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Contents

List Of Figures

List Of Tables

Introduction

Our decisions shape what we do, who we are and how our future looks like. Understanding human decision making is therefore one of the prime interests of microeconomics research. Hereby a variety of different aspects need to be considered and often a complex interplay of external factors, like institutional settings or incentives, but also internal factors, such as expectations and perceptions need to be considered when trying to unravel the decision making process. The individual's perception about the decision context and the expected potential outcomes of a choice have been shown to be determinative of the decision making process, as ultimately, it is not the *actual* outcome, but the *expected* outcome that drives decision making.
While understanding the role of individuals' perceptions for decision making is vital in many contexts, perceptions related to educational decision making are particularly consequential. Education is not only one of the biggest drivers for individual wealth, but promoting education is also seen as one of the remedies for social inequality and development (e.g. OECD, 2023; Walker et al., 2019). When investigating how choices on educational attainment turn into labor market outcomes, two perspectives on the labor market need to be considered. On the one hand, young individuals make a range of educational decisions early in their life. They choose to pursue (higher) education, which educational program they follow, how much effort they exert, and if they complete their degree or eventually drop out from an educational program. Thereby, examining students' expectations about the labor market outcomes of different educational pathways is crucial to understand their choices. On the demand side of the labor market, employers make hiring decisions based on applicant résumés and educational credentials. Understanding the role of their believes is equally relevant for assessing how education translates into labor market outcomes. Only if we understand the process behind the formation of educational returns, it is possible to mediate educational decision making through incentives, institutions and policies in order to ensure individual labor market success and enhance overall education. The first two chapters of this thesis are devoted to the above issues from the field of education economics. They investigate expectations of students and employers respectively, attempting to unravel the underlying belief-related mechanisms behind educational decisions from both sides of the labor market.

The third chapter looks at decision making from a different perspective. Instead of trying to understand the perceptions underlying the decision making process, it looks at how individuals react if their freedom of choices is encroached. Paternalistic policies are the main instrument for governments to remedy behavioral biases and manipulate decision making in a way that is individually beneficial, societal desirable or both. There are plenty of examples of well-designed paternalistic policies, that have altered decision making successfully in a preferable way for society. The most renowned, but equally controversial, example is the default regulation for organ donations. Johnson and Goldstein (2003) show that using an opt-out policy for the decision to become an organ donor increases the number of donors by thousands every year in the United States compared to an opt-in approach. However, policy design should not only be judged by its effectiveness, but also by the perception of the ones who are constraint. Interference in individual freedom of choice is often emotionally charged, potentially leading to undesired behavior like reactance (e.g. Arad and Rubinstein, 2018). The Covid-19 pandemic has shown how paternalistic polices against the spread of the virus led to far-reaching individual and social repercussions (e.g. Díaz and Cova, 2022). The third chapter therefore examines how individuals perceive a paternalistic constraint, which is a crucial facet of steering decision making, that gained even more importance in light of the corona measures during the pandemic.

The three chapters of this thesis are independent research papers dealing with understanding individual decision making in the broader sense. Each chapter draws on insights from applied microeconomics, especially from the fields of education, labor economics and experimental economics. The papers are based on empirical analysis, utilizing different data sets. In the following, I briefly summarize each chapter, the underlying data and the main results.

Chapter 1 - The (Expected) Signaling Value of Higher Education. This chapter explores students' expectations about the returns to completing higher education and provides first evidence on *perceived* signaling and human capital effects. We elicit counterfactual labor market expectations for the hypothetical scenarios of leaving university with or without a degree certificate. We make use of a large and diverse sample of German university students, that are at different stages of higher education. Next to the information on students' expectations, the data also comprise rich information on their current studies and background characteristics. The within student variation in expected educational returns, that our study design yields, allows us to circumvent common identification problems that arise when attempting to identify the origins of the returns to education.

Our findings indicate substantial expected labor market returns of around 20% in terms of starting wages for a master degree. Besides, perceived educational returns stem predominantly from signaling, exceeding the perceived productivity-enhancing returns of education (human capital) by 3-5 times during the study time. Over the expected course of career, we find lasting education premia as well as evidence consistent with employer learning.

Chapter 2 - The Impact of Higher Education on Employer Perceptions. The second chapter is concerned with the research question, why employers actually seek to attract individuals with more education. The question attempts to open the black box of the formation of educational returns. To achieve this, we conduct a survey experiment among a large pool of human resource managers, who are actively involved in hiring. We experimentally vary rates of master degree completion on applicant résumés to shift employer beliefs about candidates' productive traits. First, we measure candidate attractiveness in terms of probability to invite for an interview and to make a job offer and the offered wage conditional on hiring. Second we elicit managers' beliefs about each candidate's expertise, cognitive and non-cognitive traits and socio-economic background. Our results first of all confirm that a master's degree raises candidate desirability substantially. That is, candidates who have completed a master's degree are 4.5 percentage points more likely to be invited to a job interview, have a 3.6 percentage points higher likelihood of receiving an offer and have a higher earnings potentials by 4.8% all compared to bachelor graduates. On the contrary, having passed nearly all courses, but not having obtained a master degree leads to a reduced invitation probability of 2.3 percentage points (3.4%). Second, we find that master graduates outperform bachelor degree holders in terms of employers' perceived cognitive and non-cognitive traits as well as subject matter expertise by around 20% of a standard deviation. Conversely, master dropouts are associated with weaker non-cognitive traits. Third, a decomposition analysis reveals that these perceived traits account for up to 75% of candidate attractiveness. This paper thus provides causal evidence on employer beliefs during hiring decisions and reveals the mechanism behind the immediate returns to education.

Chapter 3 - The Monetary Value of Freedom of Choice. The third chapter explores attitudes towards paternalism in an incentivized laboratory experiment. We test if individuals are willing to give up money in order to remove a paternalistically motivated constraint that restricts their choice set in a decision under risk.
We find that individuals are willing to give up money in almost half of their decisions, in order to be able to make an unrestricted choice. Moreover, our experimental design allows us to disentangling the intrinsic value from the instrumental value of freedom of choice. We observe positive intrinsic values for freedom of choice in about 30% of all decisions which amounts to around 2€ on average in this experimental setting. Besides, occurrence and magnitude of the intrinsic value of freedom of choice vary substantially across decision contexts, i.e., for different types of risks (gains versus losses, long shots versus 50/50). Varying stake sizes affects the magnitude of the value of freedom of choice, but not the occurrence. Last, we find individual level differences that are in

line with existing survey literature. Overall, the results confirm that individuals have a monetary intrinsic value of freedom of choice, that varies with decision context and individual characteristics. The individual attitude towards paternalism is therefore not an universal concept applying equally to all individuals and decisions. It can rather be seen as a context specific reaction, that is affected by numerous factors.

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Chapter 1

The (Expected) Signaling Value Of Higher Education

Joint with Pia Pinger and Renske Stans

1.1 Introduction

Higher education is a major determinant of labor earnings as university graduates earn substantially more over the life cycle than individuals with a high-school degree (Cunha et al., 2011; Piopiunik et al., 2017; OECD, 2017; Altonji and Zhong, 2021). The importance of education for labor market outcomes is rationalized in economic theory (Becker and Chiswick, 1966; Mincer, 1958, 1974) and has been documented in a vast body of empirical literature (for reviews see e.g. Card, 1999; Patrinos and Psacharopoulos, 2020). Moreover, many papers show that individuals are aware of existing returns and adopt their educational decision-making accordingly (Dominitz and Manski, 1996; McMahon and Wagner, 1981; Manski, 2004; Delavande and Zafar, 2019).

The sources of the education premium are less well understood. According to the human capital hypothesis (Becker, 1962; Schultz, 1963; Mincer, 1974) education augments productivity because individuals acquire knowledge and useful skills during their studies. Contrary to this, the signaling hypothesis pioneered by Spence (1973) and Stiglitz and Weiss (1990) advocates that education is merely a signal of productivity. Here, the (psychic) costs of education correlate with worker productivity such that a separating equilibrium emerges where high-productivity individuals use education as a signal to earn higher wages and firms screen workers for their education to attract high-productivity-type workers.[1]

The corresponding empirical evidence on the relative importance of human capital

[1]A third hypothesis states that (higher) education premia arise because university attendance is a screening or selection device that induces students to resolve uncertainty about their individual returns. According to this presumption, only those students with sufficiently large returns decide to finish a degree (Chiswick, 1973; Lange and Topel, 2006).

versus signaling effects for (higher) education premia remains largely inconclusive (Patrinos and Psacharopoulos, 2020). While some studies report findings in support of the human capital hypothesis (e.g. Layard and Psacharopoulos, 1974; Chevalier et al., 2004; Kroch and Sjoblom, 1994; Aryal et al., 2022) others report substantive evidence of signaling effects (Hungerford and Solon, 1987; Jaeger and Page, 1996; Park, 1999; Bedard, 2001; Chatterji et al., 2003; Caplan, 2018). This discrepancy arises because both theories are observationally equivalent: *Ex-post*, individuals with education credentials are more productive, which entails a positive relation between education and wages.[2]

In this paper, we circumvent this identification problem and provide first evidence on the perceived *ex-ante* signaling value to higher education. In particular, we ask two questions: Do students anticipate considerable premia to obtaining higher education? If so, do they ascribe them to the human capital acquired or the signaling value of the degree certificate?

To answer these questions, we have collected novel data on subjective pecuniary and non-pecuniary returns to finishing higher education in a large sample of students currently enrolled at a university or college of applied sciences in Germany. Understanding the perceptions of enrolled students is important, as the distinction between signaling versus human capital is detrimental for their decision whether to continue studying or not. We elicit expected wage information among individuals who are at different stages of higher education for the hypothetical scenarios of leaving university with or without a degree certificate. Besides, the data comprise information on expected job satisfaction, the probability of finding a suitable job, expected working hours, and a large array of background variables. All expectations were elicited for the time when individuals start working and at two later points in the life cycle (at the age of 40 and 55). The data thus allow us to circumvent selection and estimate *ex-ante within-individual* graduation premia as well as to distinguish between the *perceived* signaling and human capital values of higher education.

The analysis proceeds in three steps. First, we provide general evidence on the expected returns to continued higher education, including estimates of the perceived lifetime return on investment and the perceived internal rate of return. Second, using expected wages for counterfactual scenarios of leaving university with or without a degree, we estimate within-person fixed effects models to obtain perceived wage and non-wage (job satisfaction, probability of finding a suitable job) signaling and human capital values of education. As part of this analysis, we also unveil the perceived long-term development of the graduation premium, i.e., the expected persistence of signaling and the respective importance of employer learning. Third, we investigate heterogeneities in the signaling value and the importance of returns for leaving university without a

[2]For a long time, this identification problem seemed insurmountable. As an example, Lang and Kropp (1986) write: "[M]any members of the profession maintain (at least privately) that these hypotheses cannot be tested against each other and that the debate must therefore by relegated to the realm of ideology." See also Huntington-Klein (2021).

degree.

Our estimates for master's students indicate high perceived individual returns to degree completion, with an average discounted lifetime return of €334,400. Moreover, the model parameters from a within-person fixed effects analysis suggest that signaling yields a 20 percent return in terms of starting wages, more than a standard deviation in terms of job satisfaction, and more than half of a standard deviation regarding appropriate employment. At the same time, the estimated human capital value is very small and mostly not significantly different from zero. We thus observe a considerable perceived labor market advantage of an individual who recently received a credential over someone who is just about to receive it. We also find lasting effects of the graduation signal, meaning that even in the long term a student expects to earn more in the graduation scenario compared to the scenario of leaving university without a degree despite perceived employer learning. Finally, by exploring subjective leaving probabilities, we find that the expected earnings premium plays a rather small role in the choice to leave university without a degree as compared to variables that proxy for student satisfaction or psychic costs. This finding is congruent with a large body of literature documenting small educational choice responses to monetary incentives (e.g., Arcidiacono, 2004; Beffy et al., 2012; Wiswall and Zafar, 2015). It is also in line with the signaling hypothesis, which implies homogeneous returns to finishing a certain degree, but differential costs of studying. In other words, the decision to select out of education should be driven by the (psychic) cost of education only, and not the potential earnings gain from finishing.

Whether education premia arise due to human capital augmentation or signaling holds important implications for young people's motivation to obtain higher education, as well as their educational decision-making. If education merely increases productivity, then for individuals who want to work in a high-productivity job or position, attending higher education (or at least studying the material) is without alternative. However, if education only relates to signaling, high-productivity types will only obtain a degree if there is no other, cheaper (but equally credible) way to document their future productivity. Similarly, if signaling prevails, leaving a higher educational institution (shortly) before obtaining the degree is very costly in terms of later wages, while it should matter little under the human capital hypothesis.[3] The aim of this paper is thus to explore perceived signaling and human capital values as they can determine students' decision-making. Yet, our findings may have more general implications, given a high average accuracy of reported wage expectations in our data.

The analysis in this paper builds upon and extends prior work regarding the importance of so-called graduation premia, signaling, diploma, or sheepskin effects (see e.g.

[3]The type of regime also has implications for societal investments. For example, if education augments human capital, society may subsidize it to reap positive externalities in the form of productive worker interactions, better citizenship, or knowledge spillovers. If education is simply a means to convey information, society might as well leave it to the individual to pay for it, unless it effectively reduces uncertainty about the quality of labor input to firms, which may increase total output (Wolpin, 1977).

Weiss, 1995; Lange and Topel, 2006, for reviews). Part of this research relies on a matching assumption for identification, as researchers regress wages on the number of years of schooling and degree attainment and then interpret the wage differential between degree and non-degree workers conditional on years of schooling as signaling (Hungerford and Solon, 1987; Frazis, 1993; Jaeger and Page, 1996; Park, 1999; Altonji and Pierret, 2001; Ferrer and Riddell, 2002).[4] Another part uses instruments or discontinuities to identify the graduation premium for individuals at the margin (see e.g. Acemoglu and Angrist, 1999; Tyler et al., 2000; Clark and Martorell, 2014; Barrera-Osorio and Bayona-Rodríguez, 2019). Similarly, some papers exploit changes in the curriculum, years, or intensity of schooling to investigate exogenous changes in the human capital accumulation process on wages (see e.g. Arteaga, 2018; Goodman, 2019). Our approach complements this literature in two respects. First, we only look at the supply side, i.e., by estimating signaling effects among (future) labor market participants, thus abstracting from equilibrium effects. Second, we estimate the graduation premium from within-person variation, enabling us to estimate average instead of local effects.

This paper also adds in general to the literature on subjective expectations. In particular, it relates to work on the role of expectations of returns when making educational decisions, such as starting tertiary education (Boneva and Rauh, 2017; Attanasio and Kaufmann, 2014, 2017), major and occupation choice (Arcidiacono et al., 2017; Wiswall and Zafar, 2015) or completing tertiary education (Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2016; Hastings et al., 2016). While much of this work relies on data from small, selective samples, we can rely on a dataset that allows us to make statements about a substantive population of students.

In addition, our findings pertain to a large body of literature on employer learning (Farber and Gibbons, 1996; Altonji and Pierret, 2001). This research investigates the extent to which statistical discrimination by employers based on degree signals fades over time as employers learn about the true underlying productivity of new employees (Farber and Gibbons, 1996; Lange, 2007). It also shows that employer learning may differ by the type of degree or the observability of educational content (Arcidiacono et al., 2010; Bauer and Haisken-DeNew, 2001; Aryal et al., 2022). We add to this strand of research by providing insights into the extent to which individuals anticipate signaling and employer learning effects to affect their wages in the longer run.

Finally, our paper relates to research on the role of psychic costs and non-monetary outcomes for educational decision-making (Cunha et al., 2005; Heckman et al., 2006; Jacob et al., 2018; Boneva and Rauh, 2017; Belfield et al., 2020). This literature documents that both psychic costs and non-pecuniary factors are important determinants of educational decision-making, which is in line with our findings that the perceived monetary returns matter little for the decision to complete a degree.

[4] See also Fang (2006) for a structural model of education choices to disentangle signaling and human capital effects.

The remainder of the paper is organized as follows. In section 1.2, we provide information on the data collection procedure, describe our sample and main measures. Section 1.3 provides descriptive insights into the data. Subsequently, section 1.4 contains our empirical strategy and main results for the perceived signaling value. Section 1.5 then tests two implications of the signaling theory. Finally, section 1.6 concludes.

1.2 Data

This section provides detailed information on our sample and questionnaire measures. We start by describing the data collection procedure, before we report on our measures related to expected labor market outcomes, future employment, university experience and various background characteristics. Finally, we present summary statistics of the main background variables.

1.2.1 Data Collection

Our sample was recruited as part of the German student study "Fachkraft 2020" (now called "Fachkraft 2030").[5] Students on the mailing list of a popular nationwide job board were contacted via email and asked to complete an online questionnaire with items related to future labor market expectations, current study experiences, university dropout and a broad range of background characteristics.[6] The surveys were conducted in September 2014 and March 2015 and participation in the study was incentivized using Amazon vouchers amounting to €5,000.

1.2.2 Measures

Labor market expectations. As we are interested in individuals' expected labor market outcomes for different studying scenarios, we obtain students' counterfactual labor market expectations. Specifically, we elicit job prospects for two different scenarios: (i) when students graduate from their preferred major (graduating scenario) and (ii) when they leave university without obtaining any further academic degree (leaving scenario), see appendix section 1.A for the survey items. As we exploit the fact that students are in different stages of their studies, we assume that for the leaving scenario students think about leaving university immediately, and hence their current semester is seen as the semester in which they would hypothetically leave. For students in a later semester of studying, this is consequential, as there is not much time left in which they could leave university. For students at the start of their studies, it is reasonable to

[5] See Seegers, Philipp and Bergerhoff, Jan and Hartmann, Stephan and Knappe, Anne (2016) for more information.

[6] The data were collected via the job board jobmensa.de operated by Studitemps GmbH. It is the largest platform for student jobs in Germany.

assume that in the leaving scenario students would expect to leave university immediately due to the high opportunity costs of studying. For each scenario, students indicate their expectations with respect to gross yearly labor earnings, weekly working hours, the probability of finding a suitable job, and job satisfaction, where the latter is measured on a scale from 1 to 10.[7] From the specified earnings and working hours, we construct expected hourly wages and full-time wages.

Moreover, in order to gain a better understanding of the development of perceived labor market expectations over the life course, all wage expectations were elicited for three different points in time: at the age when a person first starts working, at the age of 40 and 55.[8] With this information, we compute lifetime wage trajectories by assuming a standard Mincer-type earnings function where wages ($W_i^c(t)$) are a quadratic function of work experience:

$$W_i^c(t) = \alpha_i^c + \beta_i^c experience_i^c(t) + \gamma_i^c (experience_i^c(t))^2 \tag{1.1}$$

Experience in time t is calculated by deducting the expected age at labor market entry from the age at time t.[9] We solve equation (1.1) for each individual i and counterfactual c to obtain scenario- and individual-specific parameters β_i^c and γ_i^c.[10] Then we use these parameters to compute expected wages for each year of a person's working life for both the graduating and leaving scenarios.

In accordance with the literature (see Polachek et al., 2008, for a review), concave wage trajectories (in experience) are most prevalent in our data with 69.9 percent for the graduating scenario and 45.3 percent for the leaving scenario (see appendix figure 1.E2). Convex wage growth pattern come in second, with 24.4 percent (graduating) and 31.8 percent (leaving) respectively. Only 5.5 percent of students expect a linear increase in earnings after graduating, and 21.7 percent after leaving. A small proportion remains unclassified, which mainly originates from expected wage developments that decrease over time. For the scenario of leaving university we observe more linear and convex patterns, which is mainly due to lower initial wage growth (see appendix figure 1.E3). This observation is in line with a body of literature showing that actual wage growth is steeper for higher levels of schooling (Belzil, 2008; Dustmann and Meghir, 2005).

