

At a price of …

€ 10.00: I choose subject gets the money   I choose subject gets the ticket

€ 9.75: I choose subject gets the money   I choose subject gets the ticket

…

E. Additional Measures

1. REGRET [No-Endow Condition]

1.1. Regret over *Choosing the Money*

Assume that you choose the money over the ticket. When you choose the money, you cannot collect a prize. How much regret do you anticipate to feel over choosing the money while being aware that you forgo the prize?
Please mark one of the boxes you see below. Low values express little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

1.2. Regret over *Choosing the Ticket*

Assume that you *choose the ticket over the money*. When you *choose the ticket*, you cannot collect the money. How much regret do you anticipate to feel over *choosing the ticket* while being aware that you forgo the money?
Please mark one of the boxes you see below. Low values express little regret; high values represent a strong feeling of regret

1	2	3	4	5	6	7	8	9	10

[Endow Condition]

1.1. Regret over *Selling the Ticket*

Assume that you sell your ticket. When you sell the ticket, you will not be eligible to collect a prize. How much regret do you anticipate to feel over selling the ticket while being aware that you forgo winning a prize?
Please mark one of the boxes you see below. Low values express little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

1.2. Regret over *Keeping the Ticket*

Assume that you keep your ticket. When you keep the ticket, you cannot collect a sales price. How much regret do you anticipate to feel over keeping the ticket while being aware that you forgo the sales price?
Please mark one of the boxes you see below. Low values express little regret; high values represent a strong feeling of regret

1	2	3	4	5	6	7	8	9	10

2. LOSS AVERSION

2.1. General Rules

You are given the option to participate in two lotteries. To enable you to participate, you receive an endowment of 2.50€. You can increase your earnings when you participate in the lotteries, but you can also lose your endowment. You can decide separately for each lottery whether you want to participate in it or not. So you may decline to participate in one lottery, but take part in the other; you can also reject both lotteries, or participate in both of them.

The outcome of both lotteries will be determined by the public draw of the online lottery. The first drawn number decides the outcome of the first lottery, the second number determines the outcome of the second lottery. You win either lottery if the drawn number is even and you lose either lottery if the drawn number is uneven.

You will only be paid for one of the two lotteries. Which lottery is paid for, will also be determined randomly by the online lottery. If the third number drawn in the online lottery is uneven, then the first lottery is selected for payment, if it turns out to be even, then the second lottery is selected.

2.2. Lottery 1

Lottery 1 has the following payoff: You win €4 with a probability of ½ and you lose €2.50 with a probability of ½. If you reject to participate in Lottery 1 then you neither win an additional payoff, nor can you lose your endowment.

Please click on the “Yes” button if you want to participate in the lottery and the “No” button if you do not want to participate in Lottery 1.

Yes　　No

2.3. Lottery 2

Lottery 2 has the same payoff as Lottery 1: You win €4 with a probability of ½ and lose €2.50 with a probability of ½. However, the lottery is repeated for six times. You are paid the average of the six outcomes.

Here is an example: Assume you win the lottery three times (3x€4) and you lose the lottery three times (3x€2.50). In this case the overall payoff for Lottery 2 would be: $\frac{(3x4)-(3x2.50)}{6}$ = €0.75

If you reject to participate in Lottery 2 then you neither win an additional pay-off, nor can you lose your endowment.

Please click on the “Yes” button if you want to participate in the lottery and the “No” button if you do not want to participate in Lottery 2.

Yes No

II. Instructions for Chapter 3 - Debiasing through Social Context

The instructions are condensed by omitting introductory parts that are not relevant for the experiment. The instructions for Experiment 1 show the no-endowment and maximum price treatment. The instructions for Experiment 2 show the endowment and summed price treatment and the endowment and maximum price treatments [the changes for the latter treatment are shown in italics and squared brackets]. This reflects the core manipulation of the factors endowment and outcome scheme.

A. Instructions for Experiment 1

1. GENERAL RULES

In addition to the basic salary for participating in the experiment, you have the possibility to participate in a market transaction. Part of this market transaction is a lottery. In this lottery, there are 10 main prizes that are worth €25 each.

2. THE MARKET

2.1. Market Transaction

You have now the possibility to trade on a real market. The result of the market depends not only on your decisions but also on the decisions of two other market participants who are currently present in this room with you. Depending on the information provided by your group, either each of you receives a lottery ticket or each of you receives the same amount of money.

Please note that this is not a hypothetical transaction but that all your statements are final and binding!

2.2. The Rules of the Market

In the following, you will be presented with all possible prices between €0.25 and €10.00 for the lottery ticket, and you decide whether you would like to receive the lottery ticket or the money at that price. The other two market participants decide as well whether they would like to receive the lottery ticket or the money at the respective price.

Thereafter, the experimenter draws an amount of money by chance from an urn. This urn contains all possible prices between €0.25 and €10.00 at least once.

The experimenter then checks whether all market participants have stated whether they would prefer to keep the money at the drawn amount. If this is the case, each market participant receives the drawn amount of money.

However, if this is not the case and if one or several market participants have stated that they would prefer the lottery ticket at this amount of money, all market participants receive a lottery ticket.

Please confirm by checking the box that you have understood and accept the rules of the market.

☐

If you have questions on the rules of the market, please ask the experimenter now, so that they can be explained to you again!

3. CONTROL QUESTIONS

For training purposes, you will be presented with short scenarios. Please check what would happen:

Max has determined a selling price of €3.00 for his lottery ticket. Peter and Hans have determined that they would sell their tickets starting at €2.00. The experimenter draws a common price [offer price] of €8.00 from the urn. **What happens?**

◯ The tickets are sold. Max receives €3.00, Peter and Hans each receive €2.00.

◯ The lottery tickets are sold. Everyone receives €8.00.

◯ Everyone keeps their lottery ticket.

Max and Hans have declared that they would sell their lottery tickets starting at €1.00. Peter wants at least €9.00 for his lottery ticket. The experimenter draws a common price [offer price] of €9.00 [€8.00] from the urn.

What happens?

◯ The lot. tickets are sold. Max receives €1.00, Hans €1.00, and Peter €9.00.

◯ Everyone keeps their lottery ticket.

◯ The lottery tickets are sold. Everyone receives €9.00.

Hans has declared that he would sell his lottery ticket starting at €10.00. Max and Peter would sell their lottery tickets at €1.00. The experimenter draws a common price [offer price] of €28.00 [€10.00] from the urn.

What happens?

◯ The lottery tickets are sold. Everyone receives €10.00.

◯ The lot. tickets are sold. Hans receives €10.00, Max and Peter each €1.00.

◯ Everyone keeps his lottery ticket.

Please notify the experimenter once you are finished!

4. DECSION

Do you choose money or lottery ticket?

Please indicate for each of the following amounts whether you would like to receive the money or the lottery ticket. Your statements are binding!

At€ 10.00: ◯ I would choose the money ◯ I would choose the lot. ticket

€ 9.75: ◯ I would choose the money ◯ I would choose the lot. ticket

…

€ 0.25: ◯ I would choose the money ◯ I would choose the lot. ticket

[Post experimental questionnaire with demographic data is omitted.]

B. Instructions for Experiment 2 (Real Lottery Tickets)

1. GENERAL RULES

Eurojackpot is a lottery that is played across Europe. It is organized by the national lotteries of 14 participating European countries (among others, Germany, the Netherlands, Finland, Italy, and Spain). Draws of the Eurojackpot lottery numbers are held in Helsinki every Friday. The winning numbers and quota are published at around 11:15 pm. It is possible to receive the winnings as of Saturday morning.

In the Eurojackpot lottery five of the regular numbers between 1 and 50 and two of the so-called eight Euro numbers will be drawn (see the picture below). The rule of the game is therefore: "5 out of 50 plus 2 out of 8." For each chance of winning, the player fills five regular numbers and two Euro numbers in a line [picture of ticket is omitted see above].

There are 12 different winning classes in the lottery, among which the prize money will be distributed. For a prize in the lowest winning class, three correct numbers (including Euro numbers) are required. For the jackpot, five correct regular numbers and two Euro numbers are required. The table below shows the probability of winning and the expected winnings in the 12 classes (for one line) [table omitted; see above].

First, I would like to know whether you have heard of this lottery before. **Have you ever participated in this lottery?**

◯ YES ◯ NO

You will receive a ticket of the Eurojackpot lottery from the experimenter. Please draw a ticket from the urn that the experimenter is presenting you with! The lottery ticket is in an envelope. Please DO NOT OPEN it yet!

This lottery ticket is yours as of now. You are now the legal owner of this ticket, and you may take it home at the end of this experiment. The lottery tickets have been completed in advance according to a random algorithm. The official draws of the tickets will take place on 14 March 2014, and the results will be published on the same day at 11.15 pm on the Internet (http://www.eurojackpot.de).

You have now the possibility to trade your lottery ticket on a real market. The result of this market depends not only on your decision but also on the decisions of two other market participants who are currently present in this room with you. Together with these two randomly selected persons, you form a group. In addition to your group, there are other groups in the room that operate independently of you.The experimenter will make an offer to purchase all three lottery tickets that have just been provided to the members of your group. Depending on your actions, either all or no lottery tickets will be sold. Please note that this is a real transaction and that all your offers are final and binding!

2.1. Rules of the Market

In the following, you will be presented with all possible prices between €0.25 and €10.00, and you decide whether you would sell the lottery ticket at the respective price or not. The lowest price at which you would sell your lottery ticket determines your selling price.

An example: If you sell your ticket for €8.00 (and each higher amount), but do not sell it for €7.75 (and each lower amount), your selling price is €8.00.

The other two market participants in your group also determine at all possible prices whether they would like to sell their lottery ticket or not, thereby determining their selling price.