Future employment. Respondents were asked about the profession they plan to pursue after graduating from their current studies. They could choose out of 429 predefined occupations or make use of a free text field. This information allows us to classify whether people plan to pursue a profession that is legally regulated, meaning that individuals

[7] In the survey students were asked for the probability of *not* finding a suitable job. However, for readability we recode this as the job-finding probability.

[8] Expected job satisfaction and the probability of finding a suitable job were only elicited for labor market entry and the age of 40, not for the age of 55.

[9] Students indicated their current age and how long they still need to study until they finish their degree. With this information, we were able to calculate the expected age at labor market entry.

[10] See appendix figure 1.E1 for the distribution of parameters β and γ.

need to have a license in the form of a (specific) degree to pursue this occupation. We follow the classification of the German federal employment agency for regulated professions (Bundesagentur für Arbeit, 2020). Typical occupations for which this applies are physicians, lawyers or engineers. In addition, we elicit whether individuals aim for a civil servant job, i.e., with fixed wage regulations according to experience and education. This information allows us to control for a licensing effect after graduation. In our sample roughly 30 percent of students plan to pursue a licensed or civil servant occupation.

University experience. The survey also contains questions about various aspects of students' university experience. First, with respect to the study phase, we asked which degree respondents aim to obtain. In addition, we asked how many semesters they have studied, both with respect to their current studies as well as overall, and how many semester they still expect to need to finish their current degree.[11] Second, respondents were asked to report their study subject from a list of fifteen study fields. We group these subjects into five main categories: medicine/health, STEM, law, economics, and humanities/social sciences. Third, to obtain a measure of performance, we elicited students' grade point average. Furthermore, we asked them to estimate their perceived relative position in the distribution of all students regarding academic ability and work-related ability on a scale from 0 to 100. Fourth, to better understand the relevance of the leaving scenario, we asked students about their perceived probability of leaving university *without any further degree*, where this probability excludes switching to an alternative university study. Finally, we elicited their overall satisfaction with their studies.

Background characteristics. We also collected data on a broad range of individual characteristics, such as gender, age, migrant background, and state of residence. Moreover, we inquired about respondents' high-school GPA to have information on pre-university performance. Finally, we asked individuals to state whether neither, one, or both of their parents attended university, which is a proxy for socioeconomic background. For an overview on the most relevant variables, see table 1.1.

1.2.3 Sample Characteristics

After dropping observations with implausible wage returns or missing explanatory variables, we obtain a sample of 6,306 students.[12] Table 1.1 provides summary statistics of the main background variables for our sample and for the entire population of students in Germany in the 2014/2015 academic year. The table shows that our sample closely compares to the overall population of German students in terms of age, migration background, region, degree type and high-school GPA. An exception is that females

[11] In Germany, only roughly 30% of all students obtain a degree in regular study time (Destatis, 2018). Often internships, side jobs or stays abroad prolong the study time. We thus obtained both semesters studied and semesters left to study to approximate the students' current stage of studying.

[12] See section 1.B in the appendix for more information on the data-cleaning procedure.

Table 1.1: Summary statistics

		Our sample	Student cohort 2014/15*
Age		23.5	23.4
Male (%)		47.1	52.2
Migration background (%)		16.7	16.2
Federal state (%)	Baden-Wuerttem.	11.4	13.2
	Bayern	17.0	13.6
	Berlin	7.1	6.3
	Brandenburg	2.0	1.8
	Bremen	1.7	1.3
	Hamburg	2.8	3.6
	Hessen	8.7	8.8
	Mecklenburg-Vorp.	1.5	1.4
	Niedersachsen	7.1	7.1
	Nordrhein-Westfalen	23.3	26.9
	Rheinland-Pfalz	4.8	4.5
	Saarland	0.5	1.1
	Saxony	4.5	4.2
	Saxony-Anhalt	2.5	2.0
	Schleswig-Holstein	2.8	2.1
	Thueringen	2.4	1.9
Bsc. student (%)		77.0	78.1
Subject (%)	Medicine	5.7	6.0
	STEM	37.4	39.2
	Law	1.3	4.9
	Econ.	29.3	15.5
	Human./Social	26.3	34.5
High-school GPA		2.42	2.45
Observations		6,306	2,698,910

Notes: Table 1.1 compares the summary statistics of several background characteristics between our sample and the overall German student cohort in 2014/15. In Germany, the best grade is 1.0 and the worst passing grade is 4.0. The statistics for the total student cohort originate from Destatis (2020) and Govdata (2020).

are slightly overrepresented, potentially due to higher responsiveness to surveys among females in general (Molarius et al., 2019). In addition, there are 29.3% economics students in our sample, which is 15 percentage points more than the population share in this subject category. This higher share of economics majors mainly comes at the cost of a lower fraction of students in humanities, social sciences, and law. This imbalance might reflect that all students were approached via a job agency and having a side job could be more common for economics students. In our analysis, we take these differences into account, see section 1.4.

Our data vary in terms of respondents' study phase. For respondents aiming to obtain a master (bachelor) degree, 31.7% (10.0%) are in semester 1-2, 37.4% (26.0%) in semester 3-4, 19.6% (27.4%) in semester 5-6 and 11.3% (36.6%) in their 7th or higher semester. This variation is essential to estimate the value of human capital accumulation.

1.3 Descriptive Evidence

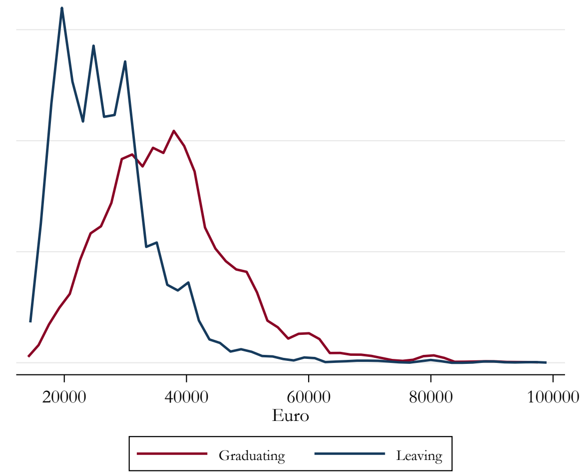
In this section, we first characterize the wage and non-wage returns that students perceive from both graduating and leaving university without a degree. Then, we provide descriptive evidence on where these returns originate from.

1.3.1 Perceived Wage Returns

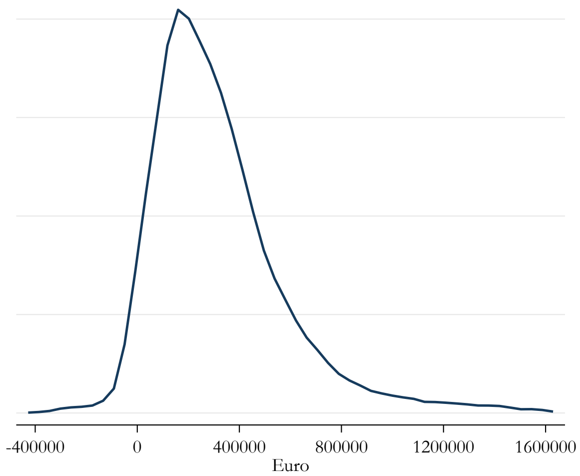
We start out by comparing the indicated perceived graduation wage to the perceived university-leaving wage at the time of labor market entry. The top panel of figure 1.1 plots the density of these two measures. In addition to substantial variation in expected starting wages between individuals, the graph shows that students expect their leaving wages to be much lower than their graduation wages. On average, students expect €27,400 of yearly earnings when leaving university instead of €38,000 when graduating, with the averages being weighted by major and gender. The perceived graduation wage average fits well with the observed labor market entry wage for university graduates, which in 2014 amounted to €36,600 (Destatis, 2017). Furthermore, the patterns of earnings expectations between university majors and gender are plausible, with on average higher expected earnings for males and STEM majors (see appendix figure 1.E4). Estimates of future earnings are also fairly accurate, as observed yearly earnings at age 60 after obtaining a university degree were €60,700 in 2014, compared to €69,200 in our sample (Destatis, 2017). These long term expectations are reasonable despite a 15% higher expected wage compared to current observed wages given that a rising skill premia will likely lead to higher wages among future cohorts of experienced workers with university degree (the ones in our sample), as compared to current ones.[13]

[13]We cannot compare the expected leaving wages to observed values, as any observed measure would be heavily influenced by selection.

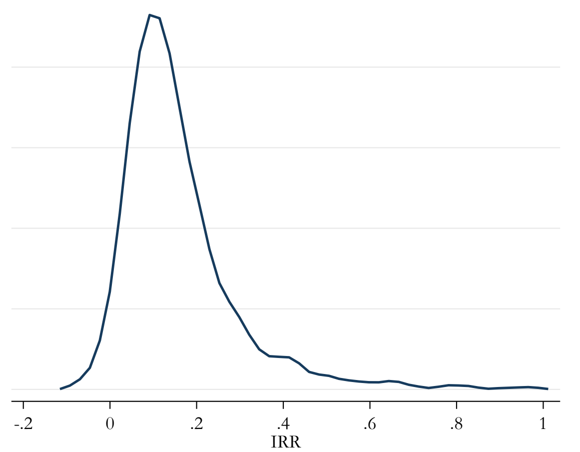
Figure 1.1: Density of starting wage and returns



(a) Expected starting wage by scenario



(b) Life time wage returns



(c) Internal rate of return

Notes: Figure 1.1 panel A shows the density of the expected wage at labor market entry for graduating and leaving university without a degree. Panel B shows the density of the lifetime wage returns, which are calculated according to equation (1.2). Finally, panel C portrays the density of the internal rate of return, as estimated in equation (1.3).

We proceed by computing lifetime earnings return, that is, the discounted sum of wage income after graduating minus wages earned when leaving minus potential study costs. Furthermore, we calculate the internal rate of return, namely the discount rate that would make an individual indifferent between finishing and leaving university. We thus solve the following two equations:

$$V_i^* = \sum_{t=t_i^f}^{65} \delta^{t-t_i^f} W_i^f(t) - \sum_{t=t_i^l}^{65} \delta^{t-t_i^l} W_i^l(t) - \sum_{t=t_i^l}^{t_i^f} \delta^{t-t_i^l} C_i \tag{1.2}$$

$$\sum_{t=t_i^l}^{65} \frac{W_i^f(t) - W_i^l(t)}{(1+\rho)^{(t-t_i^l)}} = \sum_{t=t_i^l}^{t_i^f} \frac{C_i}{(1+\rho)^{(t-t_i^l)}} \tag{1.3}$$

where V_i^* are the lifetime returns for individual i and $W_i^f(t)$ and $W_i^l(t)$ indicate expected wages after finishing studies (f) and leaving (l) at time t. Accordingly, t_i^f and t_i^l is the age at which an individual i is expected to start working when she finishes studying or leaves university. C_i are the yearly study costs an individual incurs, and they are assumed to stay constant over time. Study costs include only explicit costs such as tuition fees, spending for books or other materials needed and were elicited in the survey. Furthermore, in equation (2), δ is the time discount rate, which is set at 0.95. We also calculated the returns for $\delta = 1$ to estimate an upper bound for lifetime returns. In equation (3), ρ is the internal rate of return. An individual chooses to obtain a higher education degree if $V_i^* > 0$ or $\rho > 0$.

The density graphs of the return measures can be found in panels B and C of figure 1.1. Panel B shows that almost all respondents in our sample expect positive discounted lifetime earnings returns from graduating, with the average being around €334,400 until retirement.[14] Panel C shows a similar pattern for the estimated internal rate of return (IRR), since only 3.2 percent of all respondents expect a negative return and the average rate of return is 17.9%. Accordingly, if students in our sample face the decision whether to complete their current degree or leave university without graduating, they on average expect to encounter a 17.9% return to *completing* their studies. This percentage is substantially higher than the IRRs generally reported in the literature for the *initial* choice of starting a university study or not, e.g., the observed initial IRR within Germany in 2014 is 7.5% (OECD, 2014). First, this discrepancy is partly driven by the fact that the students in our sample have self-selected into university. Second, we observe the IRR for *completing* a degree that individuals are currently pursuing, hence students have already paid some of the direct and indirect costs of studying. It is worth mentioning that the discrepancy between initial and "course of study" IRRs points to returns mostly accruing towards the end of one's studies, while the costs are borne at the beginning.

[14] If we calculate the upper bound for the lifetime returns, setting the discount rate $\delta = 1$, the average expected return increases to €792,200.

Therefore, we also look at the IRR of students who only recently started studying. For students in their first or second semester we find an IRR of 11.4%, which comes close to the observed initial IRR.

1.3.2 Perceived Non-wage Returns

Along with the wage returns of finishing a university degree, expected non-wage returns are an important labor market outcome for students (e.g. Wiswall and Zafar, 2016). Figure 1.2 shows the expected job satisfaction and the job-finding probability when finishing and leaving university. Similar to the expected wage returns of graduating, students expect large non-wage returns to a university degree. Panel A displays substantial differences in the distribution of job satisfaction between the two scenarios. While the mean expected job satisfaction is 7.2 out of 10 for graduating, it is only 4.0 for leaving university. The density of the expected job-finding probability by the age of 40 for each scenario is displayed in panel B of figure 1.2. We look at the expected job-finding rate at the age of 40 instead of at labor market entry to prevent the results from being driven by the fact that many first-time employees need some time to initially find a suitable job.[15] The expected return to graduation is substantial, with a mean expected probability of finding a suitable job after graduating of 81.9% compared to 56.7% after leaving university.

1.3.3 Origins Of Returns

To gain a first insight into the perceived origins of the returns, we show descriptive evidence on the immediate graduation premium, as well as the development of expected returns after leaving university over the course of studying.

With respect to the graduation premium, we are interested in the impact of obtaining a degree certificate on students' wage expectations. For this purpose, we compare perceived starting wages after graduating to perceived starting wages when leaving university for master students who indicate being in either their last or second-last semester before finishing their studies. Restricting the descriptive comparison to a sample of students who have almost completed their degree minimizes the chance that the difference in returns over scenarios is (mainly) driven by accumulating human capital during one's studies.[16] Moreover, as we compare the wage expectations within an individual across the two scenarios, this comparison is free from selection bias. Panel A of figure 1.3 shows that there is a substantial difference between the average expected leaving wage and the average expected graduation wage for students in their last semester. The

[15] The results for the job-finding probability at labor market entry are qualitatively similar, and can be found in figure 1.E5 in the appendix.

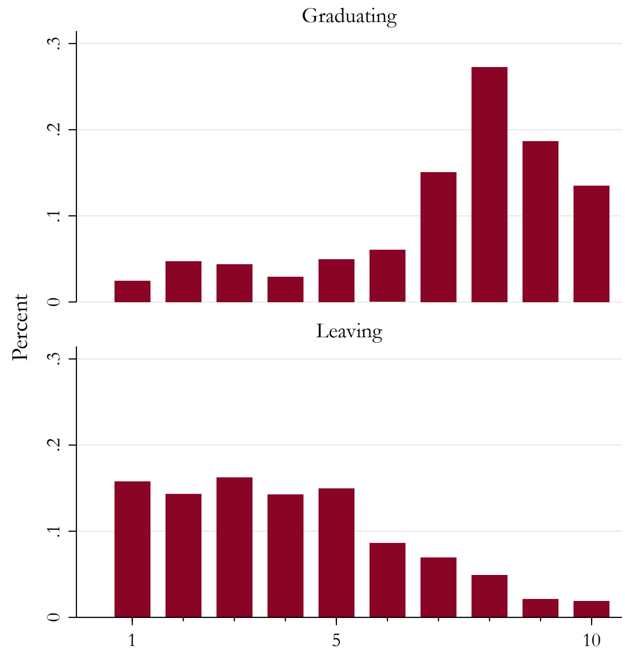
[16] Besides, we focus on master students as they obtain an additional degree, which is different from obtaining a first academic degree, as is the case for most bachelor students. See appendix 1.D for a more extensive explanation.

expected premium to graduation is 24.5%, which corresponds to €7,400 yearly gross income (€37,600 versus €30,200). This is a sizable difference, especially considering that we are only looking at master students, i.e. those who have already completed a first university degree.

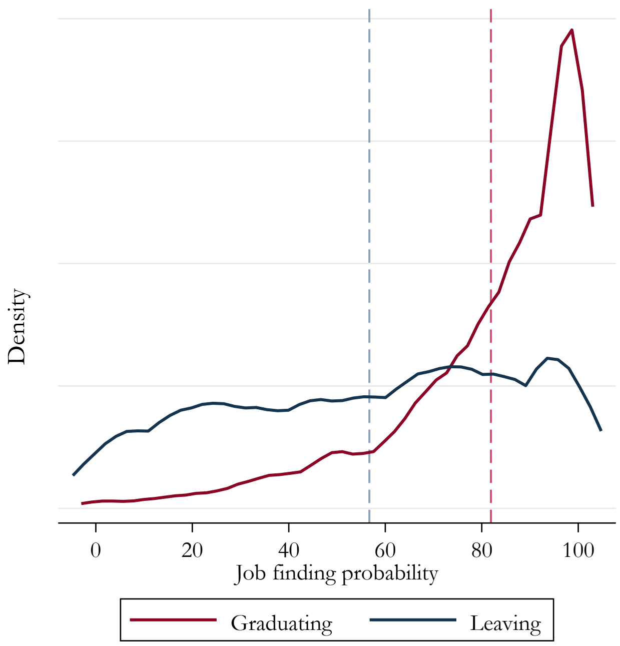
In addition, we look at how the perceived returns when leaving university without a degree evolve over the course of studying, which can be interpreted as an indication of the expected accumulation of human capital. For the following comparison (and for our estimations in section 1.4), we assume that a higher number of semesters studied is associated with a higher human capital value.[17] Panel B of figure 1.3 shows the perceived starting wage after leaving by number of semester studied for master students. As we compare expected leaving wages between individuals over different semesters, we control for background characteristics such as gender, major and age. According to the human capital theory, we should see an upward trend in expected leaving wages, as more productive human capital is accumulated over the course of studying, giving rise to higher expected wages when leaving university. However, we do not observe a conclusive pattern. Wages slightly increase between students who are in their first year compared to students in their second year of master studies by around €1,400, but the difference is not statistically significant. We do not observe any difference in expected leaving wages between students in their second and third year. Moreover, the magnitude of the effect is much less substantial than the premium of obtaining the degree.

[17] This assumption is credible as in general every semester studied involves coursework, mandatory internships, writing a thesis or the like. However, there might be some students who obtain fewer or no credits in a given semester. One can imagine that an extension in study time often comes due to stays abroad, (voluntary) internships or side jobs, which can also be seen as enhancing human capital. Thus, one more semester studied should be associated with a higher or at least similar human capital compared to the previous semester, even if students take more time to study than the regular study time.

Figure 1.2: Density of job satisfaction and expected probability to find a suitable job



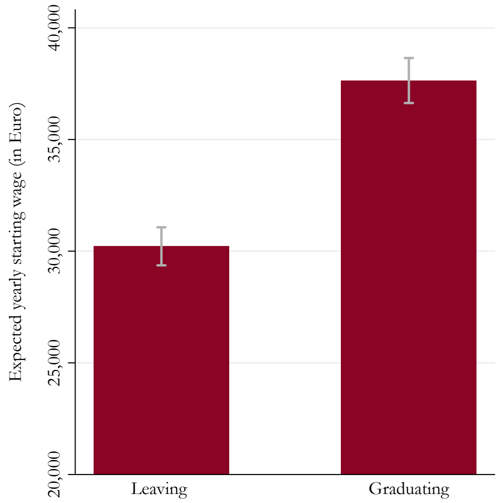
(a) Expected job satisfaction by scenario



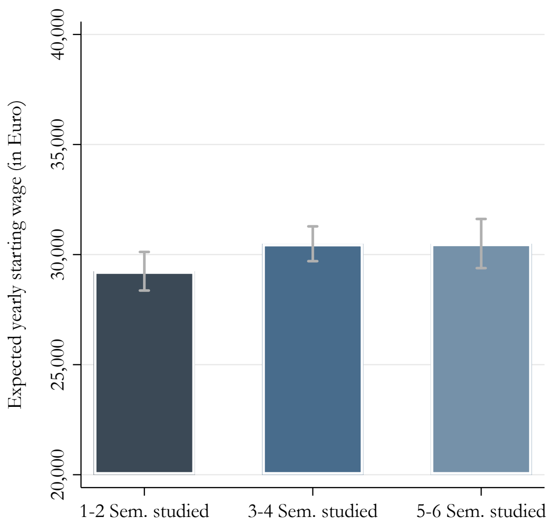
(b) Expected prob. to find a suitable job at age 40 by scenario

Notes: Figure 1.2 panel A shows the distribution of expected job satisfaction at labor market entry for the scenarios of graduating and leaving university, measured on a scale from 1 to 10. The second panel displays the density of the expected probability of finding a suitable job at the age of 40 for both scenarios. The average expected job-finding probability at the age of 40 is 81.9% for graduating and 56.7% for leaving university without a degree (dashed lines).

Figure 1.3: Graduation premium among students in their final semesters compared to development of university-leave wages



(a) Graduation premium



(b) Leaving wages by semester studied

Notes: The top panel of figure 1.3 shows the expected yearly starting wage for leaving university compared to graduating on a within-individual basis. It includes only master students who are in their (second to) last semester. The bottom panel compares the expected yearly starting wage for master students at different stages of their studies. As the comparison is between individuals, we control for gender, age, ability, SES, major and perceived work ability.

1.4 Perceived Signaling Value Of Higher Education

The descriptive findings strongly suggest that students expect substantial labor market returns from finishing their studies, which seems to be largely driven by a graduation premium. In this section, we estimate the perceived signaling effect of a degree and proxy the value of human capital accumulation more precisely on hands of our unique individual counterfactual expectations data.

1.4.1 Immediate Wage Returns

Our strategy of eliciting counterfactuals through carefully-designed survey questions allows us to estimate the effect of obtaining a degree on a within-person basis, i.e. without having to worry about other confounding factors. A growing body of literature relying on hypothetical scenarios, beliefs, and counterfactual labor expectations has shown that stated expectations and preferences tend to be close to actual realizations and informative about actual choices and behavior (see e.g. Wiswall and Zafar 2016, Mas and Pallais 2017). Yet, even if elicited labor market expectations were biased, they are nevertheless informative about beliefs that enter the individual decision-making process. Nonetheless, the considerable average accuracy of wage expectations at labor market entry portrayed in section 1.3 allows us to extend the interpretation of the following signaling results more generally.

Using the counterfactuals, we can identify the effect of a degree by comparing the two different scenarios on a within-person basis, eliminating the individual fixed effect. Additionally, we approximate the human capital effect by comparing leaving wages between individuals who are in different semesters of their studies and assume that human capital accumulates linearly over time.[18] As the signal is most prevalent at labor market entry, we first concentrate on the immediate returns from graduating, but we will also look at the long-term development of the graduation premium in section 1.4.3. Accordingly, equation 1.4 shows our main specification for immediate returns:

$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_2 semesters_i^c + \gamma_i + \epsilon_i \tag{1.4}$$

W_i^c represents the expected yearly starting wage of individual i in scenario c, with $c = f$ for graduating and $c = l$ for leaving. In this equation, as well as in equations 1.5 to 1.7, all expectations variables used are about the time of labor market entry, and hence W_i^c stands for $W_i^c(start)$, with $t = start$ indicating the time at which individual i starts working. Moreover, $degree_i^c$ is a dummy variable indicating the graduation wage, which is one for the scenario of obtaining a degree and zero for leaving without a degree. $semesters_i^c$ indicates how many more semesters an individual still has to study to finish

[18] We restrict the sample to students who indicate having at most eight semesters left to study, changing the sample size to 3,945 and 1,284 for bachelor and master students, respectively. This does not affect our main results (see section 1.4.4).

their degree, which is zero in the scenario of graduating.[19] The individual fixed effects are captured by γ_i, which controls for an individual's scenario invariant characteristics and ϵ_i is the error term clustered on individual level. Hence, β_1 measures the value of the degree certificate, while β_2 captures the expected wage premium for getting one semester closer to the degree.

The interpretation of the above analysis rests on the assumption that graduating results in a positive signaling value. However, it is conceivable that leaving university without a degree yields a negative signal instead. In this case, the absolute size of the signaling value that we estimate would be unaffected, but its interpretation would change. We provide a detailed account of this possibility in appendix section 1.C. Throughout the paper, we stick to the interpretation of a positive signaling value for obtaining the degree certificate, as this is most in line with the existing literature.[20] Under this assumption, β_1 can be interpreted as the (positive) signaling effect of a degree and β_2 can be interpreted as the human capital value per additional semester studied.

We estimate equation 1.4 separately for bachelor and master students and focus on master students throughout the main analysis, as they face less ambiguity with respect to both their own ability (Stinebrickner and Stinebrickner, 2012; Arcidiacono et al., 2016) and potential labor market outcomes (see appendix 1.D for an extensive discussion). Table 1.2 shows our main results with expected starting wages as the outcome variable for master students. In column 1, we estimate the model for wage levels, whereas the other columns use log wages as the outcome variable. The first coefficient estimated in column 1 indicates that the effect of coming one semester closer to graduating is positive but small, with roughly a €210 increase in expected yearly starting wages on average. By contrast, graduating is expected to increase returns by €7,100. Column 2 shows that this translates into a wage increase of 0.68% for an additional semester studied and 20.9% for the degree respectively. The size of the expected signaling effect is notable, especially since we only consider the returns to a master's degree, such that leaving still means being able to start working with a bachelor's degree.

Arguably, for certain (often high-paid) professions, the returns from graduating might be driven by legally-binding requirements to obtain a certain degree certificate in order to take up a specific employment. Licensing may thus capture something very distinct from future productivity. Therefore, in column 3 we include two interaction terms: first, a dummy indicating whether an individual plans to work in a legally-regulated occupation; and second, a dummy indicating whether a person plans to work as a civil servant. In Germany, many positions as a civil servant also require a completed degree

[19] To make the estimates more comprehensive, we used a negative sign on the semester variable such that a higher (less negative) semester variable means getting closer to the degree. Of course, the coefficients are unaffected by this manipulation, whereby only the sign is positive instead of negative.

[20] We believe that this is also more plausible since labor market applicants have some leeway in informing future employers about (the reasons for) leaving university without a degree. Of course, this might not always be possible, as it depends among others on the time studied, although very often applicants only include accomplishments and positive signals in their application and not failures.

Table 1.2: Wage returns

	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs
Semesters	212.277	0.007	0.007
	(157.298)	(0.004)	(0.004)
Degree	7,099.660***	0.209***	0.203***
	(549.446)	(0.015)	(0.016)
Interaction effects:			
Licence*Degree			0.029*
			(0.016)
Civil servant*Degree			0.003
			(0.018)
Constant	30,639.636***	10.287***	10.288***
	(520.848)	(0.014)	(0.014)
N	2762	2762	2754
adj. R^2	0.461	0.506	0.507

Notes: Column 1 in table 1.2 shows the effects on the level of yearly starting wages, while the dependent variable in columns 2 and 3 comprises of the log starting wage. The sample only includes master students who have maximum of eight semesters left until reaching their degree. Standard errors in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

and the earnings are predefined by a collective wage structure depending among others on the highest degree obtained. The results in column 3 show that the interaction term for licensed professions is positive and marginally statistically significant. Nonetheless, the effect size is relatively small and the magnitude of the signal is almost unaffected by controlling for licensing. At the same time, we do not observe an effect of planning to work as a civil servant. One explanation might be that although having a master's degree allows individuals to earn more when working in a public institution, in general the earnings potential as civil servant tends to be lower compared to the private sector.

In appendix table 1.E1, we present the same estimates for bachelor students. The results show a similar pattern as for the master students, with a positive but small increase of expected earnings over semesters (0.62%), and a large signaling value of graduating (32.1%). It is reasonable that the effect size of graduating is stronger for bachelor students, as graduating yields their first academic degree, possibly allowing them to enter a different segment of the labor market.

1.4.2 Immediate Non-wage Returns

In addition, we estimate the fixed effects model for expected non-wage returns, namely job satisfaction and the probability of finding a suitable job. At present, little is known about the extent to which signaling expands to non-wage returns. There are two possible scenarios. First, if wage and non-wage returns are positively correlated, we would expect to see a positive signaling value for both the perceived job-finding probability and job satisfaction. Instead, if they are negatively correlated – for example, due to compensating wage differentials (Rosen, 1974) – we would expect to see opposite or non-significant results. For the estimation of the fixed effects model, we standardize both variables across scenarios, using the value in the leaving scenario as the baseline to adjust both leaving and graduating values:

$$S_i^c = \frac{sat_i^c - \mu_{sat}^l}{\sigma_{sat}^l} \tag{1.5}$$

with S_i^c as the standardized outcome variable (in this case job satisfaction). Here, sat_i^c is the expected satisfaction of individual i for scenario c and μ_{sat}^l and σ_{sat}^l are the mean and standard deviation of the perceived satisfaction when leaving university.

Table 1.3 shows the results for the expected non-wage returns, where the first two columns examine satisfaction at labor market entry and the last two relate to the job-finding probability. For both measures, we observe similar patterns compared to wage returns. There is a large perceived graduation premium, which is statistically significant across all specifications. We observe that the degree raises expected satisfaction by 1.04 of a standard deviation, and expected job-finding probability by 0.46 of a standard deviation. At the same time, the expected human capital effect is not statistically significant, although the signs of the effects are as expected and consistent with our previous findings. Moreover, planning to enter a licensed occupation after graduation does not significantly affect expected job satisfaction. However, for the expected suitable job-finding probability licensing or becoming a civil servant substantially increases the probability. In appendix table 1.E2, we present the findings for the non-wage returns of bachelor students. These results are similar to our main findings, where graduation yields even stronger effects, i.e., approximately a 1.5 standard deviation increase in job satisfaction, and a 0.8 standard deviation increase in the job-finding probability.

1.4.3 Persistence Of The Graduation Premium

So far, our results suggest that students perceive the immediate returns from graduating to stem from signaling their ability to employers in the labor market rather than from accumulating human capital. However, in the longer run this might be different, as individuals can demonstrate their abilities and reveal their true productivity types to employers while working. As a consequence, the initial advantage of the signal might

Table 1.3: Non-wage returns

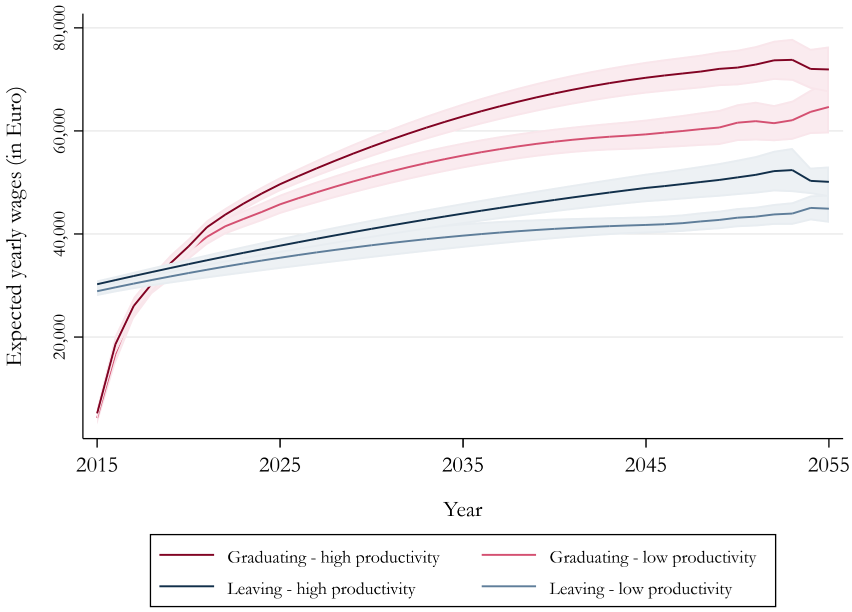
	(1) Satisfaction	(2) Satisfaction	(3) Job finding probability	(4) Job finding probability
Semesters	0.020	0.022	0.008	0.009
	(0.026)	(0.026)	(0.018)	(0.018)
Degree	1.091***	1.039***	0.519***	0.461***
	(0.093)	(0.095)	(0.066)	(0.067)
Interaction effects:				
Licence*Degree		0.153		0.125*
		(0.093)		(0.069)
Civil servant*Degree		0.087		0.148**
		(0.108)		(0.074)
Constant	0.067	0.073	0.025	0.032
	(0.088)	(0.088)	(0.062)	(0.062)
N	2762	2754	2762	2754
adj. R^2	0.424	0.424	0.240	0.243

Notes: Columns 1 and 2 in table 1.3 show the effects on expected job satisfaction at labor market entry, while the dependent variable in columns 3 and 4 is the expected probability of finding a suitable job. Both satisfaction and job-finding probability are expressed in standard deviations according to equation (1.5). The sample only includes master students who have maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

diminish over the working life. As we collected data on the expected wage returns for three points in time and computed wage expectations over the whole life span for both scenarios accordingly, we are able to examine how the initial difference between graduates and university leavers evolves over the course of career. In addition, we can investigate heterogeneities in the long term development by perceived work ability to assess the degree of perceived employer learning (see e.g. Farber and Gibbons, 1996; Lange and Topel, 2006; Aryal et al., 2022, for a discussion and evidence regarding actual wage outcomes) and the extent to which it may outweigh the signaling effect in the long run.

Figure 1.4 displays the development of expected wages after graduating (red lines) and after leaving university without a degree (blue lines), where the darker (top) lines of each color resemble the upper 50% of the perceived work ability distribution and the lighter (bottom) lines resemble the bottom 50% of perceived work ability. The colored areas around the lines indicate the 95% confidence intervals. We use the indicated perceived work ability of each individual as a proxy for later (perceived) productivity in the labor market.[21]

Figure 1.4: Expected yearly wages over the life time by perceived productivity



Notes: Figure 1.4 shows the development of average expected yearly wages for master students who do not plan to work in a legally-licensed occupation in accordance with equation 1.1. The red lines correspond to graduating, and the blue lines to leaving university without a degree. The darker (top) lines of each color correspond to the upper 50% of the perceived work ability distribution and the lighter (bottom) lines correspond to the bottom 50% of the perceived work ability distribution. Colored areas indicate the 95% confidence intervals.

From figure 1.4 we can derive several conclusions about the persistence of graduation premia, employer learning, and long-run expected wage dynamics. First, graduation premia matter in the long-run, independent of productivity type, as students expect to earn more in absolute terms at every point in time as graduates than as university leavers.[22] In fact, from all master students only 8.9 percent expect to be able to diminish part of the wage gap between the graduating and leaving scenario at some point in their career. Moreover, merely 4.2 percent of master students belief they can fully close the gap, mostly towards the end of their careers (see appendix figure 1.E6). For bachelor students these percentages are even lower, with 6.5 and 2.6 percent respectively. Second, figure 1.4 provides evidence consistent with employer learning. At the start of working life, there is only little difference between high- and low-productivity types in both scenarios, which supports the main result of our paper, namely that students expect a signaling effect to drive the initial returns of graduating. When a degree is mainly a way

[21]We focus on perceived work ability as a proxy for productivity instead of other ability measures for several reasons. First, students' GPA may be poorly comparable across majors and institutions. Second, high (perceived) academic ability does not necessarily translate into high (perceived) work ability. In our sample of students, the correlation between perceived academic and work ability is merely 0.41.

[22]In the first few years the average earnings are mechanically lower for the graduating scenario, as it takes time until all individuals have entered the labor market after finishing their degree.

to signal one's type, productivity is initially unobserved by employers. Moreover, as the signal should have the same value to everyone who obtains it, returns should be similar for all productivity types at the start of career. Then, as employers learn more about individual ability, the difference between the low- and high-ability employees within both scenarios increases. Similarly, a comparison of expected wage dynamics before and after the age of 40 displayed in table 1.E3, reveals that the coefficient on the degree signal decreases with experience at later stages of career, while the one on productivity, stays almost constant with increasing experience. This pattern has been found repeatedly in actual wage data (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Lange and Topel, 2006). It suggests that the relative importance of easily observable characteristics (like a degree) decreases with experience while that of employee productivity becomes relatively more important over the working life. Third, as regards relative wage dynamics over time, figure 1.4 unveils a lot of growth after graduating at first, i.e., when productivity is not fully revealed yet. Large perceived initial returns to experience among university graduates lead to rapidly increasing gaps between scenarios in the beginning, which then only partly close at later stages when productivity becomes relatively more important than the degree. Moreover, when including an interaction term between productivity and the signal in the estimations of table 1.E3, we find that perceived productivity and degree completion are complements when it comes to wage returns. That is, high productivity individuals seem to expect larger returns to experience after graduation, possibly because the jobs they expect to pursue with a degree require tasks that more closely match their abilities.

There are several potential reasons for the low support for diminishing initial graduation premia. One explanation is that graduating not only leads to higher perceived lifetime returns through increased starting wages, but that it also helps job beginners to get into different kinds of jobs compared to university leavers. Moreover, they may believe that initial assignment to a high-earning job allows individuals to acquire specific human capital.[23] These jobs might then have stronger potential for wage increases over time. Nonetheless, we need to recall that both our main results and figure 1.4 (as well as appendix figure 1.E3) only refer to master students who already have a university degree even in the scenario of quitting their current studies. Hence, it is not quite straightforward to expect that students with only a bachelor degree perform substantially different jobs compared to master students. Although the mechanisms behind this result are not completely evident, we can conclude that the initial expected graduation premium caused by the signaling value is not only lasting but even growing over time and that it outweighs perceived employer learning in the long run.

[23]The same effect could arise from productivity spillovers from high-performing co-workers or if the signal grants advantages in promotions, e.g., because early earnings are a signal for later earnings (see e.g. Waldman, 2016).

1.4.4 Robustness Checks

In this subsection, we assess the robustness of our results. For this purpose, we first relax the linearity and homogeneity assumptions that we made to estimate the human capital effect. We then study potential biases that may arise from dynamic selection related to student dropout over time. Finally, we assess the sensitivity of our results with respect to sample selection.