Thereafter, the experimenter will make an offer to your group, suggesting the purchase of all three lottery tickets at a common price [uniform offer price]. This common price [offer price] for the lottery tickets will be drawn by chance from an urn that contains all possible prices from €0.75 [€0.25] to €30.00 [€10.00] exactly once each.

The experimenter then checks whether the drawn common price lies above the sum of the demanded selling prices of the group members (or whether it equals this sum) [whether all members of the group would like to sell their lottery ticket at the offer price]. If this is the case, all lottery tickets will be sold, and each market participant receives the selling price demanded by him personally [and each member of the group receives the randomly drawn offer price]. If this is not the case and the sum of the prices lies below the drawn common price [and if one or several members of the group indicated that they would not sell their lottery ticket at the offer price], all members of your group keep their lottery tickets.

Please confirm by checking the box that you have understood and accept the rules of the market. If you have questions on the rules of the market, please ask the experimenter to explain them to you again!

4. DECISION

At which price would you sell your lottery ticket?

Please indicate for each of the following amounts whether you would sell your lottery ticket or not. The lowest price at which you would sell your ticket determines your selling price, which you should also fill in separately at the end of the form. Your statements are binding!

At a price of

€ 10.00: ○ I would sell the ticket ○ I would not sell the ticket
€ 9.75: ○ I would sell the ticket ○ I would not sell the ticket
…
€ 0.25: ○ I would sell the ticket ○ I would not sell the ticket

My selling price is: ________ € Please notify the experimenter now!

5. SCENARIOS

In the following, I ask you to imagine different variations of the market transaction described above. For each scenario, please indicate the selling price for your lottery ticket.

5.1. First Scenario

The transaction is fully independent of other persons. The experimenter randomly draws an offer price for your lottery ticket from an urn which contains all possible prices between €0.25 and €10.00. If the drawn price lies above (or equals) your selling price, you receive the drawn price. Otherwise, you keep your ticket.

5.2. Second Scenario

You decide in the group, and the market complies with the rules described above. However, the common price [offer price] will not be determined randomly, but will be determined by another participant in this study, who acts as buyer. The other participant has received a budget of [3 × €10.00 =] €30.00 from the experimenter and can keep the part of the budget that he does not spend. The buyer makes an offer to the group [makes the same offer to each group member]. If the sum of the three sale prices does not lie above his offer [If none of the three sale prices lie above his offer], the transaction takes place. The buyer receives the three tickets and keeps the part of his budget that remains after deducting the sale prices. [All group members receive the uniform offer price by the buyer.] However, if the transaction does not take place, he keeps his budget of €30.00, and the three group members keep their tickets.

5.3. Third Scenario

The situation equals scenario 2. However, in addition it is known that the buyer has the possibility to resell the three lottery tickets for a common price of €60.00 to the experimenter. If the transaction takes place, the buyer can thus double his initial budget of €30.00. Otherwise, he keeps his budget.

5.4. Forth Scenario

If you were asked to state the value of your ticket independently of any transaction, what would its value be?

[Demographic questions omitted]

Instructions for Chapter 4. Self-Debiasing

I conducted some parts of the study in the laboratory and other parts online. I will report the Laboratory instructions first and report on the online experiment where it differed or added treatments.

I. Laboratory Experiments

[*I report the* Base *condition first. For all other treatments I focus on the sections that deviate from* Base *to make it easy for the reader to identify the differences.*]

A. Base Condition

Dear Participant,

Thank you very much for attending my session! You will find detailed instructions for the experiment below.

1. GENERAL RULES

1.1 Anonymity and Duration

You are about to take part in an economics experiment that is financed by University funds. The experiment will last for approximately 15 minutes. All participants will remain strictly anonymous. Once the experiment is finished, nobody, including the experimenter, will be able to connect your earnings and the choices you made in the experiment with your name. Procedure

If you read the following instructions carefully, you will be able to earn a substantial sum of money, depending on the decisions you make. It is therefore very important that you read these instructions carefully.

There shall be absolutely no communication between participants during the experiment. Disobeying this rule will lead to exclusion from the experiment and will make you ineligible for any payment. If you have any questions, please ask the experimenter. Questions concerning the content of the instructions will only be answered by highlighting particular passages of the written instructions.

1.2 . Payment

Your income is calculated in €. You will be paid in cash after the experiment is finished.

2. SETUP

2.1. Lottery

You can participate in a lottery in this experiment. The lottery consists of two tickets – one “tails,” the other one “heads.” Which of the two tickets is the winner will be determined by a coin toss. If the coin shows the same symbol as the ticket that you have, then you win 8 €. If the ticket instead displays the opposite symbol than your ticket – for example your ticket shows “heads” while the coin shows “tails” or vice versa – then you get 0 € from the lottery. Your sealed ticket was selected in your presence and at random from a box. All the tickets were sealed and thus neither you nor the experimenter knows whether you have a “tails” or a “heads” ticket.

The experimenter tossed the coin at the beginning of the experiment and covered it with a mug that is marked with your booth number. The result of the coin toss will only be revealed after the experiment is completed.

2.2. The Trade

Your ticket can be traded for the alternative ticket in the lottery. If your ticket is traded, you will open the envelope containing your ticket in order to determine which ticket you have and give it to the experimenter. The experimenter will give you the opposite ticket. The trade occurs before the outcome of the coin toss is revealed.

2.3. Consequences of the Trade

If your ticket is traded, you will receive the opposite ticket of the lottery in its place. Thus, if you had a ticket with "heads," then you will receive one with "tails" and vice versa. Additionally, you get an extra 25 €-cent for the trade. If your ticket is not traded then you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of all possible payoffs you might earn in the lottery:

(1) *If your ticket is not traded and …*

(a) the coin toss matches your ticket, then you get 8 € from the lottery.

(b) the coin toss does not match your ticket, then you get 0 €.

(2) *If your ticket is traded and …*

(a) the coin toss matches your initial ticket, then you get 0 € from the lottery and 25 €-cent for the trade.

(b) the coin toss does not match your initial ticket, then you get 8 € from the lottery and 25 €-cent for the trade.

New Sheet: After participants read the general instructions, they were given the following sheet with control questions. Subjects were required to complete the questions correctly in order to proceed to the main experiment.

3. CONTROL QUESTIONS

(1) You have a "tails" ticket. You trade it for the alternative ticket. The coin toss shows "heads." How much do you earn?

(2) You have a "tails" ticket. You keep your ticket. The coin toss shows "heads." How much do you earn?

[Additional control question for Optional and Mandatory Treatments]

(3) If the agent decides in your objective interest, will he trade or keep the ticket?

[Additional control question only for the Optional Treatment]

(4) What does the agent earn if you do not involve him?

4. DECISION

Do you want to trade your ticket? YES or NO?

Please write your answer in the box:

5. POST EXPERIMENT QUESTIONAIRE

5.1. What is the prob. that a “heads” ticket wins in the lottery?

- 50%
- less than 50%
- more than 50%

5.2. What is the prob. that a “tails” ticket wins in the lottery?

- 50%
- less than 50%
- more than 50%

5.3. What is your major?

5.4. What is your sex?

5.5. Have you ever been employed (≥2 months) outside of the University?

B. Treatment MANDATORY AGENT

I report only those parts of the instructions that differ from the Base condition.

4. CHOICE OPTIONS

A personal agent has been assigned to you, who will make the decision about whether to trade the ticket for you. You can veto the decision of the agent and replace it with your own choice.

The agent was selected because in a pilot session he made his trading decision in the objective best interest of the principal. The agent will be paid 2 € if he decides to trade your ticket. If he decides not to trade, then he receives no payment. The agent is paid by the experimenter independently of whether you veto or reject the agent’s decision.

Your agent can decide to retain your initial ticket for you or exchange it for the alternative ticket in the lottery. You will be informed about his decision. You can either accept the decision of the agent, or you can veto it and replace it with your own choice. The agent does not learn whether you vetoed his choice or not.

Here is a summary of your choice options and their corresponding pay-offs:

(1) If your agent decides to trade your ticket, then you can either veto his decision and keep your initial ticket, or you can accept his decision. If you accept his decision, you receive the alternative ticket of the lottery plus 25 €-cent for the trade.

(2) If your agent decides to keep your original ticket, then you can accept his decision and keep your initial ticket. Alternatively, you can veto his decision and trade your ticket yourself. In that case you receive the alternative ticket of the lottery plus 25 €-cent for the trade.

New Sheet: After participants read the main instructions I informed them whether their agent had traded their ticket by providing the participant with the following sheet.

5. DECISIONS

Your agent decided to *trade/not trade* your initial ticket.

Do you want to veto the decision of your agent YES or NO?

Please write your answer in the box below:

New Sheet: If a subject decided to veto the agent's choice, I handed out the following sheet.

You decided to veto the decision of the agent.

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below:

New Sheet: If a subject decided NOT to veto the agent's choice, I handed out the following sheet.

You decided not to veto the decision of the agent.

This means that the agent's decision is binding on you. Following the agent`s decision *your ticket is traded you keep your original ticket.*

C. Treatment OPTIONAL AGENT

I report only those parts of the instructions that differ from the Base condition.

4. CHOICE OPTIONS

You can decide whether you want to exchange or keep your ticket yourself or you can delegate the decision of whether to trade the ticket to a personal agent, who will make the decision for you. If you decide to delegate to an agent their decision is not binding on you. You can veto the decision of the agent and replace it with your own choice.

The agent was selected because in a pilot session, he made his trading decision in the objective best interest of the principal. The agent is paid 2 € if he decides to trade your ticket. If he decides not to trade, then he receives no payment. The agent is paid by the experimenter independently of whether you veto or reject the agent's decision. Even if you do not want to involve the agent he is paid the same 2 € by the experimenter.