Linearity of human capital accumulation. First, we assume that the human capital effect is linear in semesters. This is a reasonable assumption as credit points at university normally build up linearly with an increasing number of semesters completed. However, from an individual perspective this does not always hold true. Besides, some courses or activities might be perceived as creating more human capital than others. Therefore, we estimate an alternative fixed effects specification easing the assumption that human capital accumulation is a linear process by looking at the effect of each semester separately. Equation 1.6 shows the respective specification:

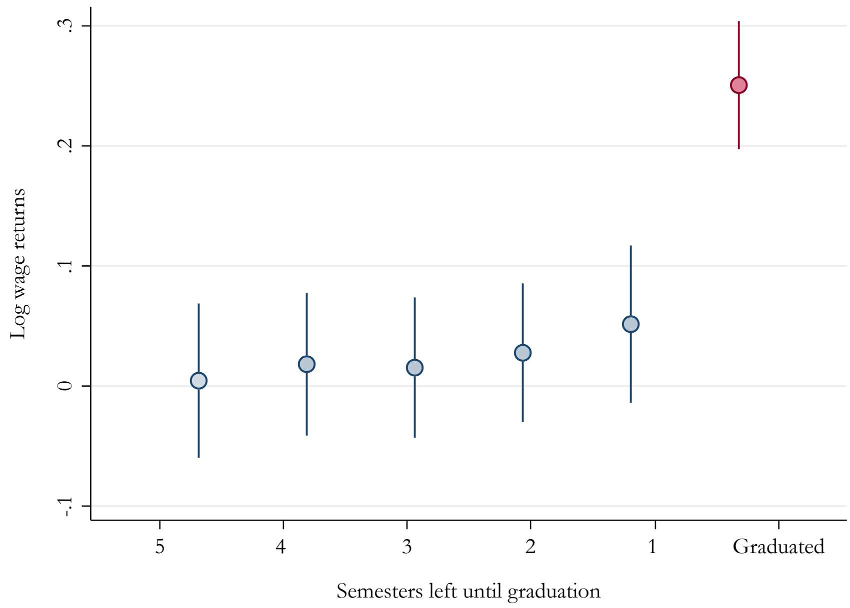
$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_n \mathbb{1}_{n,i}^c + \gamma_i + \epsilon_i, \tag{1.6}$$

where $\mathbb{1}_{n,i}^c$ is an indicator function representing a set of dummy variables for the number of semesters n that individual i still needs to study. The baseline is having 6, 7 or 8 semesters more to study, as we bundled the "high semester" students in one category due to the small number of observations.

Figure 1.5 visualizes the results of the fixed effects model with semester dummies and displays the estimated coefficients with 95% confidence intervals.[24] The coefficients indicate how expected starting wages after leaving change compared to the baseline of having 6 to 8 semesters left to study. It seems that the development over semesters is slightly increasing, although in line with the model estimated in section 1.4.1 none of the coefficients are significantly different from zero and we do not see any non-linearities. The graph shows that graduating with a master degree causes a considerable jump in expected wages of 25.1% compared to the baseline, which is in line with the estimated effect of a degree of 20.6% in our main model specification. As before, this is a substantially stronger effect compared to the value of an additional semester studied. Note, however, that in the current specification the human capital value of the final semester studied is included in the value of obtaining the degree in contrast to our main specification in section 1.4.1. Nevertheless, to explain the full degree effect solely by human capital accumulated in the final semester, it would need to be five times more valuable than the human capital accumulated in any of the other semesters during studies. Given the equal weight of each semester in terms of credit points, and the fact that the final semester for the majority of studies comprises of writing a thesis, this is a highly unlikely scenario.

[24]See appendix table 1.E4 for the regression results.

Figure 1.5: Plotted coefficients of fixed effect model with semester dummies



Notes: Figure 1.5 displays the coefficients and 95% confidence intervals from estimating equation (1.6), where the blue dots correspond to β_n and the red dot to β_1. The baseline is having six or more semesters until graduation. The regression only includes master students who have a maximum of eight semesters left to study.

Increasing human capital by semesters. A second assumption, that we make to approximate the human capital effect, is that with fewer semesters left to study the human capital value should increase. Although this is straightforward at an individual level, it might not always hold true when comparing between individuals, because students who have the same number of semesters left to study are not necessarily at the exact same stage of their studies. We test this assumption by restricting the sample to students who are studying in regular study time, meaning that the sum of semesters left to study and semesters already studied cannot exceed the regular study time plus one. Fixing the sum of these two variables ensures strong comparability of semesters between students as they are all participating in a master's program that they are about to finish in regular study time. In table 1.4, columns 1 to 3, we show that the estimated effect of obtaining a degree slightly decreases but remains at a significant 18.7% wage increase (compared to 20.6%). The estimated human capital effect remains statistically insignificant. Overall, our estimation of the signaling effect is robust to this subsample analysis.

Dynamic selection. Third, so far we have abstracted from dynamic selection. Although we have students at all study stages in our sample, the students in the later semesters of their studies might be a selected sample as they have already reached a later stage of studying. At the same time, students with a higher expected graduation premium might be less likely to leave university than students with lower expected returns of graduating, in which case we might overestimate the signaling value due to dynamic selection. To test whether our results are affected by dynamic selection, we estimate the signaling effect only for students who finished high school with an average grade in the top third of our sample. According to Isphording and Wozny (2018), a better high-school grade is highly predictive of graduating within Germany. Hence, if we restrict our analysis to the top performers in high school, this should reduce potential dynamic selection substantially, while also improving comparability between students across different study stages. Columns 4 to 6 of table 1.4 present the estimates for this sample. We observe a signaling effect of roughly 18%, which is close to the results in our main analysis. The human capital effect turns statistically significant and increases slightly compared to our main analysis, indicating that high performers benefit relatively more from education as regards their human capital accumulation. However, with a 1.5% wage return per semester it remains considerably lower than the effect of the degree. As an additional test for dynamic selection, we compare the distribution of the graduation entry wage by semesters left in appendix figure 1.E7. If our sample suffers from dynamic selection, we expect to see a higher perceived graduation wage for students who have fewer semesters left. From visual inspection of the figure no significant differences are observed, which is confirmed by Kolmogorov-Smirnov tests.[25]

Sensitivity with respect to sample selection. Finally, in our main analysis we restrict the sample to students who indicate having at most eight semesters left to study.[26] To test the sensitivity of our findings with respect to the exact thresholds of semesters, columns 7 to 9 in table 1.4 in the appendix show the results for a sample including students who report having up to 12 semesters left to study (capturing more than 99% of all students). The results show that the magnitude of the graduation premium is robust to expanding the sample to these students.

Overall, we can conclude that for master students the expected signaling effect is substantial and robust across all specifications. Throughout, the human capital value remains positive but small. Moreover, the relative importance of human capital to the signaling effect remains minor. For bachelor students, we repeat all robustness checks and find that the signaling value also remains robust across specifications (see table 1.E5 in the appendix).

[25] Further evidence that speaks against a dynamic selection problem is presented in section 1.5.2, where we show that the graduation premium does not predict the likelihood of dropout.

[26] As the regular study time for master students is four semester in Germany, we restricted the sample to double the regular amount of time needed for studying.

Table 1.4: Robustness checks

	Regular study time			Best third in high-school			Max 12 semesters		
	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs	(4) Starting wage levels	(5) Starting wage logs	(6) Starting wage logs	(7) Starting wage levels	(8) Starting wage logs	(9) Starting wage logs
Semesters	124.398	0.012	0.012	493.765*	0.015**	0.014**	180.957	0.006	0.005
	(298.834)	(0.008)	(0.008)	(253.070)	(0.007)	(0.007)	(133.325)	(0.004)	(0.004)
Degree	7,229.140***	0.191***	0.188***	5,924.103***	0.180***	0.189***	7,191.741***	0.212***	0.205***
	(988.126)	(0.025)	(0.025)	(828.544)	(0.024)	(0.024)	(493.595)	(0.014)	(0.014)
Interaction effects:									
Licence*Degree			0.010			0.001			0.032**
			(0.021)			(0.026)			(0.016)
Civil servant*Degree			0.006			-0.054*			0.007
			(0.024)			(0.030)			(0.018)
Constant	30,675.476***	10.312***	10.312***	32,090.417***	10.322***	10.321***	30,530.587***	10.283***	10.284***
	(947.072)	(0.024)	(0.024)	(810.200)	(0.023)	(0.023)	(463.361)	(0.013)	(0.013)
N	1376	1376	1372	1046	1046	1042	2822	2822	2812
adj. R^2	0.455	0.522	0.522	0.436	0.499	0.501	0.459	0.503	0.504

Notes: Table 1.4 shows the outcomes of the robustness analysis. Columns 1-3 comprise students who are expected to finish within regular study time, i.e., four semesters in total. Columns 4-6 include every student who had a high-school GPA in the highest 33%. Column 7-9 includes all students who are in the 12th semester or less. The sample only includes master students. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

1.5 Implications Of The Signaling Theory

The previous sections have shown that students predominantly believe that the signaling value is responsible for the largest part of the returns to graduating. A natural next step is to check whether further implications of the signaling theory also hold in our sample. Regarding our analysis, there are two testable implications of Spence's signaling theory. First, as the degree is assumed to be the only way to signal productivity in the labor market, the short-term returns should be the same for everyone who obtained the signal, independent of unobservable skills or background characteristics. Second, as the immediate returns from graduating should not differ between individuals, the decision to leave university should be mostly driven by the (psychic) cost of education, rather than the potential earnings after finishing.

1.5.1 Heterogeneities In Signaling

A key assumption of the signaling model is that an individual's productivity type is not directly observable and that employers therefore use the signal to infer an individual's productivity. If a degree is no more than a way of signaling (future) productivity, then the expected returns should ideally apply to everybody who obtains that signal, and the signaling value should not vary between individuals with the same observable (but different unobservable) characteristics.

However, Spence's signaling theory does not account for labor market discrimination. Some background characteristics are usually observable in the application process and labor market discrimination with respect to wage or other labor market outcomes is a widely-documented phenomenon in Western labor markets. Hence, one could expect to observe heterogeneities in the signaling value concerning characteristics that are subject to discrimination, such as gender (see Belman and Heywood, 1991, for earlier evidence on heterogeneities in signaling values for women and minorities).

Moreover, the model of Spence abstracted from the fact that various educational degrees exist, e.g., graduating from different fields or majors. These degrees can be interpreted as distinct signals, which are valued differently in the labor market. Hereby, each type of degree may signal different underlying unobservable characteristics, such as stamina, on-the-job productivity, or creativity.

To test whether there are heterogeneous signaling values, we include interaction terms between the degree dummy and various background variables in our fixed effects model. We estimate the following equation:

$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_2 semesters_i^c + \beta_3 (degree_i^c * X_i) + \gamma_i + \epsilon_i, \qquad (1.7)$$

where X_i is a set of background characteristics comprising gender, socioeconomic background, study characteristics and perceived relative job ability, to test whether these characteristics matter for the value of the degree signal in the labor market.

Table 1.5 displays the regression results. Overall, it seems that the expected returns from the degree do not strongly depend on individual skills or background characteristics, with two main exceptions: gender and major. The interaction term with the gender dummy shows a statistically significant positive effect for males, where the expected signaling value is roughly three percentage points higher for males than for females when controlling for major and other background characteristics (see column 5). The existence of gender discrimination in the labor market is an intuitive explanation for this finding. In addition, the interaction terms with the major categories (humanities/social sciences, medicine, STEM, law and economics/business) are statistically significant. With the humanities/social sciences major as a baseline, we observe a higher signaling value for medicine and STEM majors. As explained before, this result is reasonable as graduating in a different major can be interpreted as acquiring a different signal.

Simultaneously, the two characteristics associated with socioeconomic status – i.e. the indicators for migration background and having at least one parent with an academic degree – do not appear to affect the value of the signal. As especially parents' educational background is unobserved by potential employers, the lack of a significant interaction term is suggestive evidence of the signaling theory, which states that the signal should be independent of unobservable characteristics. The same holds true for perceived work ability. Table 1.5 indeed presents evidence that the perceived work ability of students has no effect on the value of the expected signal. At the same time, section 1.4.3 shows that students expect their work ability to yield wage returns in the long run. Therefore, the fact that the perceived job ability does not have an effect on the *immediate* returns of graduation further supports the signaling interpretation. Regarding GPA, the absence of a significant interaction effect may be more surprising, since this is an aspect that is observable by employers. However, it is important to realize that our findings do not exclude the possibility that GPA is a signal in and of itself. This is, as in our model the general effect of the GPA on the expected wage is captured by the individual fixed effects. Our results merely imply that the signaling value of a degree is independent from a student's GPA, which is again in line with Spence' argumentation.[27]

1.5.2 Determinants Of Leaving

The second implication from the signaling theory relates to students' decision whether or not to complete tertiary education. As the returns from graduating should not substantially differ between individuals sending the same signal, the decision to select out of education should be driven by the (psychic) cost of education only, and not by the

[27] In appendix table 1.E6, we show the same results for bachelor students. The findings with respect to gender and majors are similar to those for master students. We discuss this finding in detail in appendix section 1.D. In addition, we estimate equation 1.7 for job satisfaction and job-finding probability. For these outcomes only the study major plays a role. The results are available upon request.

Table 1.5: Wage returns - heterogeneities

	Starting wage (logs)				
	(1)	(2)	(3)	(4)	(5)
Degree	0.188***	0.204***	0.202***	0.219***	0.204***
	(0.016)	(0.017)	(0.017)	(0.045)	(0.045)
Semesters	0.006	0.006	0.007	0.005	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Interaction effects:					
Male*Degree	0.044***				0.029**
	(0.013)				(0.013)
Academic*Degree		-0.007			-0.006
		(0.012)			(0.012)
Migrat*Degree		0.029			0.028
		(0.018)			(0.018)
Perc. job ability*Degree			0.001		-0.001
			(0.012)		(0.012)
Gpa*Degree				-0.008	-0.007
				(0.006)	(0.006)
Majors:					
Medicine*Degree				0.075**	0.077**
				(0.031)	(0.031)
STEM*Degree				0.084***	0.076***
				(0.017)	(0.018)
Law*Degree				0.030	0.028
				(0.069)	(0.067)
Economics*Degree				0.019	0.014
				(0.016)	(0.016)
Constant	10.286***	10.286***	10.288***	10.283***	10.281***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
N	2754	2754	2754	2754	2754
adj. R^2	0.511	0.508	0.507	0.520	0.522

Notes: Table 1.5 includes several interaction terms between the degree premium and background characteristics. The sample only includes master students who have a maximum of eight semesters left until they reach their degree. The regressions are controlled for licensing effects. The baseline subject is humanities. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

potential earnings gain from finishing. Besides testing this implication of Spence's theory in our data, the following analysis is also informative about the determinants of student dropout. This is a relevant issue as our previous analysis has shown that the largest part of the return to studying is associated with graduating, and hence leaving university earlier is perceived to be very costly. Nonetheless, 11% of all master students in Germany leave university without a degree (Heublein et al., 2014).[28]

To test the second hypothesis, we regress the perceived probability of leaving university without a degree on the immediate wage and non-wage returns to graduating, study performance and satisfaction, study costs, and background characteristics. For the wage returns, we compute the absolute difference of expected entry wages between the graduation and leaving scenarios. For the non-wage returns, we use standardized differences of expected immediate returns between scenarios. The results are presented in table 1.6. In columns 1 and 2, we include both wage and non-wage returns and test whether the returns from graduating predict expected leaving probabilities. As we know that the signaling value depends on the chosen major, we additionally control for majors in column 2 to test whether the probability to leave is affected by major-specific wage returns. The table shows that expected wage returns do not seem to affect students' leaving probability. This finding is in line with the hypothesis that wage returns should not matter for deciding whether to obtain the signal, as the returns are the same for everybody who acquires it. For non-wage returns, it is less clear what to expect, as they might not be perfectly correlated with wage returns and, unlike wage returns, they may differ between individuals with the same type of degree. We indeed see that increased job satisfaction and job-finding probability returns reduce the probability of leaving.

Concentrating on the cost-related variables included in column 3, we find additional support for the second hypothesis. Study satisfaction – which is an indicator of the current consumption utility of studying and a proxy of psychic costs – is strongly associated with the probability of leaving university. Being satisfied instead of dissatisfied with one's studies reduces the leaving probability by over five percentage points. In contrast, financial costs, in terms of monthly study expenditures, are not significantly correlated to the dropout probability. This may be explained by the relatively low costs of studying in Germany, as most universities do not charge tuition fees. Further, we include ability measures that can be thought of as being related to effort costs, as a lower academic ability may make studying more difficult. Accordingly, we find that having a higher study GPA reduces the leaving probability.

Taken together, we find support for the second testable implication of the signaling theory. Students seem to mainly base their decision whether or not to leave university at an early stage on (psychic) cost-related factors, while wage returns are not predictive for leaving.

[28]For bachelor students, the observed dropout rate is 28%. These data were collected in Germany and refer to the student cohort graduating in 2012.

Table 1.6: Regression results for probability to leave

	Leaving probability		
	(1)	(2)	(3)
Wage returns (in 1,000 Euro)	-0.025	-0.022	-0.051
	(0.055)	(0.057)	(0.055)
Job satisfaction return	-1.333***	-1.431***	-1.433***
	(0.497)	(0.506)	(0.496)
Job finding prob. return	-1.708***	-1.679***	-1.486***
	(0.576)	(0.571)	(0.569)
Satisfied with studies			-5.377***
			(1.342)
Monthly study costs			-0.002
			(0.004)
Male			0.998
			(0.894)
Academic parent(s)			0.117
			(0.899)
Migration background			2.280
			(1.488)
Study GPA			-1.364***
			(0.408)
High-school GPA			0.202
			(0.288)
Perceived academic ability			-0.026
			(0.027)
Constant	7.452***	9.150***	23.060***
	(0.677)	(2.437)	(4.094)
N	1381	1381	1381
adj. R^2	0.012	0.012	0.041
Controlled for major	No	Yes	Yes
Mean leaving probability	7.75	7.75	7.75

Notes: Table 1.6 regresses the probability of leaving on the expected returns from graduating and several background characteristics. For the wage returns, we computed the absolute difference of expected labor market entry wages between the graduation and leaving scenario. For non-wage returns, we used standardized differences of expected immediate returns between scenarios. The sample only includes master students, who have a maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

1.6 Conclusion

While substantial returns to university education have been documented in a large body of empirical literature, the extent to which these returns reflect the signaling rather than the productivity-enhancing human capital effect of education remains open to debate. Based on novel data with measures of counterfactual labor market outcomes for graduating and leaving university without a degree, this paper documents large perceived returns to degree completion. Moreover, estimates from within-person fixed effects models unveil substantial signaling effects of around 20% in terms of starting wages for a master degree, exceeding the human capital effect of education by 3-5 times over the course of studies. Degree effects are persistent in absolute terms, but become less important relative to expected on-the-job productivity in explaining expected wage dynamics over the course of career.

Although in terms of methodology our approach differs from the existing literature, our findings are complementary. First, we provide novel evidence that among current students *perceived* signaling tends be important and highly persistent in terms of lifetime wages. Second, our findings are in line with two predictions from the signaling theory: (i) heterogeneities in perceived signaling – albeit for different fields of study – are relatively unimportant when compared to the overall effect of obtaining a degree, and (ii) when compared to the psychic cost of studying, the graduation premium matters little for the perceived probability of leaving university without a (further) degree. Third, using within-individual variation and information on students' grades we can largely dismiss an alternative (selection) hypothesis that dates back to Chiswick (1973) (see also Lange and Topel, 2006), stating that the graduation premium arises because graduates are disproportionately comprised of individuals whose returns to education are particularly large. If this hypothesis held true, it would be unlikely to observe homogeneously high within-individual returns to degree completion.

Our results hold implications for both understanding students' motivations to study and for economic policy. First, given their expectation of substantive signaling effects, students' main motivation to attend higher educational institutions seems to be to obtain credentials rather than to learn new skills, concepts, and material. Thus, in light of our findings, common complaints among professors regarding their students' limited willingness to study material beyond what is on the exam seem warranted. Moreover, our findings provide a rationale for the sustained demand for enrollment in selective educational institutions, even though many studies find no benefits in terms of learning achievements or actual wages (see e.g. Dale and Krueger, 2002). In terms of policy, the fact that most of the perceived returns to education are private implies that tuition fees should have little effect on student enrollment. Thus, our findings may explain why a temporary introduction of tuition fees in Germany – although contested politically – had only small effects on study take-up (Hübner, 2012). Finally, the finding that perceived returns are unable to predict perceived university-leaving probabilities suggests that policies to fight student dropout should focus on measures that target the psychic costs of studying rather than the perception of future returns for instance.

The paper also opens up several avenues for future research. First, our results only hold for individuals who are currently enrolled at a university or college of applied sciences. In this sense, it would be valuable to extend the analysis to high-school students, e.g., to study the effect of the perceived graduation premium for the extensive margin of student enrollment. Second, it would be interesting to go one step back and analyse how beliefs about the returns to education, especially with respect to signaling versus human capital value, are formed. Third, it would be informative to investigate whether the labor demand side (e.g., human resource managers) holds similar perceptions regarding the relative importance of signaling and human capital values and how perceptions on either side translate into equilibrium wage outcomes.

Appendix 1.A Counterfactual Labor Market Questions

How do you expect your future workday when you finish your first choice [*include major*]? Estimate the following variables for the different stages of life.

	Working hours/Week	Salary/Year (gross in €)
at career start	[ ]	[ ]
at age 40	[ ]	[ ]
at age 55	[ ]	[ ]

(Original: *Wie erwarten Sie Ihren zukünftigen Arbeitsalltag, wenn Sie ihre erste Wahl [...] zu Ende studieren? Schätzen Sie die folgenden Variablen jeweils für die verschiedenen Lebensabschnitte.*)

How do you expect your future workday when you cannot complete a degree and start working without a degree? Estimate the following variables for the different stages of life.

	Working hours/Week	Salary/Year (gross in €)
at career start	[ ]	[ ]
at age 40	[ ]	[ ]
at age 55	[ ]	[ ]

(Original: *Wie erwarten Sie Ihren zukünftigen Arbeitsalltag, wenn Sie kein Studium abschließen können und ohne Studienabschluss beginnen zu arbeiten? Schätzen Sie die folgenden Variablen jeweils für die verschiedenen Lebensabschnitte.*)

How do you rate the likelihood of not finding a suitable job for the various scenarios at the time of starting your career?

Completion first choice [*major*]	[ ]
Dropout - no degree	[ ]

(Original: *Wie schätzen Sie die Wahrscheinlichkeit zum Zeitpunkt des Berufseinstiegs keinen passenden Job zu finden für die verschiedenen Szenarien ein?*)

How do you rate the likelihood of not finding a suitable job for the various scenarios at age 40?

Completion first choice [*major*]	[ ]
Dropout - no degree	[ ]

(Original: *Wie schätzen Sie die Wahrscheinlichkeit mit 40 Jahren keinen passenden Job zu finden für die verschiedenen Szenarien ein?*)

How do you rate your professional satisfaction at the time you started your career for the various scenarios?
1 → very dissatisfied, 10 → very satisfied

	1	2	3	4	5	6	7	8	9	10
Completion first choice [*major*]	○	○	○	○	○	○	○	○	○	○
Dropout - no degree	○	○	○	○	○	○	○	○	○	○

(Original: *Wie schätzen Sie Ihre berufliche Zufriedenheit zum Zeitpunkt des Berufseinstiegs für die verschiedenen Szenarien ein?*)

How do you rate your professional satisfaction at age 40 for the various scenarios?
1 → very dissatisfied, 10 → very satisfied

	1	2	3	4	5	6	7	8	9	10
Completion first choice [*major*]	○	○	○	○	○	○	○	○	○	○
Dropout - no degree	○	○	○	○	○	○	○	○	○	○

(Original: *Wie schätzen Sie Ihre berufliche Zufriedenheit mit 40 Jahren für die verschiedenen Szenarien ein?*)

Appendix 1.B Data-cleaning Rules

For our analysis, it was important that all included individuals filled in the following variables: expected labor market outcomes for the leaving and finishing scenarios at all points in time, probability of leaving, probability to change majors, gender, age, degree enrolled in, semesters done, semesters left until next degree, perceived academic ability, perceived job ability, GPA, high-school GPA, study costs, study satisfaction, university major, academic parents, and migration background. If one of these were missing, we excluded the individual from our sample.

As individuals could fill in any expected wage and working hours, we cleaned them to remove implausible values. With respect to working hours, this means that we exclude values above 168 hours, as this is the maximum amount of hours within a week (amounts to less than 0.05% of our sample). For wages, we first calculated the wage per hour by dividing the yearly wage by 52 and the indicated working hours per week. We then exclude everybody who has a hourly wage of below €7.50, which is even lower than the minimum wage of €8.50 that was introduced in Germany at the beginning of 2015. In addition, we exclude people who have an hourly wage above €80 at labor market entry or above €240 at age 40 and 55. For the remaining sample, we multiply the hourly wage by 2080 to obtain yearly full-time wage expectations.

Appendix 1.C Negative Signaling

In this paper, we assume that obtaining a degree from university yields a positive signaling value in the labor market. Alternatively, it is conceivable that leaving university without a degree sends a negative signal in the labor market. Similar to a positive signal when graduating, leaving university might also inform potential employers about unobservable abilities, such as a lack of perseverance or motivation. In the following, we will explain why we think the assumption of a positive signaling value is reasonable. We will then show that even without this assumption, the absolute size of our estimated signaling value remains valid. Assuming that education sends a positive signal in the labor market is in line with most of the literature. The latter assumption is reasonable as individuals usually have the freedom not to inform employers about an unfinished degree. As leaving university without graduating is not a (negative) signal that has to be necessarily sent in the labor market, individuals very often would not mention it in their application. When applying to a job, students who left before graduating would most of the time only include their highest education level obtained and if possible would not make dropout salient. Thus, education can be used as a positive signal in the labor market, although it is unlikely to be used as a negative signal. Nevertheless, even if a (partly) negative signal exists, the overall value of the estimated signal stays the same. The main difference between graduating yielding a positive signal and graduating meaning to avoid sending a negative signal lies in the relative importance of the human capital effect. The following equations show the implications of this assumption. In our data, we observe the university-leaving wage $W_i^l(t)$ and the graduation wage $W_i^f(t)$ for individual i at time t both in expectation. Obtaining a positive signal when graduating implies that the university-leaving wage shortly before the degree (at time T) resembles the human capital effect $HC_i^+(T)$, where the "+" indicates that we assume a positive signal here: $signal_i^+$ (likewise a "$-$" indicates the supposition of a negative signal: $signal_i^-$). The following equations show how the signal is calculated assuming it to be positive:

T	end of last semester studied
$W_i^f(T)$	expected wage if graduation at time T
$W_i^l(T)$	expected wage if leaving university at time T

Positive Signal of Graduation

$HC_i^+(T)$	expected wage for accumulated human capital at time Tl
$signal_i^+$	expected positive signaling value at time T

Negative Signal of Graduation

$HC_i^-(T)$	expected wage for accumulated human capital at time Tl
$signal_i^-$	expected negative signaling value at time T

$$HC_i^+(T) = W_i^l(T)$$
$$HC_i^+(T) + signal_i^+ = W_i^f(T)$$
$$\boldsymbol{\Rightarrow signal_i^+ = W_i^f(T) - W_i^l(T)}$$

Now we can calculate the signal under the assumption that graduating means avoiding to send a negative signal in the labor market. Hence, the expected graduation wage corresponds to the full human capital value $HC_i^-(T)$, whereas the university-leaving wage resembles the human capital value minus the absolute value of the negative signal: $|signal_i^-|$.

$$HC_i^-(T) = W_i^f(T)$$
$$HC_i^-(T) - |signal_i^-| = W_i^l(T)$$
$$\boldsymbol{\Rightarrow |signal_i^-| = W_i^f(T) - W_i^l(T)}$$

We can see that the absolute value of the signaling value is unaffected by the assumption regarding the sign of the signal as $|signal_i^-| = signal_i^+$. Hence, even without making assumptions on the sign of the signal our estimations are valid. However, as we assume that $W_i^f(T) > W_i^l(T)$, the human capital value of a degree differs between the two suppositions, with a smaller human capital value under the assumption of a positive signaling value: $HC_i^+(T) < HC_i^-(T)$.

Note that both outcomes also hold true if we assume that graduating leads to *both* a positive signal due to the degree *and* the avoidance of a negative signal that would be associated with leaving university (see equations below).

$$HC_i^{both}(T) = W_i^l(T) + |signal_i^-|$$
$$HC_i^{both}(T) + signal_i^+ = W_i^f(T)$$
$$\Rightarrow W_i^f(T) = W_i^l(T) + |signal_i^-| + signal_i^+$$
$$\boldsymbol{\Rightarrow |signal_i^-| + signal_i^+ = W_i^f(T) - W_i^l(T)}$$

In this case, measuring the human capital value is not possible without making further assumptions on the size of the two signals, as there exists no state of the world in which no signal is sent. Nevertheless, one could calculate a lower and upper bound as the magnitude of the human capital value must lie between the other two scenarios $HC_i^+(T) < HC_i^{both}(T) < HC_i^-(T)$.

Altogether, the assumptions regarding the sign of the signaling value has an impact on how to interpret the human capital value and how to evaluate the relative importance of human capital vs signaling. However, our estimate of the size of the signal stays valid under all possible assumptions.

Appendix 1.D Bachelor Vs. Master Students

In our analysis of the signaling effect, we focus on master students for the reason that they face less ambiguity about both their own abilities and the possible pathways in the counterfactual labor market scenarios. While a master's degree is an additional university degree on top of an existing bachelor degree, bachelor students only achieve their first academic degree when graduating. Therefore, leaving bachelor studies is likely to be associated with higher uncertainty compared with leaving master studies.

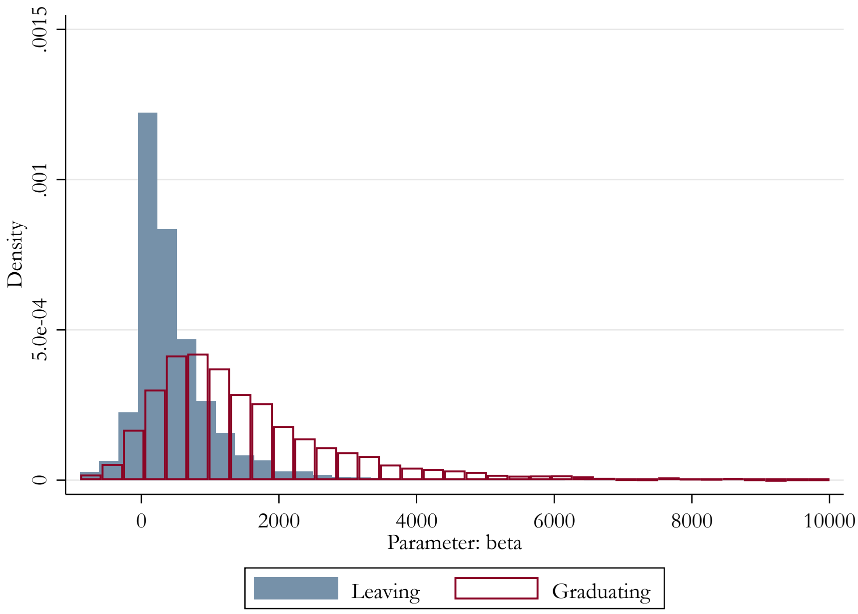
First, the potential pathways in the labor market after leaving are more straightforward for master students. Bachelor students who do not obtain a degree will enter the labor market without any academic degree, while leaving master studies always comes with the outside option of "falling back" on one's first academic degree. As most job opportunities for master graduates are also open for bachelor graduates (and so master dropouts), job prospects for leaving are much closer to the graduating plans that master students would pursue. For bachelor students, there exists not only uncertainty with respect to the wage when leaving, but also with respect to the type of job they can do. Non-degree leavers might need to apply to different kind of jobs – potentially even in a different sector – compared to graduates. We mitigate this effect by controlling for licensing, although compared to master students the uncertainty bachelor students face remains higher.

Second, students might face some ambiguity with respect to their own study and work ability. When survey respondents estimate future wages for the two labor market scenarios, they might condition their beliefs on their own abilities, which are ex-ante still unknown to themselves. For the leaving scenario, they might be expecting to find themselves in a bad state, in which their ability turned out to be worse than for the graduating scenario. In the master studies, prior study experience should help to resolve the uncertainty about own study ability and the productivity-enhancing effect of obtaining a degree. However, for bachelor students, graduating informs them about their abilities and part of the premium that we observe for bachelor students might stem from individuals conditioning the counterfactual expectations on the signal about their productivity (Stinebrickner and Stinebrickner, 2012; Arcidiacono et al., 2016). This would lead to an overestimation of the signaling effect. For master students, the premium to finishing the degree is less affected by ambiguity about own ability, as students have already spent several years at university. They thus dispose of information on their skills from their bachelor studies.

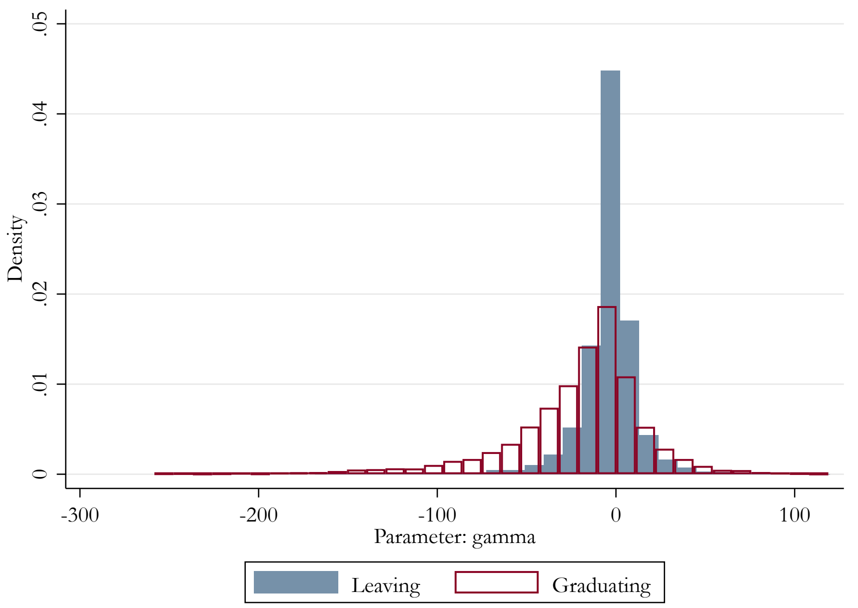
When we look at our results, the higher uncertainty for bachelor students makes it unsurprising that we indeed find the magnitude of the estimated signal to be higher for bachelor students (32.8%) than for master students (20.6%). Nonetheless, the patterns for bachelor and master students are still closely comparable for our results in section 1.4. However, the differences between bachelor and master students become more prominent when we examine heterogeneities in section 1.5.1. For master students, the signal in general does not depend on background characteristics, which is in line with the signaling theory. For bachelor students, having a migration background, academic parents and a higher perceived job ability positively influence the importance of the signal, although the magnitude of the effects remains moderate compared to the effect size of the signal itself. These heterogeneous effects are likely to be driven by the larger ambiguity that bachelor students face about the two scenarios. For instance, if there is high uncertainty about the segment of the labor market in which a person can work after leaving, and having academic parents is only beneficial if the student enters an academic job, a discrepancy based on parental background may arise.

Appendix 1.E Additional Figures And Tables

Figure 1.E1: Computed parameters of the mincer wage equation by scenario



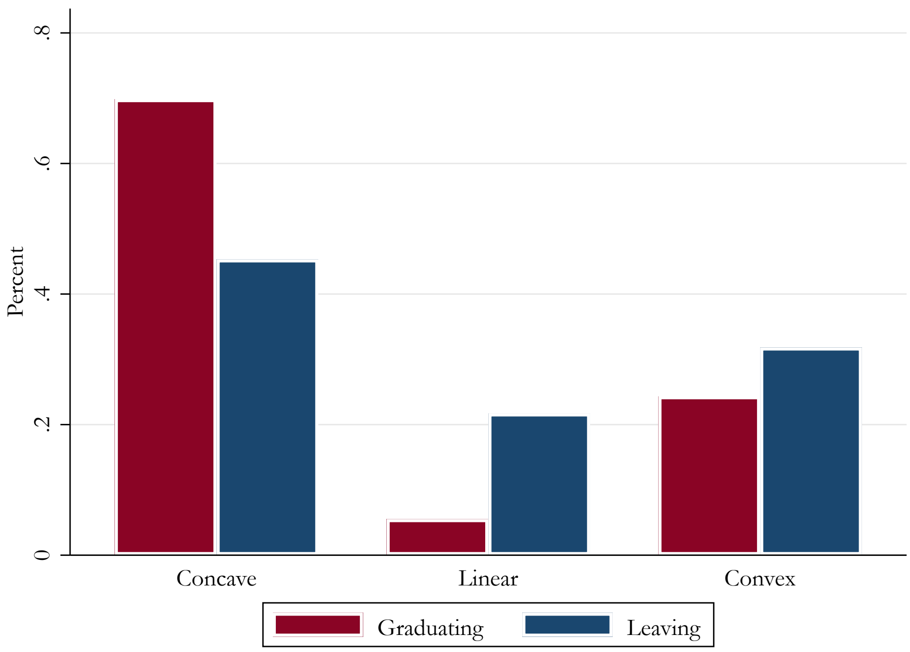
(a) Density of slope parameter β



(b) Density of curvature parameter γ

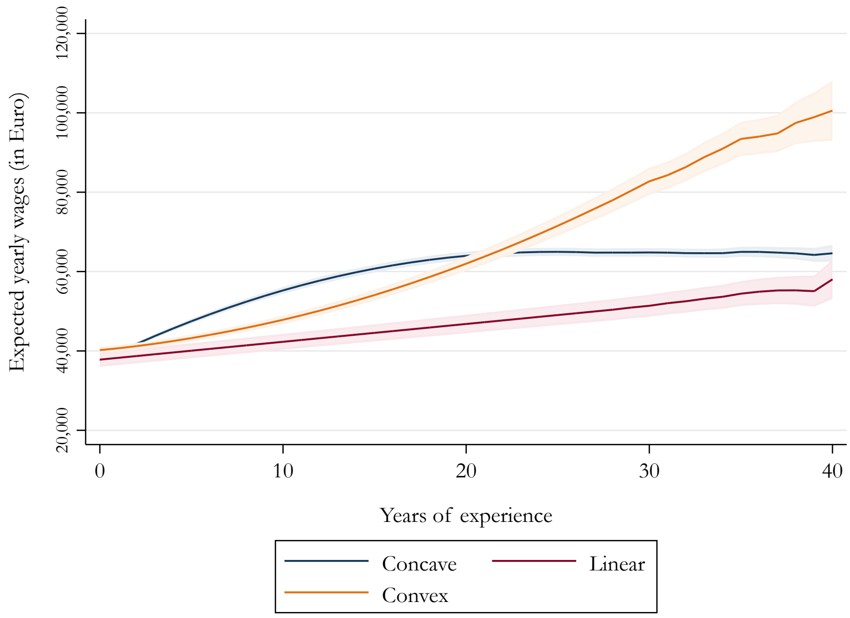
Notes: Figure 1.E1 panel A shows the distribution of the computed slope parameter β from equation (1.1). Panel B shows the respective curvature parameter γ of equation (1.1). Both graphs only display parameters that lie between the 1st and the 99th percentile of the distribution of graduating parameters.