Your agent can either choose to retain your initial ticket or exchange it for the alternative ticket in the lottery. You will be informed about his decision. You can either accept the decision of the agent, or you can veto it and replace it with your own choice. The agent will not learn whether you vetoed his choice or not.

Here is a summary of your choice options and their corresponding payoffs:

(1) If you delegate to your agent then you can either a) veto or b) accept his decision.

(a) If you veto the agent's choice when he decided to ….
(b) trade, then you keep your initial ticket.
(c) keep your initial ticket, then you trade and receive the alternative ticket of the lottery plus the extra 25 €-cent.

(2) If you accept the agent's choice when he decided to …
(a) trade your ticket, then you trade and receive the alternative ticket of the lottery plus the extra 25 €-cent
(b) keep your ticket, then you keep your initial ticket.

(3) If you do not delegate to your agent, then you can decide to a) trade or b) keep your original ticket by yourself.

(a) If you trade then you receive the alternative ticket of the lottery plus the extra 25 €-cent.
(b) If you do not trade then you keep your original ticket.

5. DECISION

You received a ticket at the beginning of the experiment. Do you want to delegate the decision whether to keep or trade your ticket to the agent? YES or NO?

Please write your answer in the box below:

New Sheet: Handed only to subjects who decided to delegate.

Your agent decided to trade your initial ticket.

Do you want to veto the decision of your agent? YES or NO?

Please write your answer in the box below:

New Sheet: Handed to any subject who decided to veto their agent's decision after delegating to them.

You decided to veto the decision of the agent.

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below:

Alternative Sheet: Handed only to subjects who decided not to delegate.

You decided not to delegate the decision to your agent.

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below:

D. Treatment INFORMATION ONLY

I report only those parts of the instructions that differ from the Base condition.

4. CHOICE OPTIONS

Before you decide whether to trade your ticket, you can observe the decision of a personal agent who is assigned to another participant, who I call the principal. The agent makes the decision in place of the principal. However, the principal can veto the decision of the agent and replace it with his own choice.

The agent was selected because in a pilot session, he made the trading decision in the objective best interest of the principal. The agent is paid 2 € if he decides to trade the ticket of the principal. If he decides not to trade, then he receives no payment. The agent is paid by the experimenter.

The agent can keep the initial ticket for the principal or exchange it for the alternative ticket in the lottery. The principal can either accept the decision of the agent, or he can veto it and replace it with his own choice.

New Sheet: Handed only to subjects after they read the main instructions.

5. DECISION

The agent traded the original ticket of the other principal.

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below:

II. ONLINE TREATMENTS

The experimental protocol of the online experiment differed from the laboratory experiment because participants could not receive a sealed envelope with their ticket and I could not throw a coin in their presence. The protocol I used online was identical for all treatments. To present this protocol, I report the instructions of the *Base* condition. I then report the instructions for all treatments (*Default, Guided Agent, Voting, Optional Agent* and *Optional Voting*) I did not conduct in the laboratory. I focus on those parts of the instructions that differ from the *Base* condition. W do not report the treatment instructions for online Mandatory as they were the same I used in the lab. The instructions for No-Agent were identical with the online *Base* condition.

A. BASE CONDITION

Dear Participant.

Thank you very much for attending my session! All necessary instructions for the experiment will be presented to you on the screen. Note that you cannot use your login key more than once. If you log out before you have fully completed the experiment you will not be able to finish and receive payment.

1. GENERAL RULES

1.1. Anonymity and Duration

You are about to take part in an economics experiment that is financed by University funds. The experiment will last for approximately 10 minutes. All data will be anonymous. Once the experiment has concluded and payment is completed, the Lime-Survey software will automatically delete any connection between your name and both the choices you made and your earnings in this experiment.

1.2. Procedure

If you read the following instructions carefully, you will be able to earn a substantial sum of money, depending on the decisions you make. It is therefore very important that you read these instructions carefully.

1.3. Payment

Your income is calculated in €. You will be paid immediately after completing the experiment via electronic bank transfer or PayPal – depending upon your preference.

[New Screen]

2. SETUP

2.1. Lottery

You can participate in a lottery in this experiment. The lottery consists of two tickets – one “tails,” the other one “heads.” The winning ticket will receive 4 € and the losing ticket will receive 0 € from the lottery. You were given the information whether you have a “tails” or a “heads” ticket already, but I have used a code to hide this information from you. I will reveal the code after you have completed the whole experiment. This procedure assures you that you were assigned a particular ticket, either “heads” or “tails,” from the beginning of the experiment. Which ticket you were assigned was selected randomly using the invitation list containing all participants. If you were listed with an even subject number, then you received a “tails” ticket; if you were listed with an uneven number then you received a “heads” ticket. At the end of the experiment, you will be asked to indicate whether the “heads” or the “tails” ticket shall be the winner of the lottery. This ensures that the outcome of the lottery is indeed determined by chance only. Since you do not know which of the two tickets you have until the experiment is over, you cannot influence the outcome of the lottery by making this choice.

[New Screen]

2.2. Task

You will be given the opportunity to trade your ticket for the alternative ticket in the lottery. If you decide to trade the ticket, you receive a bonus of 25 €-cent. If you decide to trade, you will be given the alternative ticket of the lottery in exchange for your ticket. If you decide against the trade, you hold on to your original ticket.

2.3. Consequences of the Trade

If you trade the ticket then you will receive the alternative ticket of the lottery. Thus, if you had a "heads" ticket then you will receive a "tails" ticket and vice versa. Additionally, you get an extra 25 €-cent for the trade. If you decide against the trade then you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of your choice options and their corresponding pay-offs:

(1) *If you keep your ticket and ...*

(a) you were assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If you trade your ticket and ...*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b) in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

New Screen: After participants read the general instructions for the experiment, they were given the following control questions which they had to complete correctly in order to proceed to the next stage.

3. CONTROL QUESTIONS

3.1. You were initially assigned a "tails" ticket. When you traded your ticket you received a "heads" ticket in exchange. You determined that "tails" should win the lottery. How much do you earn?

3.2. You were initially assigned a "tails" ticket and decided to keep your ticket. You determined that "tails" should win the lottery. How much do you earn?

[New Screen]

4. DECISION

Do you want to trade the ticket for the alternative ticket? YES or NO?

○ ***YES*** ○ **NO**

[New Screen]

5. RESPONSIBILITY & REGRET

5.1. Responsibility [Trade]

Please assume that you decided to trade your ticket. You received the alternative ticket of the lottery in exchange for your original ticket. Please assume that your original ticket, which you traded, won the lottery, while the new ticket you received in exchange, lost.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark the boxes you see below. Low values express little responsibility; high values represent a strong feeling of responsibility.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.2. Regret [Trade]

Please imagine the same situation as before: You decided to trade your ticket. Please assume that your original ticket, which you traded, won the lottery, while the new ticket you received in exchange lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below Low values express little regret; high values represent a strong feeling of regret.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.3. Responsibility [Keep]

Please assume that you decided to keep your original ticket and that this ticket lost the lottery, while the alternative ticket of the lottery, which you did not receive, won.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.4. Regret [Keep]

Please imagine the same situation as before: You decided to keep your ticket. Please assume that your original ticket lost the lottery, while the alternative ticket of the lottery, which you did not receive, won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below.

Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

6. POST-EXPERIMENT QUESTIONAIRE

6.1. What is the probability that a “heads” ticket wins in the lottery?

(1) 50%

(2) less than 50%

(3) more than 50%

6.2. What is the probability that a “tails” ticket wins in the lottery?

(1) 50%

(2) less than 50%

(3) more than 50%

6.3. What is your major?

6.4. What is your sex?

B. DEFAULT Treatment

I report only those parts of the instructions that differ from the Base condition.

2. SETUP

2.1. Task

You will be given the opportunity to trade your ticket for the alternative ticket in the lottery. If you decide to trade the ticket you receive a bonus of 25 €-cent. Your ticket will be automatically traded for the opposite ticket in the lottery unless you intervene. Thus, if you want to keep your ticket you need to veto the automatic trade of your ticket. If you decide not to veto, you will be given the alternative ticket of the lottery in exchange for your ticket.

2.2. Consequences of the Trade

If your ticket is traded then you will receive the alternative ticket of the lottery. Thus, if you had a "heads" ticket then you will receive a "tails" ticket and vice versa. Additionally you will receive an extra 25 €-cent for the trade. If your ticket is not traded then you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of your choice options and their corresponding payoffs:

(1) *If you veto the trade of your ticket and …*

(a) you were assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If your ticket is traded and …*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b) in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

New Screen: After participants read the general instructions for the experiment, they were given the following control questions which they had to complete correctly in order to proceed to the next stage.

3. CONTROL QUESTIONS

3.1. You were initially assigned a "tails" ticket. When you accepted the trade you received a "heads" ticket in exchange. You determined that "tails" should win the lottery. How much do you earn?

3.2. You were initially assigned a "tails" ticket. You vetoed the trade and kept your ticket. You determined that "tails" should win. How much do you earn?

[New Screen]

4. DECISION

Your ticket can be traded for the opposite ticket in the lottery. The ticket is traded automatically unless you decide to veto the trade. Thus, if you want to keep your ticket you need to veto the automatic trade. If you decide not to veto, you will be given the alternative ticket of the lottery in exchange for your original ticket.

Do you want to veto the trade? YES or NO?

○ **YES** ○ **NO**

[New Screen]

5. RESPONSIBILTIY & REGRET

5.1. Responsibility [Trade]

Please assume that you did not veto the trade, so your ticket is traded and you receive the alternative ticket of the lottery. Your original ticket, which you traded, won the lottery, while the new ticket you received in exchange, lost.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Trade]

Please assume the same situation as before: You decided not to veto the trade. Your original ticket, which you traded, won the lottery, while the new ticket you received in exchange, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.3. Responsibility [Keep]

Please assume that you vetoed the trade, so you keep your initial lottery ticket. Your original ticket, which you kept, lost the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.4. Regret [Keep]

Please assume the same situation as before: You decided to veto the trade. Your original ticket, which you kept, lost the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

C. 2 Treatment GUIDED AGENT

I report only those parts of the instructions that differ from the Base condition.

2. SETUP

2.1. Task

A personal agent has been assigned to you, who will make the decision over whether to trade the ticket for you. You cannot veto the decision of the agent.

However, you can incentivize your agent's choice. You can determine either that the agent earns 2 € for trading the ticket and nothing for keeping it for you, or that the agent earns 2 € for keeping the ticket for you and nothing for trading it. The agent is paid by the experimenter. Even though incentivized the agent retains a free choice. You will be informed about his trading decision.

[New Screen]

2.2. Consequences of the Trade

If your agent decides to trade your ticket, then you receive the alternative ticket of the lottery. Thus, if you had a "heads" ticket then you receive a "tails" ticket in exchange and vice versa. In addition you get an extra 25 €-cent for the trade. If the agent decides that you keep your ticket, then your ticket is not traded and you keep your initial ticket, but you do not receive the extra 25 €-cent.

Here is a summary of your choice options and their corresponding pay-offs:

(1) *If your ticket is not traded and ...*

(a) you were originally assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were originally assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If your ticket is traded and ...*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b**)** in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

New Screen: After participants read the general instructions for the experiment, they were given the following control questions which they had to complete correctly in order to proceed to the next stage.

3.CONTROL QUESTIONS

3.1. You were initially assigned a "tails" ticket. The agent decided to trade your ticket so you receive a "heads" ticket in exchange. You determined that "tails" should win. How much do you earn?

3.2. You were initially assigned a "tails" ticket. The agent decided not to trade your ticket. You determined that "tails" should win. How much do you earn?

[New Screen]

4.DECISION

Your agent will either be paid 2 € for trading or for keeping the ticket for you.

Do you want to incentivize your agent to trade your ticket? YES or NO?

YES **NO**

[New Screen]

5.RESPONSIBILITY & REGRET

5.1. Responsibility [Trade]

Please assume that you incentivized your agent to trade. The agent decided to trade your ticket and earned the 2 € bonus. You received the alternative ticket of the lottery. Please assume that your original ticket won the lottery, while the new ticket you received in exchange, lost.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.2. Regret [Trade]

Please assume the same situation as before: you incentivized your agent to trade. The agent traded your ticket and earned the 2 € bonus. Please assume that your original ticket won the lottery, while the new ticket you received in exchange, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.3. Responsibility [Keep]

Please assume that you incentivized your agent to keep your ticket. The agent decided to keep your ticket and earned the 2 € bonus. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if the agent had traded, won the lottery.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.4. Regret [Keep]

Please imagine the same situation as before: you incentivized your agent to keep your ticket. The agent decided to keep your ticket. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

D. 2 Treatment VOTING WITH VETO

I report only those parts of the instructions that differ from the *Base* condition.

2. SETUP

2.1. Task

You are offered the opportunity to trade your ticket for the alternative ticket in the lottery. If you trade the ticket you will receive a bonus of 25 €-cent for the transaction.

However, you do not make the trading decision alone. All participants in your session will decide together by majority vote whether the group will trade or keep the tickets. The majority decision applies to all tickets including your own.

At least 80 subjects will submit their vote. Access to the session will be closed once 80 subjects have participated. Only the subjects who at that point have already started the experiment will be allowed to finish it.

You can veto the majority vote. If the majority decides to keep the tickets and you veto the vote, then you can trade your ticket and will be given the alternative ticket of the lottery in exchange for your own ticket. If you decide not to veto, the majority decision applies to your ticket and you keep your original ticket. By contrast, if the majority decides to trade and you veto the decision, then you will keep your own ticket. If you decide not to veto, the majority vote applies to your ticket and you trade it for the second ticket in the lottery.

Note that your veto has no consequences on whether any other participant's ticket will be traded or not – it affects only your own ticket. If the majority votes to keep the tickets and you veto, then you will trade your own ticket, while the others may keep their ticket. If the majority votes to trade the tickets and you veto, then you will keep your own ticket, while the others may trade. The veto only affects you.

2.2. Consequences of the Trade

If the majority votes to trade all tickets, then you receive the alternative ticket of the lottery unless you veto their decision. Thus, if you had a ticket with "heads" then you receive one with "tails" and vice versa. Additionally, you get an extra 25 €-cent for the trade. If you veto the majority`s decision you keep your initial ticket without getting the bonus for trading. If the majority decides that all participants should keep their ticket, then your ticket will not be traded. If you veto the majority's decision not to trade, then you receive the alternative ticket of the lottery and receive the extra 25 €-cent.

Here is a summary of your choice options and their corresponding payoffs:

(1) *If your ticket is not traded and …*

(a) you were assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If your ticket is traded and …*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b) in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

[New Screen]

After participants read the general instructions for the experiment, they were given the following control questions which they had to complete correctly in order to proceed to the next stage.

3. CONTROL QUESTIONS

3.1. You have a "tails" ticket. The majority voted for the trade, and you did not veto the trade, so you received a "heads" ticket in exchange. You determined that "tails" should win. How much do you earn? How much would you earn if you vetoed the majority vote?

3.2. You have a "tails" ticket. The majority voted not to trade, and you did not veto the decision, so you kept your ticket. You determined that "tails" should win. How much do you earn? How much would you earn if you vetoed the majority vote?

[New Screen]

4. DECISIONS

4.1. Voting Decision

Do you want to vote for trading the tickets? YES or NO?

- **YES**
- **NO**

[New Screen]

4.2. Veto Decision

You will only learn the decision of the majority once the whole session is over. Therefore, I will ask you to make a decision for each of the two possible outcomes: either the majority votes for trading the tickets or the majority decides that all participants should keep their tickets.

First, assume that the majority voted to trade all tickets. In that case your ticket will be traded. You can now veto this majority vote. Recall that your veto has consequences only for your own ticket.

Do you want to veto a majority vote to trade all tickets? YES or NO?

- **YES**
- **NO**

Second, assume that the majority voted to keep all tickets. In that case you will keep your ticket. You can now veto this majority vote. Recall that your veto has consequences only for your own ticket.

Do you want to veto a majority vote to keep all tickets? YES or NO?

- **YES**
- **NO**

[New Screen]

5. RESPONSIBILITY & REGRET

5.1. Responsibility [Trade]

Please assume that you voted for the trade. The majority vote also decided for the trade and you did not veto this decision. Your ticket was traded and you received the alternative ticket of the lottery. Your original ticket which you traded won the lottery, while the new ticket you received in exchange has lost.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Trade]

Please assume the same situation as before: Both you and the majority voted for the trade and you did not veto this decision. Your ticket was traded and you received the alternative ticket of the lottery. Your original ticket which you traded won the lottery, while the new ticket you received in exchange has lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.3. Responsibility [Keep]

Please assume that you voted to keep the tickets, while the majority voted to trade the tickets. You vetoed the majority's decision and kept your original ticket. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.4. Regret [Keep]

Please assume the same situation as before: You voted to keep the tickets, but the majority voted to trade the tickets. You vetoed the majority decision and kept your ticket. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

E. 2 Treatment VOTING WITHOUT VETO

3. SETUP

2.1. Decision

You are offered the opportunity to trade your ticket for the alternative ticket in the lottery. If you trade the ticket you receive a bonus of 25 €-cent.

However, you do not make the trading decision alone. All participants in your session will decide together by majority vote whether you trade or keep your tickets. You can submit your own vote for or against trading. The majority decision applies to all tickets including your own.

At least 80 subjects will submit their vote. Access to the session will be closed once 80 subjects have participated. Only the subjects who at that point have already started the experiment will be allowed to finish it.

Note that you cannot veto the decision of the majority. Whatever the majority vote decides will apply to your ticket, as well as to the tickets of all other participants in your session.

2.2. Consequences of the Trade

If the majority votes to trade tickets, then you receive the alternative ticket of the lottery. Thus, if you had a ticket with "heads," then you receive one with "tails" and vice versa. Additionally, you will receive an extra 25 €-cent for the trade. If the majority decides that all participants should keep their ticket, then your ticket is not traded and you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of the possible consequences of the majority vote and their corresponding payoffs:

(1) *If your ticket is not traded and ...*

(a) you were initially assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were initially assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If your ticket is traded and ...*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b) in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

New Screen: After participants read the general instructions for the experiment, they were given the following control questions which they had to complete correctly in order to proceed to the next stage.

3. CONTROL QUESTIONS

3.1. You have a "tails" ticket. The majority voted to trade the tickets and you received a "heads" ticket in exchange. You determined that "tails" should win. How much do you earn?

3.2. You have a "tails" ticket. The majority voted to keep the tickets and you kept your ticket. You determined that "tails" should win. How much do you earn?

[New Screen]

4. DECSISION

Do you want to vote for the trade? YES or NO?

YES NO

F. 2 Treatment OPTIONAL AGENT

The online version of the *Optional Agent* treatment implemented a within-subject design that consisted of two conditions subjects had to complete sequentially: the *Optional* treatment, and the *Base* treatment. The instructions for the *Base* condition (which I call *No-Agent* to distinguish it from the stand-alone *Base* condition) were identical with the stand-alone *Base* online treatment and thus are not repeated here. The instructions for *Optional* online did not differ from the instructions I gave subjects in the stand-alone treatment I conducted in the laboratory except for two changes: First, I informed participants that they had to pay 5 €-cent to use their agent. Second, I asked participants to indicate their maximum willingness to pay for involving the agent. In the general part of the instructions I informed subjects that the experimenter would implement either the decisions they had made in part 1 of the session (*Optional*) or the one they had made in part 2 (*Base*). Here I present only the parts of the instructions that differ from the stand-alone *Optional* and *Base* treatments.