Figure 1.E2: Patters of wage trajectories

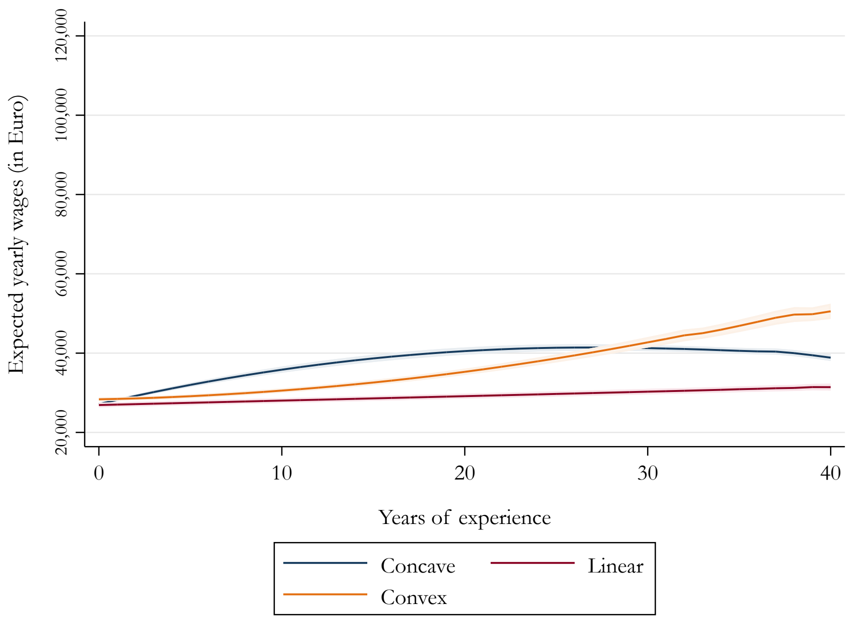


Notes: Figure 1.E2 shows the share of different wage trajectory patterns, that were classified on hands of the parameters of the mincer equation (see equation 1.1) by scenario.

Figure 1.E3: Expected wage trajectories by scenario and type



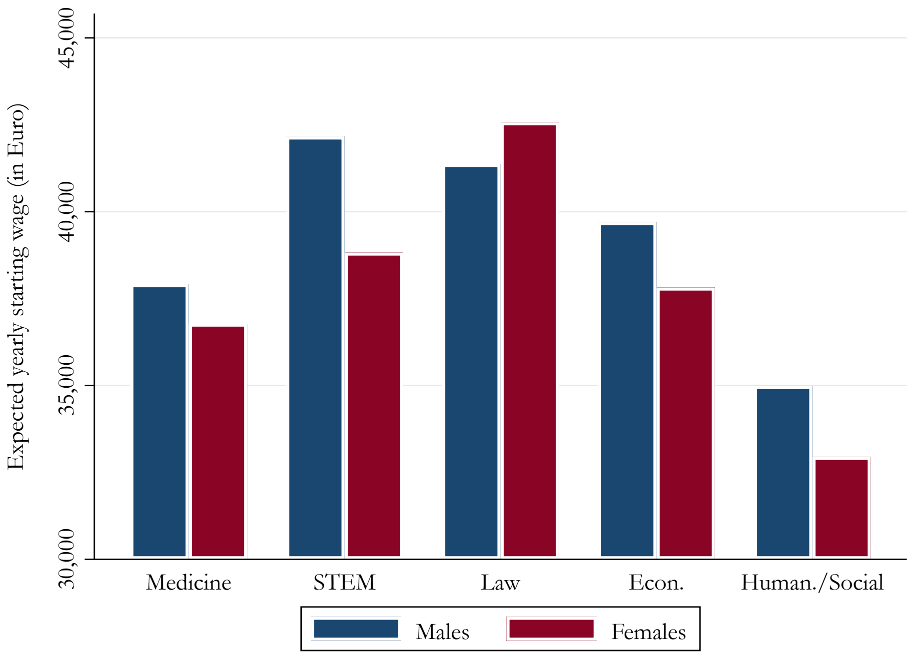
(a) Average wage after graduation, by wage function classification



(b) Average wage after leaving, by wage function classification

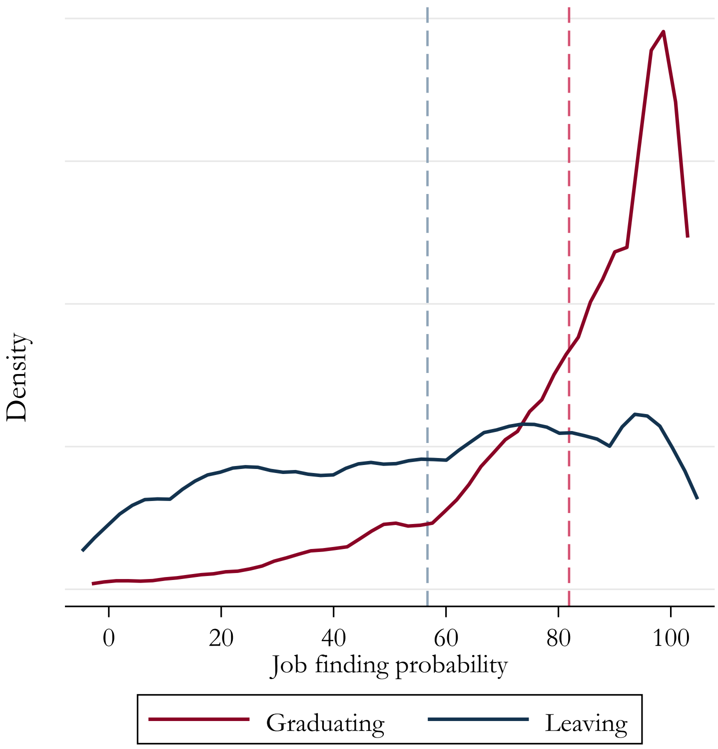
Notes: Figure 1.E3 shows the expected wage trajectories by different wage function classifications. Panel A shows the wage trajectories for the scenario of graduating and panel B for the scenario of leaving university. The wage trajectories are classified in terms of different parameters of equation (1.1).

Figure 1.E4: Expected yearly earnings after graduating at labor market entry by gender and major



Notes: Figure 1.E4 displays the average expected yearly starting wage after graduating university by gender and major. The sample includes both bachelor and master students.

Figure 1.E5: Expected probability to find a suitable job at labor market entry by scenario



Notes: Figure 1.E5 displays the density of the expected probability to find a suitable job at labor market entry for both scenarios. The average expected job-finding probability at labor market entry for graduating is 71.1% and for leaving university without a degree 47.0%. The sample includes both bachelor and master students.

Table 1.E1: Wage returns (bachelor students)

	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs
Semesters	253.067***	0.006***	0.006***
	(92.238)	(0.002)	(0.002)
Degree	10,491.155***	0.326***	0.321***
	(405.822)	(0.010)	(0.011)
INTERACTION EFFECTS:			
Licence*Degree			0.035***
			(0.010)
Civil servant*Degree			-0.026**
			(0.012)
Constant	27,991.612***	10.173***	10.175***
	(396.736)	(0.010)	(0.010)
N	8768	8768	8730
adj. R^2	0.486	0.598	0.600

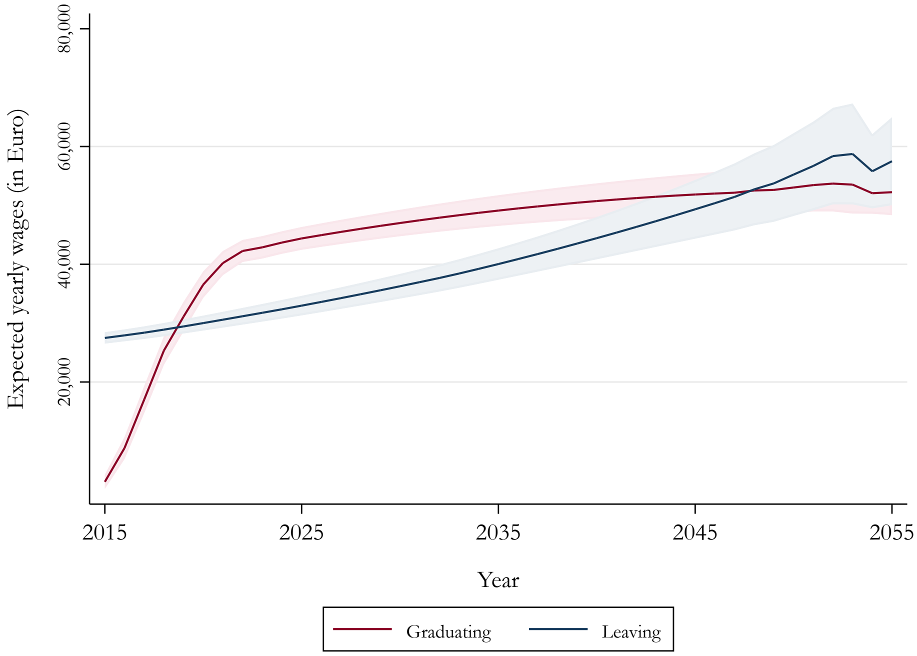
Notes: Column 1 in table 1.E1 shows the effects on the level of yearly starting wages, while the dependent variable in columns 2 and 3 are log starting wages. The sample only includes bachelor students, who have maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 1.E2: Non-wage returns (bachelor students)

	(1) Satisfaction	(2) Satisfaction	(3) Job finding probability	(4) Job finding probability
Semesters	0.007	0.008	0.009	0.010
	(0.013)	(0.013)	(0.009)	(0.009)
Degree	1.484***	1.464***	0.815***	0.776***
	(0.060)	(0.062)	(0.042)	(0.043)
INTERACTION EFFECTS:				
Licence*Degree		0.176***		0.239***
		(0.061)		(0.042)
Civil servant*Degree		-0.163**		-0.130***
		(0.068)		(0.050)
Constant	0.030	0.037	0.038	0.044
	(0.057)	(0.057)	(0.040)	(0.040)
N	8768	8730	8768	8730
adj. R^2	0.461	0.462	0.347	0.353

Notes: Columns 1 and 2 in table 1.E2 show the effects on expected job satisfaction at labor market entry, while the dependent variable in columns 3 and 4 is the expected probability to find a suitable job. Both satisfaction and job-finding probability are expressed in standard deviations according to equation (1.5). The sample only includes bachelor students who have at most eight semesters left until they reach their degree. Standard errors are in parentheses.***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Figure 1.E6: Expected yearly wage over the life time



Notes: Figure 1.E6 shows the development of the expected yearly wage over the life time for students who expect to diminish the wage difference between the graduating and leaving scenario. The colored areas around the lines indicate the 95% confidence intervals.

Table 1.E3: Employer learning by work experience

	Starting age & age 40		Age 40 & age 55	
	(1)	(2)	(3)	(4)
	Log Wages	Log Wages	Log Wages	Log Wages
Semesters	0.012**	0.012**	-0.002	-0.002
until next Degree	(0.005)	(0.005)	(0.007)	(0.007)
Signaling	0.203***	0.186***	0.337***	0.313***
(Graduated)	(0.016)	(0.019)	(0.025)	(0.027)
Work	0.016***	0.015***	0.011***	0.011***
Experience (years)	(0.001)	(0.001)	(0.001)	(0.001)
Interaction Effects:				
Semester*Experience	0.000	0.000	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Signal*Experience	0.008***	0.008***	-0.001*	-0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Productivity*Experience	0.003***	0.003***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Productivity*Signal		0.027**		0.038**
		(0.013)		(0.016)
Constant	10.304***	10.305***	10.369***	10.371***
	(0.016)	(0.016)	(0.024)	(0.023)
N	5524	5524	5524	5524
adj. R^2	0.579	0.580	0.568	0.569

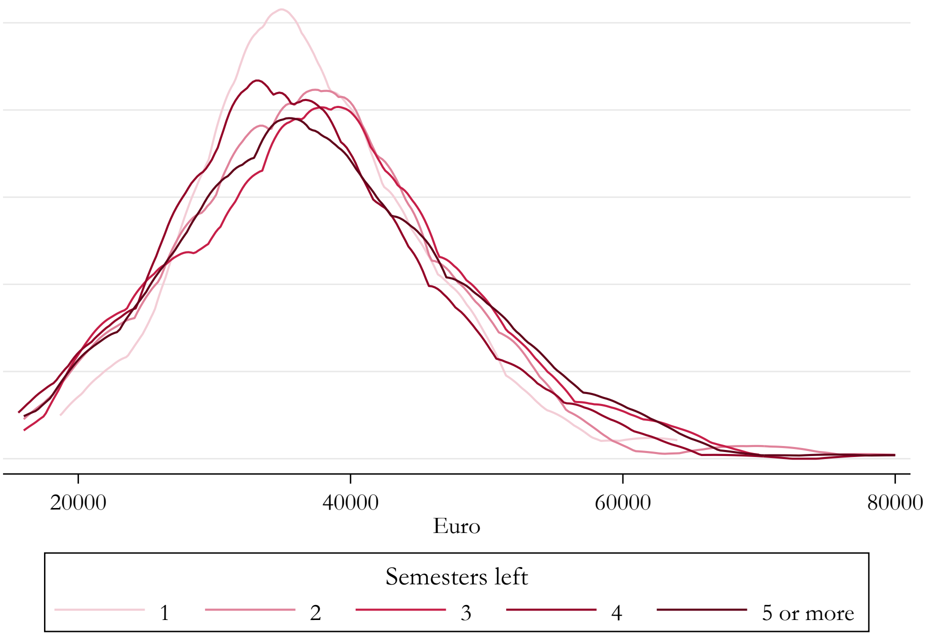
Notes: Table 1.E3 shows the effects of semesters studied, the degree and work experience on expected log wages for both scenarios of graduating and leaving university. Column 1 and 2 include wage expectations for the points in time when participants would start a job and at age 40. Column 3 and 4 include wage expectations for the points in time where participants would be 40 and 55 years old. The sample only includes master students who have maximum of eight semesters left until reaching their degree. Standard errors are in parentheses.***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 1.E4: Robustness analyses - wage returns with semester dummies (master students)

	Immediate returns
5 Semes. until degree	0.004
	(0.033)
4 Semes. until next degree	0.018
	(0.030)
3 Semes. until next degree	0.015
	(0.030)
2 Semes. until next degree	0.028
	(0.029)
1 Semes. until degree	0.052
	(0.033)
Degree	0.251***
	(0.027)
N	2762
adj. R^2	0.506

Notes: Table 1.E4 displays the coefficients from estimating equation (1.6). The regression only includes master students who have at most eight semesters left to study. The baseline is to have six or more semesters until graduation. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Figure 1.E7: Expected yearly graduation wage at labor market entry by semesters left



Notes: Figure 1.E7 shows the density of the expected yearly graduation wage at labor market entry by semesters left for master students. Because of readability we only show wages smaller than €80.000.

Table 1.E5: Robustness analyses (bachelor students)

	Regular study time			Best third in high-school			Max 12 semesters		
	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs	(4) Starting wage levels	(5) Starting wage logs	(6) Starting wage logs	(7) Starting wage levels	(8) Starting wage logs	(9) Starting wage logs
Semesters	90.060	0.008	0.008	62.166	-0.001	-0.000	70.804	0.002	0.002
	(244.628)	(0.006)	(0.006)	(163.854)	(0.004)	(0.004)	(68.489)	(0.002)	(0.002)
Degree	10,884.868***	0.312***	0.304***	11,374.875***	0.362***	0.352***	11,166.608***	0.341***	0.336***
	(932.567)	(0.023)	(0.023)	(749.475)	(0.019)	(0.019)	(348.585)	(0.009)	(0.009)
Interaction effects:									
Licence*Degree			0.060***			0.040**			0.034***
			(0.020)			(0.018)			(0.010)
Civil servant*Degree			-0.032			-0.009			-0.024**
			(0.024)			(0.023)			(0.012)
Constant	27,121.896***	10.174***	10.176***	26,977.514***	10.135***	10.137***	27,148.962***	10.154***	10.156***
	(921.197)	(0.022)	(0.022)	(726.193)	(0.018)	(0.018)	(330.106)	(0.008)	(0.008)
N	2622	2622	2610	2842	2842	2828	9588	9588	9548
adj. R^2	0.452	0.566	0.569	0.488	0.600	0.601	0.483	0.596	0.597

Notes: Table 1.E5 shows the outcomes of the robustness analysis. Columns 1-3 include students who are expected to finish within regular study time, i.e. four semesters in total. Columns 4-6 include every student who had a high-school GPA in the highest 33%. Column 7-9 includes all students who are in the 12th semester or less. The sample only includes bachelor students. Standard errors are in parentheses. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 1.E6: Wage returns (bachelor students) - heterogeneities

	Starting wage (logs)				
	(1)	(2)	(3)	(4)	(5)
Degree	0.288***	0.306***	0.309***	0.246***	0.206***
	(0.011)	(0.011)	(0.012)	(0.022)	(0.022)
Semesters	0.006***	0.006***	0.007***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
INTERACTION EFFECTS:					
Male*Degree	0.072***				0.040***
	(0.009)				(0.009)
Academic*Signal		0.015*			0.019**
		(0.009)			(0.009)
Migrat*Degree		0.051***			0.041***
		(0.012)			(0.012)
Perc. job ability*Degree			0.021**		0.015*
			(0.009)		(0.009)
Gpa*Degree				-0.004	-0.003
				(0.003)	(0.003)
MAJORS:					
Medicine*Degree				0.080***	0.083***
				(0.020)	(0.020)
STEM*Degree				0.160***	0.146***
				(0.012)	(0.012)
Law*Degree				0.168***	0.165***
				(0.045)	(0.045)
Economics*Degree				0.104***	0.096***
				(0.011)	(0.012)
Constant	10.174***	10.175***	10.176***	10.172***	10.172***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
N	8730	8730	8730	8730	8730
adj. R^2	0.606	0.602	0.600	0.619	0.623

Notes: Table 1.E6 includes several interaction terms between the degree premium and background characteristics. The sample only includes bachelor students who have a maximum of eight semesters left until they reach their degree. The regressions are controlled for licensing effects. The baseline subject is humanities. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

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Chapter 2

The Impact Of Higher Education On Employer Perceptions

Joint with Pia Pinger and Renske Stans

2.1 Introduction

Employers have an interest in hiring individuals who provide an added value to their company. In this context, individuals with more education credentials are more attractive to employers, who reward them with higher wages and better employment prospects (see e.g. Ashenfelter and Ham, 1979; Card, 1999; Cunha et al., 2011; Falato and Milbourn, 2015; Piopiunik et al., 2017; Patrinos and Psacharopoulos, 2020; Altonji and Zhong, 2021; Lovenheim and Smith, 2022). However, what exactly makes candidates with more education particularly attractive to employers in screening and hiring processes? Does a higher education certificate (or the lack thereof) prompt expectations about particular productive traits, acquired expertise, or candidate background? This paper provides causal answers to these questions by eliciting belief-related candidate judgment among employers in an experimental setting.

To date, little is known about the belief-related sources of educational attractiveness, as perceived or sought after by employers on the labor market. One explanation for the premium is that degree holders possess more knowledge acquired during their studies (Becker, 1962; Schultz, 1963; Chevalier et al., 2004; Aryal et al., 2022). Alternatively, degrees may reflect productive but mostly pre-determined traits, such as IQ or personality traits, i.e., that relate to the psychic costs of studying and future employee productivity (Spence, 1973; Stiglitz and Weiss, 1990; Bedard, 2001; Chatterji et al., 2003; Caplan, 2018). Empirical models on the education premium typically incorporate years of schooling or education degrees (Mincer, 1974; Card, 2001), but provide little explicit evidence on *why* employers may seek workers with more education. Nonetheless, it is important for applicants, firms, and management to understand which traits are sought after in higher-educated workers, while this also holds importance for educational institutions, e.g., as regards the selection and promotion of students and graduates.

In this paper, we conduct a survey experiment among a large pool of human resource managers to assess employer-driven returns to degree completion, as well as education-related beliefs about candidates' productive characteristics. Managers are randomly assigned three realistic résumés of fictitious candidates who have either completed, partly completed or not started a master's degree. Importantly, completed education is one of many varying résumé characteristics and it is *not* particularly emphasized to the managers. After reviewing each résumé, managers indicate the likelihood that their company would interview or hire the candidate, as well as the wage their company would most likely pay conditional on hiring. We then elicit managers' beliefs about each candidate's (i) expertise acquired at university, (ii) cognitive (trainability and IQ) traits, (iii) non-cognitive (perseverance, conscientiousness, commitment and emotional stability) traits, and (iv) socio-economic background.

Our analysis proceeds in three steps. First, we investigate how a degree (partially completed or completed) influences employer assessments of candidates' attractiveness. Second, we explore how employers' beliefs about candidates' acquired and inherent traits differ by educational attainment. Third, we decompose the education premium into belief-related mechanisms to understand the extent to which the elicited beliefs about latent traits can explain differences in candidate attractiveness.

We find that master degree graduates are more desirable to employers than individuals with a bachelor's degree, given that they have higher chances of being invited for an interview or offered a job and are proposed higher starting wages. Second, we show that compared to having a bachelor's degree, obtaining a master degree raises employers' beliefs about a candidate's cognitive and non-cognitive traits, as well as subject matter expertise. An unfinished master's degree tends to lower employers' expectations about the non-cognitive traits of a candidate. We show that up to 75% of the education premium in interview and hiring probabilities can be attributed to differences in beliefs about candidates' characteristics.

This paper contributes to several existing strands of literature. Methodologically, our work most closely relates to Heinz and Schumacher (2017) and Piopiunik et al. (2020), who also confront human resource managers with applicant résumés. Our study differs from these papers in that focus on the causal effect of higher education credentials on candidate attractiveness. Moreover, we innovate by directly eliciting employer beliefs about a substantial number of unobserved applicant traits, to yield a more comprehensive picture about employer beliefs and preferences when making decisions at the screening stage of the application process.

Our research further relates to literature using résumé-based audit studies to causally identify the importance of different worker characteristics. While audit studies have mainly been used to study racial or gender discrimination (Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Kline et al., 2022; Ruffle and Shtudiner, 2015; Kang et al., 2016), they have also been employed to uncover how labor markets reward work experience, or type of educational institution (Deming et al., 2016; Lennon, 2021; Farber et al., 2016; Nunley et al., 2016). For example, Deming et al. (2016) use an audit study to show that employers prefer applicants with degrees from public institutions over those with degrees from for-profits, and Gaulke et al. (2019) report no significant returns to a post-baccalaureate business certificate on the call-back rate. Moreover, Chen (2023) presents evidence from a large-scale audit study showing that U.S.-educated applicants are on average 18% less likely to receive a callback than applicants educated in China when applying on the Chinese labor market. At present, no audit studies exist on (partially completed) degree effects. The advantage of our experimental approach with respect to this literature is twofold: first, our design does not rely on deception;[1] and second, we can study outcomes beyond call-back probabilities including beliefs about hiring probabilities, wages and – most importantly – a large vector of applicant characteristics.

In addition, by unveiling the underlying mechanisms behind the employer demand for higher education credentials, our findings speak to a large body of literature on the sources of returns to higher education, such as productivity differentials, sheepskin effects, human capital, or signaling (see e.g. Weiss, 1995; Lange and Topel, 2006, for reviews). It also complements work focusing on students' expected returns to degree completion (Ehrmantraut et al., 2020) by focusing on the labor demand side.

Finally, we contribute to the literature concerning the importance of cognitive and non-cognitive skills for labor market success. This literature documents that in particular personality aspects related to conscientiousness and emotional stability prove valuable on the labor market (Almlund et al., 2011; Nyhus and Pons, 2005; Salgado, 1997). It also shows that employers likely want to hire and reward individuals with higher levels of cognitive and non-cognitive skills, for several reasons. First, individuals with higher cognitive and non-cognitive abilities are better at information processing and task completion. Second, these traits are both incentive-enhancing and related to intrinsic motivation, allowing employers to induce effort at lower costs (Bowles et al., 2001; Segal, 2012). Employers thus have an incentive to seek and interpret signals about cognitive and non-cognitive skills and act upon these beliefs when selecting candidates.

The remainder of the paper is organized as follows. In section 2.2, we describe the sample recruitment procedure, survey design and main measures. Subsequently, section 2.3 describes our results for each of the three sub-questions and the robustness analysis. Finally, section 2.4 discusses our findings and concludes.

[1] For a discussion of deception in audit studies, see Kessler et al. (2019).

2.2 Study Design

2.2.1 Sample Recruitment

Our survey experiment addresses human resource (HR) managers with real-life hiring responsibilities. HR managers are a target group that hold strong interest for research on the labor market returns to education but are usually difficult to reach. To engage this group of professionals from a wide variety of industries, we drew on the same sample of top-level German HR managers whose judgment also provides the basis for the most well-known employer-based German university ranking ("Wirtschaftswoche Hochschulranking"). We include HR managers who 1) work for companies with at least ten employees, 2) are actively involved in hiring and 3) regularly hire business majors. The latter criteria is a prerequisite for our experimental set-up (see subsection 2.2.2).

Based on the above criteria and excluding individuals with response times of less than four minutes or those who gave non-sensible answers to the open questions, 485 HR managers are included in the sample. In addition, we condition on having filled in reasonable values for prospective wage offers, leaving us with 433 respondents (see appendix 2.C for more details).[2] On average, participants took 10.5 minutes to finish the survey. HR managers were approached by a business partner company and participation was incentivized by a one-time fixed payment. Approaching HR managers via a business partner company served as a "firewall" between the data collection and research teams. This comprised several measures: (i) at no point in time were HR managers informed about the purpose of the study or asked to focus specifically on candidates' educational attainment; (ii) all respondents were approached during work hours and in their role as HR professionals to ensure truthful and unbiased evaluations; and (iii) the research team never interacted directly with the managers excluding potential researcher demand effects. For descriptive statistics on the managers, the companies for which they work, and their hiring process, see appendix table 2.E1.

2.2.2 Applicant Profiles

Each employer in our sample was asked to evaluate three hypothetical applicant profiles.[3] We first present a respective applicant's résumé, after which the employer answers several questions about her perception of the applicant (see subsection 2.2.3). The information on candidate résumés is as realistic as possible in terms of applicant information. The layout is standardized, to ease screening and to avoid distraction or inference that may come from using different front or alignment of information. As many firms use online forms to collect applicant information, one may think of this standardized résumé information as output generated by one of these information systems. While all applicants

[2] In the robustness section, we test the sensitivity of our results to sample selection.
[3] The pre-registration of our survey experiment can be accessed at https://osf.io/tupw3.

are business majors, their profiles experimentally vary in terms of education completed, personal information, and work experience. We chose business majors because most companies hire business graduates irrespective of the type of industry. Moreover, business studies is the most relevant major in the German context, as it is by far the most popular field of study.[4] All applicant information on the résumés was randomized at the respondent level (see appendix 2.B for more details).

Education - Our main characteristic of interest is the applicants' level of higher education obtained. Importantly, employers were not in any way primed or asked to focus on candidate education, and completed education was simply one of many résumé characteristics. We randomly vary between four scenarios: (i) having only a bachelor's degree, denoted as Bsc.; (ii) having a bachelor's degree plus having completed 25% of a master studies (30 ECTS); denoted as Bsc.+25; (iii) having a bachelor's degree plus having completed 75% of a master studies (90 ECTS), denoted as Bsc.+75; or (iv) having completed a master's degree (120 ECTS), denoted as Msc.[5] Each respondent receives résumés with three out of four potential education scenarios.[6]

Résumé design might matter for candidate attractiveness (Kristal et al., 2023). In particular, candidates might highlight degree completion in different ways on their résumé. To ensure that scenarios (ii) and (iii) are conveyed realistically, our résumé designs are based on online recommendations about ways to present university drop-out on a résumé.[7] Our aim was to draw on the same information as real-life applicants (see figures 2.B1 - 2.B4 for the résumé designs). Nevertheless, recent work shows that around one third of applicants omit information about partially completed schooling in the US (Kreisman et al., 2023). Assuming that this evidence extends to European labor markets, it is thus important to remember that our results about dropouts are informative only as regards the 70% of candidates that do reveal this information.

In addition, we vary grades corresponding to the 10th, 50th and 90th percentile of the actual GPA distribution of a large sample of German university students who studied (business) economics to cover a substantial range of educational performance. Finally, we vary the university where students obtained their degree, using three top-rated universities for the subject of business administration, namely Universities of Cologne, Frankfurt, and Munich, all of which, are well-renowned large public universities. The

[4]In the academic year 2022/2023 there were 237,581 students enrolled in business studies, compared to, e.g., 143,582 in informatics, the second most popular field of studies.

[5]The choice of 30 and 90 ECTS is based on the course structure at German universities, which implies that credits are generally awarded in blocks. Besides, students most likely drop out having finished the ECTS of a full semester, which leads to the division in 25% blocks, as master studies in Germany have four semesters. Especially the last semester generally comprises writing a thesis worth 30 credits. It is thus unrealistic to leave university without a diploma while having obtained more than 90 ECTS.

[6]With 11% of all master students in Germany leaving university without a degree, these scenarios are realistic and relevant (Heublein et al., 2014).

[7]The resumes convey that applicants with an unfinished master degree are not in the process of finishing the degree, but instead left university with no intention to return.

corresponding master programs are very similar in terms of student selection and degree quality.

Work Experience - While in many countries, including the US, it is common for business graduates to start working after obtaining a bachelor's degree and before starting a master's degree or MBA program, this is rarely the case in Germany. Instead, more than 80% of master students have entered the program right after completing their bachelor's education. To mimic this institutional feature, résumés includes work experience only in the form of an internship (in one of three fields: sales, project management or controlling). We vary not only the field of work but also the length and type of internship and the company name.

Variation of other résumé items - In order to create realistic applicant résumés, we vary multiple other characteristics across the three résumés that each HR manager evaluated. Using résumés with varying characteristics also ensured that the different educational scenarios did not play an overly prominent role on each résumé. Further, the candidates differ in terms of gender – indicated by the name of the applicant – and age. The applicants' years of birth slightly differ to avoid gaps on the résumé when presenting different lengths of education. Other variation on the résumés is related to the applicants' language skills, free-time activities, IT skills and secondary school grade. For more details on the creation of the résumés and the different components, see subsection 2.B.1 in appendix and the corresponding table 2.B1.

2.2.3 Candidate Attractiveness And Beliefs

To elicit perceived candidates' attractiveness and traits, we ask employers to imagine they want to fill an entry-level job at their firm. The respective entry-level job is randomly chosen to be a position either in project management or controlling (each with a 50% probability). We use two fields of specialization that most hiring employers are familiar with irrespective of the industry and that are often filled with applicants who have a background in business administration. We vary the type of position to investigate whether degree returns or beliefs about expertise or traits differ by the type of job. Controlling arguably requires more study-specific knowledge, while project management is more demanding in terms of non-cognitive traits. To measure hiring outcomes, we ask the employers to answer the following questions for each applicant profile:

1. What is the likelihood that you would invite [*applicant name*] for a job interview? (0-100%)

2. Conditional on satisfactory performance in the interview, what is the likelihood that you would offer [*applicant name*] a job? (0-100%)

3. Which gross yearly salary excluding bonus payments would you offer [*applicant name*]? (in Euro)

The questions correspond to the different steps of the hiring process allowing us to investigate at which stage (not) having a degree matters most. In addition, we are interested in which underlying traits employers associate with a (unfinished) degree and expect to secure when hiring a candidate. For education to function as relevant proxy information, these traits should positively relate to applicant productivity and negatively relate to the (psychic) cost of studying (Weiss, 1995). Existing studies offer an indication of important traits in this context. First, in the employer learning literature cognitive measures such as IQ and trainability are often considered relevant measures of productivity, while also being imperative for educational attainment (see e.g. Thurow, 1975; Farber and Gibbons, 1996; Altonji and Pierret, 2001; Lange, 2007; Di Stasio, 2014; Arcidiacono et al., 2010; Aryal et al., 2022). Second, non-cognitive traits are shown to be predictive for both educational and labor market outcomes, with conscientiousness and emotional stability being the most important ones (Heineck and Anger, 2010; Mueller and Plug, 2006; Nyhus and Pons, 2005; Almlund et al., 2011; Heckman et al., 2019). Moreover, grit may be especially relevant in our context where some applicants left without a degree. Grit is defined as "perseverance and passion for long-term goals" and correlated with both education and employment outcomes (Duckworth et al., 2007; Duckworth and Quinn, 2009). However, as grit is a concept that is potentially unknown to employers, we use the terms perseverance and commitment instead.[8] Third, employers may seek subject matter knowledge acquired during studying (henceforth called expertise), in line with the predictions from human capital models (Becker, 1962; Chevalier et al., 2004). Finally, there is evidence of students from more advantaged socio-economic backgrounds experiencing higher labor market returns, while also facing lower costs of education (see e.g. Björklund and Salvanes, 2011; Solon, 1999). Moreover, individuals from lower socio-economic backgrounds often face systematic disadvantages in hiring processes (Belmi et al., 2023).

We ask each employer to judge applicant characteristics compared to the entire cohort of recently graduated business students along the dimensions of trainability and IQ (cognitive traits), commitment, perseverance, conscientiousness and emotional stability (non-cognitive traits), subject matter expertise, and socio-economic status. Employers evaluate each trait on a scale from -100 to 100, where positive (negative) values mean the candidate scores above (below) average.[9]

[8] Although grit is correlated with conscientiousness it has additional predictive power, especially when focusing on perseverance (Credé et al., 2017).

[9] For a complete overview of our survey questions, please see appendix 2.A.

2.3 Results

2.3.1 How Does Education Shape Candidate Attractiveness?

In a first step, we assess how educational differences translate into candidate attractiveness to confirm the existence and magnitude of education premia in our data.

Figure 2.1, presents the raw averages of the three main employment outcomes for each of the four educational scenarios: bachelor degree (Bsc.), bachelor degree but dropped out after obtaining 25% or 75% of additional master credits (30 or 90 ECTS in the European system), and a completed master degree (Msc.). A series of t-tests is conducted to assess whether the differences between the Bsc. scenario and other respective scenarios are significantly different from zero. The figure shows that outcomes for the first two bachelor scenarios are similar, but the invitation probability is significantly lower for the Bsc.+75% scenario. For the master scenario, all outcomes are significantly better. Throughout the paper, we distinguish labor market returns that increase the chance of being hired (invitation probability and probability to offer a job), and wage returns conditional on getting the job. Employers thus "reward" a completed master degree during all steps of the hiring process, while solely obtaining partial credits of a master's degree does seem to negatively affect candidate attractiveness. The corresponding regression estimates are presented in columns 1, 3 and 5 of table 2.1 . In addition, we estimate the returns to education as follows:

$$Y_{i,m} = \alpha + \beta_{25}\, Bsc.25_i + \beta_{75}\, Bsc.75_i + \beta_{Msc.}\, Msc._i + \beta_X\, X_i + \gamma_m + \epsilon_{i,m}, \tag{2.1}$$

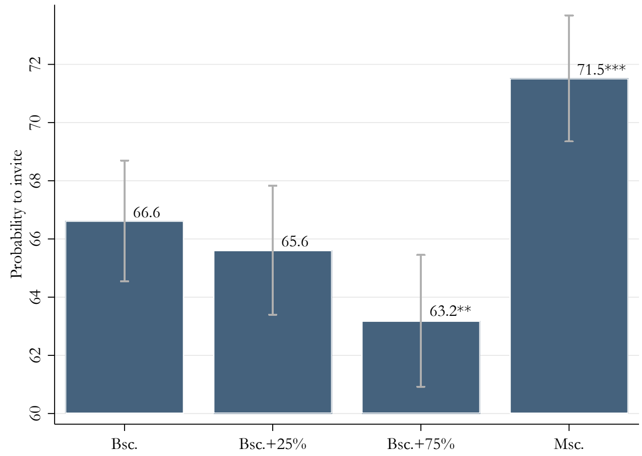
where $Y_{i,m}$ is a respective measure of candidate attractiveness (invitation probability, offer probability, potential wage) for applicant profile i assessed by employer m. X_i is a vector of control variables comprising all randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. The employer fixed effect is represented by γ_m and captures employer traits and the type of job (controlling or project management). The coefficients of interest are $\beta_{Bsc.25}$, $\beta_{Bsc.75}$ and $\beta_{Msc.}$, which yield the return to education with respect to the baseline of obtaining a bachelor degree.

The results are presented in columns 2, 4 and 6 of table 2.1. Controlling for other résumé items and including employer fixed effects only slightly reduces the effect sizes and significance of the coefficients.[10] Obtaining a master degree over a bachelor degree increases the probability of being invited or offered a job by 4.5 and 3.6 percentage points (6.6 and 5.3%), respectively. The wage offered increases by 4.8% after obtaining a master degree.

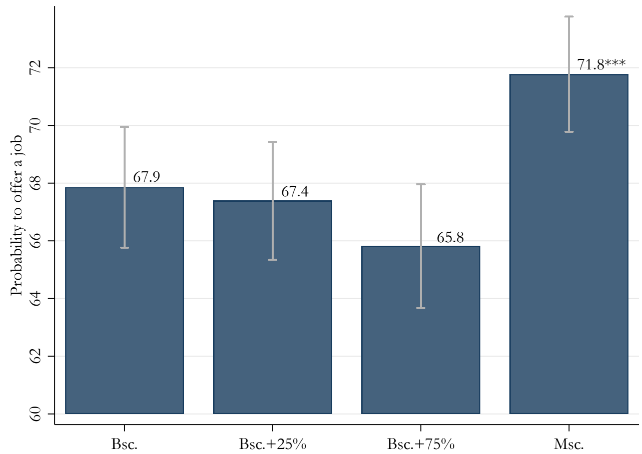
Besides, the results indicate a marginally significant negative effect on the invitation

[10]See table 2.E2 in appendix 2.E for the coefficients of all other résumé items.

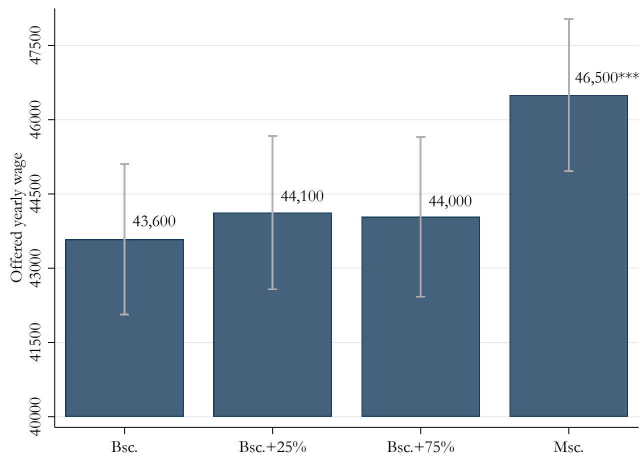
Figure 2.1: Employment outcomes by educational scenario



(a) Invitation probability



(b) Probability of offering a job



(c) Prospective wages

Notes: The figure displays the average invitation probability (Panel A), the probability of offering a job (Panel B), and the prospective wage (Panel C) by educational achievement. The stars indicate significance from a series of two-sided t-tests, that compare the average of the Bsc. scenario with the respective averages of each of the other scenarios. Error bars indicate 95% confidence intervals.