1. GENERAL RULES

1.4. Random Selection of Condition that is Paid for

You will be presented with two different experiments. Both of them will give you the opportunity to trade your ticket for the alternative ticket in the lottery. You will complete the two experiments sequentially. After this session is over, the experimenter will determine randomly which of the two scenarios will be implemented. You will be paid according to the decisions you made in the experiment that is randomly selected.

4. DECISION

4.1. Your Decision: Payment for Delegation to Agent

You can decide whether you want to exchange or keep your ticket yourself or you can delegate the decision of whether to trade to a personal agent, who will make the decision for you. If you decide to involve the agent, his decision is not binding on you. You can veto his decision and replace it with your own choice. If you make use of the option to involve an agent, 5 €-cent will be subtracted from your earnings.

5. POST EXPERIMENTAL QUESTIONAIRE

5.1. Hypothetical Willingness to Pay for Delegation

You had the opportunity to delegate your trading decision to an agent. If you used this option, 5 €-cent were subtracted from your earnings. How much would you be willing to pay for the opportunity to involve the agent in your trading decision? You can indicate any amount from 0 up to 400 €-cent.

Please type your answer in the box below.

6. RESPNSIBILTIY & REGRET

6.1. Responsibility [Trade]

Please assume that you delegated your choice to the agent. The agent traded your ticket. Since you did not veto his decision you received the alternative ticket of the lottery. Your original ticket which you traded won the lottery, while the new ticket you received in exchange, lost.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

6.2. Regret [Trade]

Please assume the same situation as before: you delegated your choice to the agent. The agent traded your ticket. Since you did not veto his decision you received the alternative ticket of the lottery. Your original ticket which you traded won the lottery, while the new ticket you received in exchange has lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

6.3. Responsibility [Keep]

Please assume that you delegated your choice to the agent. The agent traded your ticket. Since you vetoed his decision you kept your original lottery ticket. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How responsible do you feel for not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little responsibility; high values represent a strong feeling of responsibility.

1	2	3	4	5	6	7	8	9	10

[New Screen]

6.4. Regret [Keep]

Please assume the same situation as before: you delegated your choice to the agent. The agent traded your ticket. Since you vetoed his decision you kept your original lottery ticket. Your original ticket, which you kept, lost in the lottery, while the alternative ticket, which you would have received in exchange for your ticket if you had traded, won the lottery.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below. Low values indicate little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

G. 2 Treatment OPTIONAL VOTING

Optional Voting was a hypothetical treatment, so the subjects' choices did not influence their payoffs. After subjects completed the *Base* condition, they were presented with a scenario that asked them to assume that they could either make the trading decision by themselves or delegate it to a majority vote, which they then could accept or veto.

2. SETUP

Please assume the same situation as in the experiment you just completed: You are offered the opportunity to trade your ticket for the alternative ticket in the lottery. You receive a bonus of 25 €-cent for trading. You can make this decision yourself or you can delegate your decision to a majority vote of all participants in your session. Assume that in order to delegate your choice you would have to pay 5 €-cent from your earnings. Assume that the session is closed once at least 80 subjects completed the experiment. If you decide to delegate your decision, the majority vote will apply to your ticket. In other words, if the majority decides to trade, your ticket will be traded; if the majority opts to keep the tickets, you will keep your ticket as well.

However, please assume that the majority vote to which you can delegate your decision leaves you with a veto. If the majority votes to keep the tickets and you veto their vote, then you can trade your ticket by yourself and you will be given the alternative ticket of the lottery in exchange for your own ticket. If you decide not to veto their vote, the majority decision will apply to your ticket and you keep your original ticket. By contrast, if the majority votes to trade and you veto their decision, then you will keep your own ticket. If you decide not to veto, the majority vote applies to your ticket and you trade it for the alternative ticket in the lottery.

Please assume that your veto has no consequences on whether any other participant trades or not – it affects only your own ticket. If the majority votes to keep the tickets and you veto that decision, then you would trade your own ticket, while the others may keep their lottery ticket. If the majority votes to trade the tickets and you veto, then you would keep your own ticket, while the others still may trade. Thus, in any case your veto will only affect you.

3. DECISIONS

3.1. Delegation to Majority Vote

Do you want to delegate your trading decision to a majority vote? YES or NO?

- **YES**
- **NO**

3.2. Veto Decision

(1) You decided to delegate your trading choice to the majority vote. Please assume that the majority vote decided to trade all tickets. Thus your ticket will be traded. You can now veto this majority vote. Recall that your veto would have consequences only for your own ticket.

Do you want to veto a majority vote to trade all tickets? YES or NO?

- **YES**
- **NO**

(2) You decided not to delegate your trading choice to the majority vote. Therefore assume now that you have to make the decision on your own.

Do you want to trade your ticket? YES or NO?

- **YES**
- **NO**

IV. Instructions for Chapter 5 - Strategic Bias-Insulation

A. Laboratory Experiments

I report the entire instructions of the Base condition first. For all other treatments I focus on the sections that deviate from Base to make it easy for the reader to identify the differences.

A. 1 Base Condition

Dear Participant!

Thank you very much for attending my session! You will find detailed instructions for the experiment below.

1. GENERAL RULES

1.1. Anonymity and Duration

You are about to take part in an economics experiment that is financed by University funds. The experiment will last for approximately 15 minutes. All participants will remain strictly anonymous. Once the experiment is finished, nobody, including the experimenter, will be able to directly connect your earnings and the choices you made in the experiment with your name.

1.2. Procedure

If you read the following instructions carefully, you will be able to earn a substantial sum of money. Your earnings depend on the decisions you make. It is therefore very important that you read these instructions carefully.

There shall be absolutely no communication between participants during the experiment. Disobeying this rule will lead to exclusion from the experiment and will make you ineligible for any payment. If you have any questions, please ask the experimenter. Questions concerning the content of the instructions will only be answered by highlighting particular passages of the written instructions.

1.3. Payment

Your income is calculated in €. You will be paid in cash after the experiment is finished.

2. SETUP

2.1. Lottery

You can participate in a lottery in this experiment. The lottery consists of two tickets – one "tails," the other one "heads." Which of the two tickets is the will be determined by a coin toss. If the coin shows the same symbol as the ticket that you have, then you win 8 €. If the ticket instead displays the opposite symbol than your ticket – for example your ticket shows "heads" while the coin shows "tails" or vice versa – then you get 0 € from the lottery. At the beginning of the session your sealed ticket was selected at random from a box in your presence. All the tickets were sealed and thus neither you nor the experimenter knows whether you have a "tails" or a "heads" ticket.

The experimenter tossed the coin at the beginning of the experiment and covered it with a mug that is marked with your booth number. The result of the coin toss will only be revealed after the experiment is completed.

2.2. The Trade

Your ticket can be traded for the alternative ticket in the lottery. If your ticket is traded, you will open the envelope containing your ticket in order to determine which ticket you have and give it to the experimenter. The experimenter will give you the opposite ticket. The trade is completed before the outcome of the coin toss is revealed.

2.3. Consequences of the Trade

If your ticket is traded, you will receive the opposite ticket of the lottery in its place. Thus, if you had a ticket with "heads," then you will receive one with "tails" and vice versa. Additionally, you get an extra 25 €-cent for the trade. If your ticket is not traded then you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of all possible payoffs:

(1) *If your ticket is not traded and …*

(a) the coin toss matches your ticket, then you get 8 € from the lottery.

(b) the coin toss does not match your ticket, then you get 0 € from the lottery.

(2) *If your ticket is traded and …*

(a) the coin toss matches your initial ticket, then you get 0 € from the lottery and 25 €-cent for the trade.

(b) the coin toss does not match your initial ticket, then you get 8 € from the lottery and 25 €-cent for the trade.

3. CONTROL QUESTIONS

New Sheet: After participants read the general instructions for the experiment, I handed them the following sheet with control questions. Subjects were required to complete the questions before the part labeled "The Experiment."

3.1. Please assume you have a "tails" ticket. You trade it for the alternative ticket. The coin toss shows "heads." How much do you earn?

3.2. You have a "tails" ticket. You keep your ticket. The coin toss shows "heads." How much do you earn?

[New Sheet]

4. DECISION

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below:

[New Sheet]

5. POST EXPERIMENT QUESTIONNAIRE

5.1. What is the probability that a "heads" tickets wins the lottery?

(a) 50%

(b) less than 50%

(c) more than 50%

5.2. What is the probability that a "tails" ticket wins the lottery?

(a) 50%

(b) less than 50%

(c) more than 50%

5.3. What is your major?

5.4. What is your sex?

5.5. Have you been employed for more than two month?

B. 2 HERDING Treatment

I report only those sections of the instructions that differ from *Base* condition.

4. DECISION

In October I conducted a lottery study in this laboratory. The participants were recruited in this building using the same advertisement as I used this time. I had 45 participants of which 31 decided to trade their ticket for the alternative one in the lottery. 14 participants decided to keep their ticket.

Do you want to trade your ticket? YES or NO?

Please write your answer in the box below!

[New Sheet]

5. POST-EXPERIMENT QUESTIONNAIRE

In the Herding treatment I used the same Post-Experiment Questionnaire as in the Base condition except that I added the following question.

5.1. You were informed about the decisions participants had made in an earlier treatment. What can you learn from their decisions about whether your ticket or the ticket you can exchange your ticket for will win the lottery?

B. Online-Experiments

The experimental protocol of the online experiment differed from the laboratory experiment because online participants could not receive a sealed envelope with their ticket; as I could not throw a coin in their presence, I had to use a different method to randomly determine the outcome of the lottery. The protocol I used online was identical for all treatments. I present the online procedure the same way as the laboratory instructions above: I report the complete instructions of the *Base* condition first. For all other conditions I present only those parts of the instructions that differ from the *Base* condition.

A. 1 Base Condition

Dear Participant!

Thank you very much for attending my session! All necessary instructions for the experiment will be presented to you on the screen. Note that you cannot use your login key I sent you by E-mail more than once. If you logout before you have fully completed the experiment you will not be able to finish the experiment and receive a payment.

1. GENERAL RULES

1.1. Anonymity and Duration

You are about to take part in an economics experiment that is financed by University funds. The experiment will last for approximately *[depended on the treatment subjects participated in]* minutes. Once the experiment has concluded and payment is completed, the Lime-Survey software will automatically delete any connection between your name and the choices you made in this experiment.

1.2. Procedure

If you read the following instructions carefully, you will be able to earn a substantial sum of money. Your payment will depend on the decisions you make. It is therefore very important that you read these instructions carefully.

1.3. Payment

Your income is calculated in €. You will be paid immediately after completing the experiment via electronic bank transfer or PayPal – depending upon your own preference.

[New Screen]

2. SETUP

2.1. Lottery

You can participate in a lottery in this experiment. The lottery consists of two tickets – one “tails,” the other one “heads.” The winning ticket will receive 4 € and the losing ticket will receive 0 € from the lottery. You were given the information whether you have a “tails” or a “heads” ticket already, but we have used a code to hide this information from you. We will reveal the code after you have completed the whole experiment. This procedure assures you that you were assigned a particular ticket, either “heads” or “tails,” from the beginning of the experiment. Which ticket you were assigned was selected randomly using the invitation list containing all participants. If you were listed with an even subject number, then you received a “tails” ticket; if you were listed with an uneven number then you received a “heads” ticket. At the end of the experiment, you will be asked to indicate whether the “heads” or the “tails” ticket shall be the winner of the lottery. Since you do not know which of the two tickets you have until the experiment is over, you cannot influence the outcome of the lottery by making this choice. The process ensures that the outcome of the lottery is indeed determined by chance only.

[New Screen]

2.2. The Trade

You will be given the opportunity to trade your ticket for the alternative ticket in the lottery. If you decide to trade the ticket you receive a bonus of 25 €-cent. If you decide to trade, you will be given the alternative ticket of the lottery in exchange for your ticket; if you decide against the trade you keep your original ticket.

2.3. Consequences of the Trade

If you trade the ticket then you will receive the alternative ticket of the lottery. Thus, if you had a “heads” ticket you will be given a “tails” ticket and vice versa. Additionally, you get an extra 25 €-cent for the trade. If you decide against the trade then you keep your initial ticket, but do not receive the extra 25 €-cent.

Here is a summary of your choice options and the corresponding payoffs:

(1) *If you keep your ticket and …*

(a) you were assigned the ticket you determined should win, then you get 4 € from the lottery.

(b) you were assigned the ticket you determined should lose, then you get 0 € from the lottery.

(2) *If you trade your ticket and …*

(a) in exchange you received the ticket you determined should win, then you get 4 € from the lottery and 25 €-cent for the trade.

(b) in exchange you received the ticket you determined should lose, then you get 0 € from the lottery and 25 €-cent for the trade.

New Screen: After participants read the general instructions of the experiment, they were given the following control questions which they had to answer correctly in order to proceed to the next experimental stage.

3. CONTROL QUESTIONS

3.1. Please assume you were initially assigned a “tails” ticket. When you traded your ticket you received a “heads” ticket in exchange. You determined that “heads” should win the lottery. How much do you earn?

3.2. You were initially assigned a “tails” ticket and decided to keep your ticket. You determined that “heads” should win. How much do you earn?

New Screen: After participants completed the control questions, they were asked to make their trading decision in the next stage.

4. DECISION

Do you want to trade the ticket for the alternative ticket of the lottery? YES or NO?

YES ○ ○ **NO**

[New Screen]

5. REGRET

New Screen: Below I report the instructions for measuring regret. Please note that whenever I obtained data on regret, I also elicited the responsibility subjects felt for winning or losing the lottery. The wording of the instructions and the Likert scale was the same for responsibility as for regret. As I do not need to use the responsibility data in the current paper, I leave out this section of the instructions.

5.1. Regret [Trade]

Please imagine that you decided to trade your ticket. Assume that your old ticket which you traded won the lottery, while the new ticket that you received in exchange lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Keep]

Please imagine that you decided to keep your ticket. Assume that your original ticket that you kept lost the lottery, while the alternative ticket of the lottery - which you did not receive - won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

6. POST-EXPERIMENT QUESTIONNAIRE

In Base Online I asked the same Post-Experiment Questionnaire as in Base treatment that was conducted in the laboratory.

B. 2 HERDING Treatment

In the following, I will report only those parts of the instructions that differ from the Base condition.

4. DECISION

In an earlier study subjects were assigned a ticket for a lottery with a 50% chance of winning 4 € and could trade their ticket for the identical alternative ticket in the lottery and a bonus for trading of 25 €-cent. Subjects decided together by vote whether they wanted to trade; the majority of the votes determined whether all participants traded or kept their tickets. The outcome of this lottery is unrelated to the lottery you participate in. A majority of 89% of the participants decided to trade the tickets; 11% voted for keeping the tickets.

Do you want to trade your ticket for the alternative ticket of the lottery and 25 €-cent? YES or NO?

- **YES**
- **NO**

[New Screen]

5. REGRET

5.1. Regret [Trade]

Please imagine that you decided to trade your ticket in line with 89% of the participants of the earlier session who voted to exchange their ticket as you were informed. Assume that your old ticket - which you traded - won the lottery, while the new ticket that you received in exchange, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Keep]

Please imagine now that you decided to keep your ticket, while 89% of the participants in the earlier session decided to vote for the trade. Assume that your original ticket that you kept, lost the lottery, while the second ticket of the lottery - which you did not receive - won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

6. POST-EXPERIMENT QUESTIONNAIRE

In Herding Online I asked the same Post-Experiment Questionnaire as in the Herding treatment that was conducted in the laboratory. For the instructions, please refer to the above.

C. 2 Treatment INDUCED REFERENCE POINT

I have two treatments that manipulate the subjects' focus. One treatment is designed to shift focus from keep to trade in *Base* and the second is designed to shift focus from trade to keep in *Herding*. This manipulation of focus was the only difference to the *Base* and the *Herding* treatments. I therefore present here only those parts of the instructions that deviate from *Base* and *Herding*; for all other sections please refer to the instructions of *Herding* and *Base*

D. 2 Treatment Herding with Focus on Keep

I added the manipulation below directly after the control questions and before subjects made their trading choices. The rest of the instructions do not differ from Herding.

4.1. Reasons for Trading

Please indicate all reasons that speak from your perspective for KEEPING your lottery ticket. Please separate the different reasons from one another by semi-colon, when writing them in the box below.

[New Screen]

4.2. Reasons for Keeping

Please indicate now all reasons that speak from your perspective for TRADING your ticket for the second ticket in the lottery. Please separate the different reasons from one another by semicolon, when writing them in the box below.

E. 2 Treatment BASE WITH FOCUS ON TRADE

I added the manipulation below directly after the control questions and before subjects made their trading choices. The rest of the instructions do not differ from *Base*.

4.1. Reasons for Trading

Please indicate now all reasons that speak from your perspective for TRADING your lottery ticket for the second ticket in the lottery. Please separate the different reasons from one another by semicolon.

[New Screen]

4.2. Reasons for Keeping

Please indicate all reasons that speak from your perspective for KEEPING your lottery ticket. Please separate the different reasons from one another by semi-colon.

F. 2 SEEK INFORMATION Treatment

I report only those parts of the instructions that differ from the Base condition. I do not report the Post-Experiment Questionnaire which is the same as in *Herding*.

4. DECISION

4.1. Optional Information

You can trade your ticket for the alternative ticket of the lottery plus 25 €-cent. In an earlier study subjects were assigned a ticket to a lottery with a 50% chance of winning 4 €, just as you were. They could trade their ticket for the alternative equivalent ticket in the lottery plus a trading bonus of 25 €-cent. The outcome of this lottery is unrelated to the lottery you are participating in. The subjects in that treatment decided together by vote; the majority of the votes determined whether all traded or kept their tickets.

You have the option to get informed about the decisions these participants made in the earlier study. You will learn what percent of the participants traded their ticket and the percentage of subjects, who kept it. If you want to receive this information, you have to wait 1 minute before the data will be available on your screen. The information will be displayed for the time of one minute before you are automatically transferred to a screen that presents you with the choice whether you want to *Trade* or *Keep* your ticket.

If you do not want to receive this information you will be immediately transferred to the screen that presents you with the choice to either Trade or Keep your lottery ticket.

[New Screen]

4.2. Decision on Information

Do you want to be informed about the trading choices of the participants in the earlier study? YES or NO?

○ **YES** ○ **NO**

[New Screen: Only for subjects who selected "YES"]

4.2. Information on Trading Choices

You decided that you want to be informed about the trading choices of the participants in the earlier study. A majority of 89% of the participants voted for trading their tickets, 11% voted for keeping the tickets.

[New Screen: All subjects]

4.2./ 4.3. Your Decision [The Trade]

Do you want to trade your ticket for the alternative ticket of the lottery? YES or NO?

○ **YES** ○ **NO**

[New Screen]

5. REGRET

5.1. Regret [Trade *with* Majority Information]

Please imagine that you decided to get informed about the trading choices of the participants in the earlier study. You learned that 89% of these participants voted to trade their ticket. As these participants did, you decided to trade your own ticket and received the alternative ticket of the lottery in exchange. Assume that your original ticket won the lottery, while the new ticket that you hold now, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**

[New Screen]

5.2. Regret [Trade *without* Information]

Please imagine now that you decided not to get informed about the decision of the participants in the earlier study. You decided to trade your ticket and received the alternative ticket of the lottery in exchange. Assume that your original ticket won the lottery, while the new ticket that you hold now, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

G. 2 BLOCK INFORMATION Treatment

The following treatment *Block Information* differs from *Base* in that subjects are paid 25 €-cent for KEEPING their ticket rather than for trading it. I report only those parts of the instructions that differ from the *Base* condition. I do not report the Post-Experiment Questionnaire as it was the same as in *Herding*.

2.2. The Trade

You can trade your ticket in exchange for the alternative ticket in the lottery. If you decide to keep your original ticket instead you receive a bonus of 25 €-cent.

2.3. Consequences of the Trade

If you trade the ticket, then you will receive the alternative ticket of the lottery. Thus, if you had a "heads" ticket then you will receive a "tails" ticket and vice versa. If you decide against the trade, then you keep your initial ticket. Additionally, you get an extra 25 €-cent for keeping your original ticket.

Here is a summary of your choice options and their corresponding payoffs:

(1) *If you keep your ticket and ...*

(a) your original ticket is the one, you determined should win, then you get 4 € from the lottery and 25 €-cent for keeping the ticket.

(b) your original ticket is the one, you determined should lose, then you get 0 € from the lottery and 25 €-cent for keeping the ticket.

(2) *If you trade your ticket and ...*

(a) in exchange you received the ticket, you determined should win, then you get 4 € from the lottery.

(b) in exchange you received the ticket, you determined should lose, then you get 0 € from the lottery.

[New Screen]

4. DECISION

4.1. Optional Information

You can trade your ticket for the alternative ticket of the lottery or you can keep your ticket and receive an additional 25 €-cent.

In an earlier study, subjects were assigned a ticket for a lottery with a 50% chance of winning 4 € as you were. They could trade their ticket for the alternative equivalent ticket in the lottery. If they decided against the trade and kept their ticket, they received a bonus of 25 €-cent. The outcome of this lottery is unrelated to the lottery you participate in. The subjects in that treatment decided together by vote; the majority of the votes determined whether all traded or kept their tickets.

You will get informed about the decision these participants made in the earlier study. You will learn what percentage of participants traded their ticket and the percentage of subjects, who kept it. You have the option to block receiving this information. In that case you will not receive any information about how the participants in the earlier session decided. When you decide to block this information, you have to wait 1 minute before an empty screen is presented to you that otherwise would contain the data. The empty screen will be displayed for the time of one minute before you are transferred to a screen that presents you with the choice whether you want to *Trade* or *Keep* your ticket. If you do not want to block the information you will be immediately transferred to a screen that presents you with the data. Once you press the "CONTINUE" button, you can decide to either Trade or Keep your lottery ticket.

[New Screen]

4.2. Decision on Information

Do you want to block getting informed about the trading choices of the participants in the earlier study? YES or NO?

○ **YES** ○ **NO**

[New Screen: Only for subjects who selected "YES"]

4.3. Information on Trading Choices

You opted to be informed about the trading choices of the participants in the earlier study. A majority of 91% of the participants voted for keeping their tickets, 9% voted for trading the tickets.

[New Screen: All subjects]

4.3./ 4.4. Your Decision [The Trade]

Do you want to trade your ticket for the alternative ticket of the lottery? YES or NO?

○ **YES** ○ **NO**

[New Screen]

5. REGRET

5.1. Regret [Keep *without* Information]

Please imagine that you decided to block and not receive the information about the trading choices made by the participants in the earlier study. You decided to keep your original ticket. Assume that your original ticket lost the lottery, while the alternative ticket, which you could have received in exchange, won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below!

Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Keep *against* Majority]

Please imagine now that you opted to be informed about the decisions made by the participants in the earlier study. Assume that you learned that 92% of these participants voted to trade their ticket. Imagine that, in contrast to these participants, you decided to keep your original ticket. Assume that your original ticket lost the lottery, while the alternative ticket, which you could have received in exchange, won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

H. 2 MULTIPLE REFERENCE POINTS Treatment

I report only those parts of the instructions that differ from the *Base* condition. I do not report the Post-Experiment Questionnaire which was the same as in *Herding*.

4. DECISION

In earlier studies two groups of subjects were asked to decide whether to trade. They were assigned a ticket for a lottery with a 50% chance of winning 4 € as you were. They could trade their ticket for the alternative equivalent ticket in the lottery plus a bonus of 25 €-cent. In the first group, each subject decided alone whether she wanted to trade. A majority of 70% of these subjects decided to keep their original ticket and did not trade. In the second group, subjects were assigned an agent. As the lottery is random the agent did not have more information on its outcome and the benefits of the trade than the subject herself. The agent was paid only if he or she recommended the subject to trade; the agent was not informed about the lottery`s later outcome and the subject`s choice. A majority of 77% of these subjects accepted their agent`s recommendation to trade and traded their ticket.

Do you want to trade your ticket for the alternative ticket of the lottery? YES or NO?

O YES **O NO**

[New Screen]

5. REGRET

5.1. Regret [Trade]

Please imagine that you decided to trade your ticket as the majority of participants did who were assigned an agent in the earlier session you were informed about, but in contrast to the majority of those participants who decided alone and kept their ticket. Please assume that your original ticket, which you traded, won the lottery, while the new ticket that you received in exchange, lost.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

[New Screen]

5.2. Regret [Keep]

Please imagine now that you decided to keep your ticket, as the majority of those participants did, who decided alone in the earlier session you were informed about, but in contrast to the majority of participants who were assigned an agent and who traded. Please assume that your original ticket, that you kept, lost the lottery, while the alternative ticket of the lottery, which you could have received in exchange, won.

How much regret do you feel over not getting the 4 € payoff for winning the lottery?

Please mark one of the boxes you see below! Low values indicate that you expect to experience little regret; high values represent a strong feeling of regret.

1	2	3	4	5	6	7	8	9	10

V. Instructions for Chapter 6 – Self-Nudging

A. Instructions for Self-Nudging

[SCREEN 1]

Preliminaries

Thank you very much for participating in my experiment! Please notice the time restrictions that are imposed on you when fulfilling the experimental tasks. If you do not meet the restrictions, the experiment will be automatically terminated, though you will still be paid according to your performance up until that point in the experiment

As announced in the invitation e-mail you received, every 5th participant will receive a payment. Which of the participants gets paid will be determined randomly. All additional instructions will be provided in the course of completing the experiment.

[SCREEN 2]

The Contracts

You can form a legally valid contract with the experimenter about performing a task. Please note that the contract will be legally binding under section §241 BGB. Your task under the contract will be to count how often a particular digit is contained in a window displayed on your computer screen.

Contracts to Choose Between

You will be offered three different contracts that specify a number of screens you are obliged to count. However, by choosing between the different contract offers you can determine the number of tasks you want to perform yourself. Here are the three offers that specify a number of screens: The first contract offer would oblige you to count 5 screens, the second obliges you to count 15 screens while the last offer would oblige you to count 50 screens. The following paragraphs specifies the terms and earnings of the three contracts:

> "Agreed upon is the correct counting of 5 (15; 50) screens for a fixed payment of €15 (€15; €50). For each screen you count beyond the 15 (15; 50) you will receive a bonus payment of €1. If you count less than the agreed upon 15 (15; 50) screens, you breach the contract. In this case, you will owe a penalty of €1 for each screen short of the 15 (15; 50) agreed upon screens“

By contrast the fourth contract you are offered does not specify a number of screens you are obliged to count. For the fourth contract the following paragraph of the contract stipulates your earnings:

> "Agreed upon is that you will be paid €1 for each screen that you count.”

You can choose between the four offers and select the contract you prefer. After you have made your choice, your preferred contract will be offered to you and can you decide whether you want to agree to the contract or not. You are free to agree to or reject any of the contracts.

[SCREEN 3]

Comprehension

To make sure that you fully comprehend the instructions please fill out the following questions:

Please assume you expected to count 12 tables but went on to complete 10 tables in the task. How much would you be paid? Please type in the amount in cents you would receive:

[SCREEN 4]

Indicate Your Expectations

Recall that I will present you the four contracts in random order. Please first type the number of screens you expect to complete under the following contract:

> "Agreed upon is the correct counting of 5 (15; 50) screens for a fixed payment of €15 (€15; €50). For each screen you count beyond the 15 (15; 50) you will receive a bonus payment of €1. If you count less than the agreed upon 15 (15; 50) screens, you breach the contract. In this case, you will owe a penalty of €1 for each screen short of the 15 (15; 50) agreed upon screens.“

Type in the Number of Screens you Expect to Complete under the above Contract:

(Note, subjects were presented with one separate screen for each contract. The paragraph for the Standard contract states:

> **”Agreed upon is that you will be paid €1 for each screen that you count.”)**

[SCREEN 5]

Selecting Who is Paid According to Expectations

I will now select the participants who will be paid for the expectations you just indicated. To assure you that the selection process is indeed probabilistic, I will present you on the next screen with the picture of a real-world house. I have taken the photo so you will not find the photo on the internet. The house has a street number between 1 and 5. After you have seen the picture of the house you will be asked to indicate a number between 1 and 5. On the following screen I will reveal the true street number of the house. If you indicated the true number, then you are selected and will be assigned a contract randomly. You will be paid according to the expectation that you indicated for the assigned contract and your performance in the effort task.

If the number you indicate is not correct, then you are transferred to the next experimental stage, where you can choose the contract that you want to be offered.

Please enter a number between 1 and 5 in the box below:

[SCREEN 6]

Selection

The true street number of the building is: **5**

The full address of the house is 10012, 8th street No. **5**. You may now check the building on Google Street View (LINK). You will see that street view shows the building on the photograph at this address.

As you indicated a wrong number, you are not selected for payment according to your expectations. Thus, the expectations you indicated will not be payoff relevant for you. You are now transferred to the next stage of the experiment, where you can choose one of the four contracts you were presented with. The contract you choose will be offered to you for your acceptance.

[SCREEN 7]

Choosing the Preferred Contract

You can now choose between the four contracts I presented you with. Recall, the three threshold contracts oblige you to either count 5, 15 or 50 tables. The fourth contract prescribes only your payment, but does not oblige you to count a particular number of screens.

Please enter the number of screens you want the contract to oblige you to count in the box below. For one of the threshold contracts you enter either a "5", "15" or "50". For the contract that does not prescribe a particular number of screen type in "0".

Please enter the number in this box:

[SCREEN 8]

Offering the Preferred Contract

You are offered the contract you have chosen. Your earnings are specified in the following paragraph of your contract:

"Agreed upon is the correct counting of 5 screens for a fixed payment of €15. For each screen you count beyond the 15 you will receive a bonus payment of €1. If you count less than the agreed upon 15 screens, you breach the contract. In this case, you will owe a penalty of €1 for each screen short of the 15 agreed upon screens"

If the Standard Contract was chosen:

" Agreed upon is that you will be paid €1 for each screen that you count."

Accepting the Contract

On the next screen you will be asked whether you want to agree to the contract, including the conditions you selected. You will be able to click two buttons that will be displayed on the screen: one for "Yes" if you want to agree to the contract as it is offered to you; and one for "No" if you want to reject the contract. If you click on the "No" button, then your session will be terminated. If you agree to the contract, then it is binding by law under section §241 BGB. Once you have agreed to the contract, the experimental task will start immediately.

[SCREEN 9]

Table # 1

Please count how often the digit "1" is contained in the table below. Please write the correct number in the small box below. You can only move on to the next screen if you enter a count that is considered correct.

A	5	3	5	2	1	4	2	6	4	7	3	5	1	7	5	8	9	3	9	7
B	2	4	3	6	1	3	5	0	1	7	8	5	0	2	6	0	9	9	2	2
C	2	7	1	9	0	6	9	3	6	5	3	8	2	4	2	6	4	8	9	1
D	3	4	5	2	7	1	5	2	9	5	0	7	8	8	4	6	2	2	1	0
E	2	0	6	8	7	7	2	5	1	7	3	0	5	3	7	1	9	0	1	6
F	5	3	1	6	4	8	1	5	2	9	8	0	5	0	3	9	4	5	2	7
G	1	2	6	1	7	8	3	0	4	7	2	8	3	8	6	1	7	3	8	4
H	1	5	2	8	5	0	1	6	3	8	3	9	0	0	4	7	2	8	2	7
I	5	5	2	8	4	9	1	6	2	8	3	0	2	4	1	6	2	7	4	9
J	8	3	7	2	8	5	9	3	7	1	8	3	7	5	0	1	5	3	8	1

[SCREEN 10]

Repeating the Task

Your entry was incorrect. Please decide whether you want to recount the same table or not. If you want to repeat the screen, please click on the "Yes" button. If you do not want to continue the task, please click the "No" button.

Yes No

[SCREEN 11]

Continuing the Task

Your entry was correct! Please decide, whether you want to continue fulfilling the contract or not. If you want to continue, please click on the "Yes" button; if you want to stop, please click on the "No" button. In this case you will be transferred to the next stage of the experiment.

Yes No

[SCREEN 12]

Participation in Lotteries

In the next part of this study, I ask you to decide whether you want to participate in two lotteries. You can increase your earnings by participating, but you can also lose some portion of whatever you may have earned in the earlier parts of this experiment.

Note: The two decisions whether or not to participate in the first or second lottery are independent of each other. Therefore, you can decline to participate in one lottery, but take part in the other; you can also reject both lotteries, or participate in both. The outcome of the lotteries will be determined by this week`s Eurojackpot lottery. If the last drawn number is even, the lottery is won, if it is odd, the lottery is lost. Your earnings will be added to your account, while your losses will be subtracted.

Please click "Yes" on the next screen if you wish to participate in the lottery and "No" if you do not wish to participate.

Yes No

[SCREEN 12]

Participation in Lotteries

In the next part of this study, I ask you to decide whether you want to participate in two lotteries. You can increase your earnings by participating, but you can also lose some portion of whatever you may have earned in the earlier parts of this experiment.

Note: The two decisions whether or not to participate in the first or second lottery are independent of each other. Therefore, you can decline to participate in one lottery, but take part in the other; you can also reject both lotteries, or participate in both. The outcome of the lotteries will be determined by this week`s Eurojackpot lottery. If the last drawn number is even, the lottery is won, if it is odd, the lottery is lost. Your earnings will be added to your account, while your losses will be subtracted.

Please click "Yes" on the next screen if you wish to participate in the lottery and "No" if you do not wish to participate.

Yes No

[SCREEN 13]

Lottery 1

Lottery 1 has the following payoff: You win €8 with a probability of ½ and lose €5 with a probability of ½. If you participate in this lottery, either €8 will be credited to your account or €5 will be deducted from your account. If you reject to participate in the lottery your earnings in this part of the experiment are €0, that is, you neither win an additional payoff, nor do you lose earlier earnings.

Please click on the "Yes" button if you want to participate in the lottery and the "No" button if you do not want to participate in Lottery 1.

Yes No

[SCREEN 14]

Lottery 2

Lottery 2 has the same payoff as Lottery 1: You win €8 with a probability of ½ and lose €5 with a probability of ½. However, the lottery is repeated six times, that is, there are six independent payoffs which will be consolidated in one overall payoff by adding and subtracting. Here is an example: Assume you win €8 three times and lose €5 three times. The overall payoff for Lottery 2 would be €9 [(3x8) – (3x5)=9].

If you reject to participate in Lottery 2 your earnings in this part of the experiment are €0, that is, you neither win an additional payoff, nor do you lose earlier earnings.

Please click on the "Yes" button if you want to participate in lottery 1 and the "No" button if you do not want to participate in Lottery 1.

Yes No

Instructions of the Single Contract Treatments

[SCREEN 1]

Preliminaries (see above)

[SCREEN 2]

The Contract

You can form a legally valid contract with the experimenter to perform a task. Please note that the contract you can form will be legally binding under §241 BGB. Your task is to count how often a particular digit is contained in a window displayed on your computer screen. Your earnings are specified in the following paragraph of your contract:

> "Agreed upon is the correct counting of 5 (15, 50) screens for a fixed payment of €5 (15, 50). For each screen you count beyond the 15 you will receive a bonus payment of €1. If you count less than the agreed upon 5 (15, 50) screens, you breach the contract. In this case, you will owe a penalty of €1 for each screen short of the 5 (15, 50) agreed upon screens."

[Standard Contract]

"Agreed upon is that you will be paid €1 for each screen that you count €1."

Entering the Contract

After reading the instructions you will be asked whether you agree to the conditions presented and want to enter into the contract. You can choose between two buttons that will be displayed: one for "Yes" if you want to accept the contract; and one for "No" if you want to reject the contract. If you click on "No" your session will be terminated. If you enter into the contract it is binding by law and the experiment will start immediately.

[SCREEN 3]

Contract Formation

You can now enter the contract with the terms described above. Please note that this contract is legally binding under § 241 BGB. If you want to enter into the contract with the mentioned terms, then please check the "Yes" box.

If you want to reject the contract please mark the "No" box. Your session will be terminated if you do not accept the offered contract.

Yes No

[SCREEN 4]

Table # 1 (the experiment begins followed by the loss aversion measure).

B. Alternative Loss Aversion Measure

In the following table you find a list of coin tosses with different outcomes. For each row you need to indicate whether you want to toss the coin or not. The outcomes differ in how much you lose if the coin turns up heads. As you cannot watch us throw a coin, I take the Eurojackpot lottery [Link] instead: If the last number drawn in the lottery is even the toss is won, if the number is odd, the toss is lost.

To determine your payoff one of the six rows will be randomly selected. If you have indicated that for the randomly selected row you want to toss the coin, then you will win, if the drawn number is even, and lose if it is odd, you will be paid accordingly. For the selection of the row, I use the last additional

Euro-number. It can range from 1-12. The first toss is selected if the Euro-number is either 1 or 6; the second if it is either 2 or 7; the third: 3 or 9; forth: 4 or10; fifth:

Please indicate for each row, whether you want to toss the coin or not.

	Toss	NO Toss
If the number is even you lose €2; if the number is odd you win €6.		
If the number is even you lose €3; if the number is odd you win €6.		
If the number is even you lose €4; if the number is odd you win €6.		
If the number is even you lose €4; if the number is odd you win €6.		
If the number is even you lose €5; if the number is odd you win €6.		
If the number is even you lose €6; if the number is odd you win €6.		
If the number is even you lose €7; if the number is odd you win €6.		

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Tontrup works in the field of empirical legal studies and experimental law and economics; he is interested in jurisprudence and the impact of new technologies on legal policy, regulation and private ordering. His empirical research focuses on contract law, behavioral contracting, privacy law, public goods provision, and voting behavior. He also uses experimental methods for cross-cultural comparisons measuring differences in preferences for institutions across countries, with a particular focus on China. Tontrup conducts laboratory, internet and field experiments to analyze the cognitive mechanisms of decision making in legal institutions. In this dissertation, he develops a theory of behavioral self-management that shows that people consciously use legal institutions to control biases such as loss aversion or hyperbolic discounting. Individuals can even use these biases to their advantage - in one set of experiments for example, they are shown to utilize their loss aversion to improve their performance in fulfilling a work contract. Tontrup's current research extends this theory to the area of social preferences with applications in contract law and corporate governance. In his economic PhD he analyzes the economic costs of legal signals that cause counterproductive effects, such as crowding out, or the psychological costs of aspirational rights that are violated.