Table 2.1: Employment outcomes by educational scenario

	Base Bsc.					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pr. to invite	Pr. to invite	Pr. to offer	Pr. to offer	Log wage	Log wage
Bsc.+25%	-1.010	-0.492	-0.467	0.361	0.011	-0.001
	(1.311)	(1.238)	(1.255)	(1.170)	(0.015)	(0.008)
Bsc.+75%	-3.435**	-2.302*	-2.042	-0.831	0.007	0.000
	(1.391)	(1.267)	(1.347)	(1.241)	(0.015)	(0.008)
Msc	4.897***	4.486***	3.920***	3.601***	0.070***	0.048***
	(1.233)	(1.169)	(1.228)	(1.179)	(0.016)	(0.008)
N	1299	1299	1299	1299	1299	1299
Ind. FE	No	Yes	No	Yes	No	Yes
Controls (other)	No	Yes	No	Yes	No	Yes

Notes: All columns show coefficients that are estimates from a linear regression, with columns 2, 4 and 6 including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

probability of dropping out after completing 75% of studies. Hence, even conditional on academic performance, obtaining (a large) part of the credits from a master degree does not improve labor market prospects when compared to a bachelor degree. Thus, a completed degree serves – at least to some extent – as a positive signal to employers, whereas an unfinished degree fails to do so, even if a substantial part of the coursework has been successfully completed. If anything, leaving university with completed coursework but no degree seems to be perceived as a negative signal by employers.

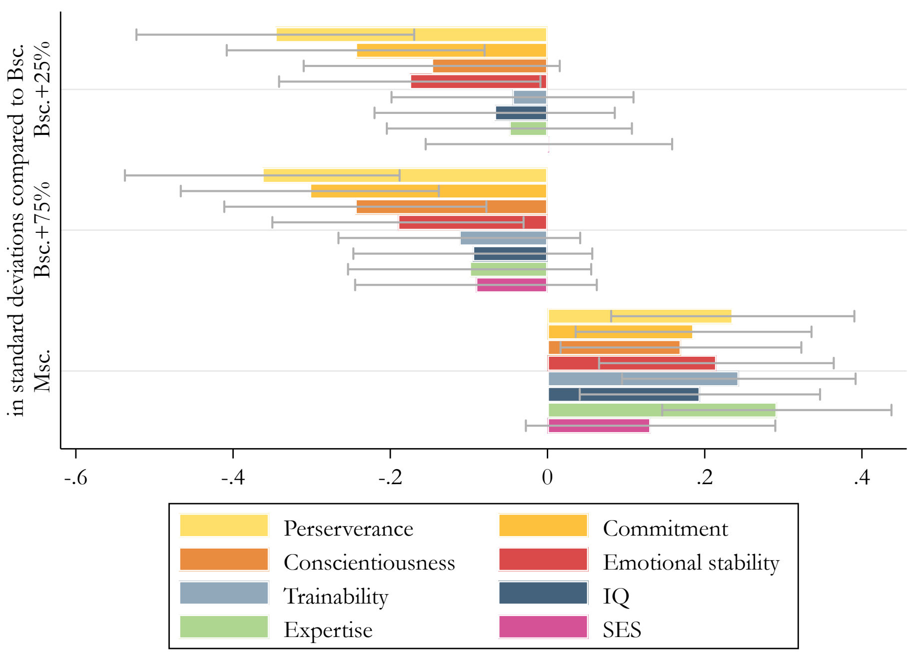
There are several (non-exclusive) explanations for this finding. First, employers might not believe in human capital accumulation if no degree was obtained. Second, not finishing a degree might be associated with adverse non-cognitive traits, outweighing any positive human capital effects. The marginally negative effect size in columns 1 and 2 of table 2.1 supports this presumption. The smaller – and insignificant – negative coefficients for applicants who left after finishing 25% of the course material suggest that the negative signal of dropping out increases with time spent in the degree program. Third, human capital in the form of expertise might not be valued by employers. We further investigate these hypotheses in subsection 2.3.2.

2.3.2 Education Effects On Employer Beliefs

In this section, we investigate the underlying mechanisms by assessing how education affects employer beliefs about candidate characteristics. In line with economic theory, we distinguish between (i) pre-determined productive traits such as cognitive (trainability and IQ) and non-cognitive traits (commitment, perseverance, conscientiousness and emotional stability) related to the psychic costs of studying (Spence, 1973; Stiglitz and Weiss, 1990; Bedard, 2001; Chatterji et al., 2003; Caplan, 2018), and (ii) accumulated human capital in the form of subject matter expertise (Becker, 1962; Schultz, 1963; Mincer, 1974; Chevalier et al., 2004; Aryal et al., 2022). Besides, we assess beliefs about socio-economic status as a proxy for family support and financial constraints.

Figure 2.2 displays mean differences in employer beliefs about Bsc.+25%, Bsc.+75%, and Msc. candidates compared to Bsc. candidates, where all trait scores are standardized using the respective Bsc. distributions. Error bars indicate 95% confidence intervals.[11]

Figure 2.2: Employer beliefs by educational scenario



Notes: The figure displays standardized differences in trait scores of the Bsc. +25%, Bsc. +75%, and Msc. scenarios compared to the Bsc. scenario, with all scores being standardized with respect the Bsc. distributions. The gray bars indicate 95% confidence intervals.

We observe several patterns. First, starting but not finishing a master degree induces a downward shift in beliefs regarding non-cognitive traits when compared to the bachelor scenario. The negative effect is particularly strong for perseverance and commitment but also apparent for conscientiousness and emotional stability. For Bsc.+75% candidates,

[11] See table 2.E3 for the accompanying averages and p-values.

these effect sizes amount to over 19% of a standard deviation for emotional stability and over 30% of a standard deviation for perseverance and commitment. Second, regarding cognitive traits, the differences between Bsc. and Bsc.+25% or Bsc.+75% candidates are not statistically significant. Thus, the number of study credits completed does not prompt employers to believe in higher accumulated expertise. Third, master graduates score significantly higher on all trait dimensions compared to bachelor graduates, with effect sizes amounting to 17-30% of a standard deviation. In particular, master graduates are perceived to perform better in terms of trainability (24% of a stand. dev.), and expertise (29% of a stand. dev.) compared to bachelor graduates. Perceived socio-economic status is positively (but not significantly) affected by Msc. attainment.

We now turn to the question whether employers hold correct beliefs about the traits of individuals from different educational groups. As there are no readily available datasets that contain information on the above traits as well as measures of master dropout, we have conducted a follow-up survey on degree completion among individuals who formerly participated in a large student survey in Germany (Fachkraft 2030). The data contains high quality measures of IQ, emotional stability, and conscientousness, as well as socio-economic status.[12] We find that, with one exception, the differences in actual traits displayed in figure 2.D1 are surprisingly similar to the employer beliefs reported in this section. We interpret this as suggestive evidence that employers on average hold accurate beliefs.

The findings in this section improve our understanding in several respects. First, adverse beliefs about non-cognitive traits of Bsc.+25% and Bsc.+75% candidates substantiate the notion that leaving university without a degree is perceived as a negative signal about pre-determined non-cognitive traits. Moreover, the fact that employers do not acknowledge subject matter expertise in Bsc.+25% and Bsc.+75% candidates indicates that human capital effects are closely tied to the signal of obtaining a degree. Finally, obtaining a degree serves as a positive signal about both pre-determined cognitive and non-cognitive traits *and* acquired human capital.

2.3.3 Beliefs And Candidate Attractiveness

We now explore how much of the differences in candidate attractiveness for bachelor, unfinished master degree, and master degree holders can be explained by differences in beliefs about the candidates' expertise, (non-)cognitive traits, and socio-economic status. To assess the relative importance of the elicited belief mechanisms, we present candidate attractiveness as a function of productive traits. We then use this function to conduct a mediation analysis with the aim of quantifying and decomposing how any of the significant differences in candidate attractiveness by educational attainment relate to employer beliefs about candidate characteristics.

[12] See Appendix 2.D for a detailed description of the data and available measures.

Assuming that candidate attractiveness ($Y_{i,d}$) is a function of a candidate's productive characteristics, where beliefs about these characteristics vary by randomly assigned degree completion, candidate i's market attractiveness – when degree assignment is set to "treated" ($d = 1$) for master or Bsc.+75% or "control" ($d = 0$) for bachelor – is written as:

$$Y_{i,d} = \kappa_d + \alpha_d^C C_{i,d} + \alpha_d^N N_{i,d}^A + \alpha_d^E E_{i,d}^A + \alpha_d^{SES} SES_{i,d}^A + \alpha_d^U U_{i,d} + \epsilon_{i,d}, \quad d \in \{0,1\}, \tag{2.2}$$

where $Y_{i,d}$ represents attractiveness as measured by the invitation and offer probabilities and wages offered, respectively. κ_d is an intercept and α_d^C and α_d^N are vectors, denoting the effects of beliefs about cognitive and non-cognitive ability. Similarly, α_d^E and α_d^{SES} are scalar parameters for the effect of beliefs about expertise acquired at university and socio-economic background. Moreover, α_d^U is a vector denoting the effect of several unobserved factors ($U_{i,d}$). Finally, ϵ_i denotes an error term that is independent of the mechanisms and pre-determined variables.

Random assignment of résumé characteristics in terms of degree finalization ensures that the treatment effects on attractiveness and belief mechanisms are easily computed, as shown in Figures 2.1 and 2.2. The following decomposition will now assess the relative importance of each belief mechanism on all of the significant differences in candidate attractiveness (as displayed by Figure 2.1), thus bringing our results full circle. We make two assumptions: first, we assume that the impact of each trait on labor market outcomes is the same across educational groups, i.e. $\alpha_0^C = \alpha_1^C$, $\alpha_0^N = \alpha_1^N$ etc.; and second, we assume that the unobserved traits (U) are statistically independent from observed belief mechanisms (C, N, E, and SES) conditional on the random assignment of résumés.[13] Prior research (e.g. Heckman et al., 2013) shows that under these assumptions the effect of belief mechanisms can be decomposed into:

$$\begin{aligned} \underbrace{\mathbb{E}[Y_1 - Y_0]}_{\text{Attractiveness}} &= \underbrace{\tau_1 - \tau_0}_{\text{Unexplained}} + \underbrace{\alpha^C\ \mathbb{E}[C_1 - C_0]}_{\text{Cognitive}} + \underbrace{\alpha^N\ \mathbb{E}[N_1 - N_0]}_{\text{Noncognitive}} + \\ &\quad \underbrace{\alpha^E\ \mathbb{E}[E_1 - E_0]}_{\text{Expertise}} + \underbrace{\alpha^{SES}\ \mathbb{E}[SES_1 - SES_0]}_{\text{SES}}, \end{aligned} \tag{2.3}$$

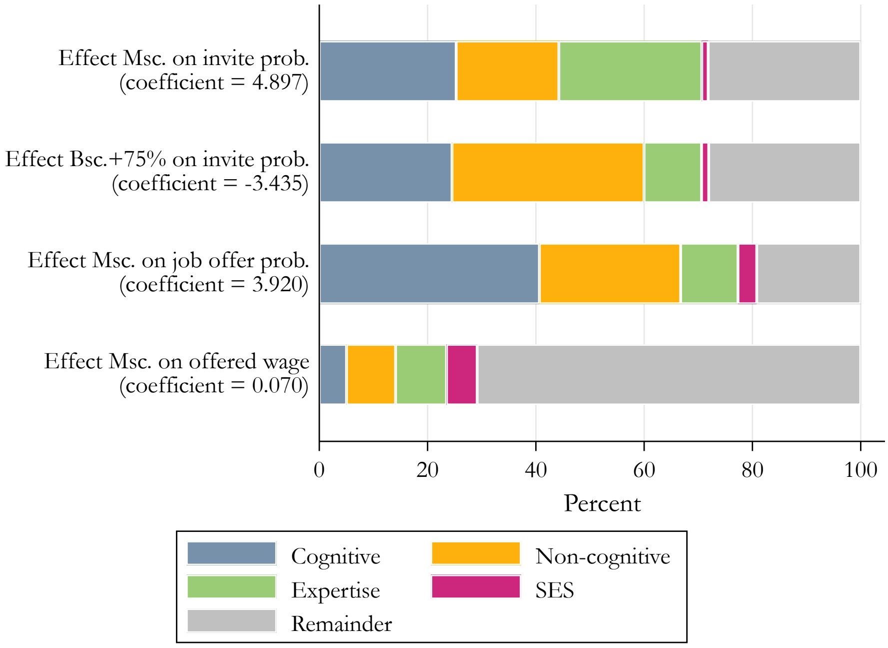
where $\tau_d = \kappa_d + \sum_{j \in J_U} \alpha_d^j\ \mathbb{E}[U_d^j]$, such that $\tau_1 - \tau_0$ captures the contribution of treatment-induced changes in a number of J_U unmeasured mechanism variables.

The results of this decomposition analysis are displayed graphically in Figure 2.3, with the traits being grouped together as cognitive ability, non-cognitive ability, expertise

[13] The first assumption can be relaxed by allowing for different parameters or including interaction terms, whereby doing so yields very similar results.

and SES for readability.[14]

Figure 2.3: Decomposition of differences in candidate attractiveness



Notes: The figure shows the decomposition of the significant differences in candidate attractiveness (see the effect sizes in brackets). See equation 2.3 for details on the decomposition. The traits are grouped in four categories: cognitive skills (trainability and IQ), non-cognitive skills (perseverance, commitment, conscientiousness and emotional stability), expertise and SES. The bars represent how much of the difference between the Bsc. scenario versus the Msc. degree scenario (bars 1, 3, 4) and the Bsc.+75% scenario (bar 2) can be explained by the traits. The remainder reflects the sum of the unexplained part and the negative coefficients (see table 2.E8).

The figure shows that for the probabilities of both inviting a candidate and offering them a job, the included traits can explain up to 75% of the significant differences discussed in section 2.3. Subject matter expertise, cognitive ability and non-cognitive ability all explain a significant share of the differences between the Bsc. scenario and the Msc. and Bsc.+75% scenarios. On the contrary, the effect of SES is insignificant, indicating that candidate background is a proxy for productive traits, being used at best as a means for stereotypical assessment in the absence of direct evidence of productivity or expertise.[15] Depending on the precise difference in candidate attractiveness, the relative importance of the traits slightly differs. Differences in perceived expertise offer an important explanation why master degree holders are more likely to be invited to an interview. The reduced likelihood of being invited to a job interview after leaving

[14] Appendix table 2.E8 shows all separate coefficients and t-statistics. As few coefficients have a (insignificant) negative sign, the "remainder" in Figure 2.3 reflects the sum of the unexplained part and the negative coefficients. This practice eases the interpretation of the figure, while the difference is negligible due to the small effect sizes of the negative coefficients.

[15] There is a substantial unconditional positive relationship between perceived SES and perceived (non-)cognitive abilities and expertise, as displayed in appendix figure 2.E1.

university with only 75% of the course work completed, is mainly explained by differences in perceived non-cognitive abilities. Contrary to this, the attractiveness of Msc. candidates with respect to the job offering probability can be mostly explained by differences in ascribed cognitive ability. The importance of acquired expertise is thus in line with human capital accumulation theories, while the finding that inherent cognitive and non-cognitive abilities of an applicant matter for the perception of candidates with an incomplete degree is in line with signaling theory. For all three cases, the difference in invitation and offering probabilities remaining after controlling for candidates' traits – i.e. the unexplained difference – is statistically insignificant.

For the difference in log wages offered between bachelor and master degree holders i.e., in the final step of the hiring process – the picture is different. Here, a larger part of the difference remains unexplained, and none of the traits displays a statistically significant effect on the wage difference. A potential reason for this finding could be that the wage offered is tied mostly to education and previous experience, which leaves less room for interpretation. Alternatively, as wages were assessed conditional on the decision to hire the applicant, employers might no longer take the perceived skills into account at this stage, knowing that the wage offered is based on a positive decision to hire the applicant, which must have been driven by a successful interview, i.e., a favorable update of potentially adverse résumé-driven perceptions.

2.3.4 Heterogeneities And Robustness Checks

To assess the generalizability and robustness of these results, we conduct several additional analyses. First, we explore the importance of grades obtained during (unfinished) Msc. studies for attractiveness across the different educational scenarios. Existing research shows that a better GPA can result in higher (immediate) wage returns (Hansen et al., 2021). However, in our context this may not necessarily be true, given that leaving university while having a high GPA may be perceived as an even worse signal of non-cognitive skills. We find that having a higher GPA is advantageous for Bsc.+25% and Msc. applicants, while for Bsc.+75% applicants the opposite is true. However, we lack statistical power to further investigate these patterns (see table 2.E4 in the appendix).

Two other aspects that may create heterogeneities in the attractiveness of an educational degree are the type of job and size of a firm. In our setup, employers are hiring for a job in either controlling or project management. Since controlling requires more course-specific knowledge, additional study credits might matter more for controlling jobs. Similarly, larger companies may rely more on degrees, as they have more streamlined hiring processes. However, the results neither display significant differences in the return to study credits between the two types jobs nor by firm size (see tables 2.E5 and 2.E6 in the appendix).

Further, employer characteristics may matter for their assessment of candidates with varying rates of educational degree completion. In particular, there might be important differences by employer expertise, as measured by the number of years for which an individual has worked in HR. We thus regress employers' beliefs of candidates' traits on an interaction term with categories of work experience. The results show that more experienced employers place less value on an Msc. degree when assessing a candidate's level of cognitive ability (see table 2.E7 in the appendix). Hence, it seems that more experienced employers are less likely to downward shift their beliefs about cognitive abilities when a candidate did not finish the degree.

Next, we test the sensitivity of our findings by re-running our analyses while imposing alternative specifications. First, we check whether loosening or tightening our sample restrictions alters our results. We thus relax the wage restrictions and include all respondents in one specification, whereas in another we add the requirement that respondents spent more than seven minutes answering the survey. Second, we test whether the results are driven by a company's wage-setting policy. For this purpose, we drop respondents whose company has a hiring rule in place that favors master degree holders. Third, we investigate the influence of the Covid-19 pandemic, by splitting the sample between below- versus above-median beliefs on how much the pandemic changed the hiring requirements of a respective company.
We performed these alternative specifications for all three main analyses (see appendix tables 2.E9, 2.E10, 2.E11). Findings show that the statistical significance and economic interpretation of our main estimates remain robust throughout all specifications.

2.4 Conclusion

This study opens the blackbox of why higher education is desirable and perceived as a signal about future productivity to employers. After randomly varying master degree attainment on candidate résumés' we elicit candidate attractiveness and – more importantly – employer beliefs about eight productivity-related applicant characteristics comprising socio-economic status, cognitive and non-cognitive abilities, and subject matter expertise, all of which have been shown to relate both to the psychic cost of education and labor market productivity.

We thus provide novel evidence on why educated individuals are perceived as valuable by employers. In a first instance, we confirm that a master degree increases candidate attractiveness. All else being equal, candidates who have completed a master's degree are 4.5 percentage points (7%) more likely to be offered a job interview than candidates with a bachelor's degree. The size of this effect is roughly similar to the effect of having a migration background (Weichselbaumer, 2020). Moreover, degree completion increases the likelihood of receiving an offer by 3.6 percentage points (5%) and earnings potentials by 4.8%. Having completed nearly all coursework but not having obtained a degree results in a reduced invitation probability of 2.3 percentage points (3.4%). While these results confirm that master graduates are more desirable to employers than candidates with a bachelor degree only, they are not directly comparable to the results from audit studies, nor to education premia as reported in the literature. First, employers looked at each résumé separately and in the absence of competition from other candidates, i.e., as would typically be the case in a hiring situation. Second, wages were reported *conditional* on having successfully completed a job interview. Since a job interview, however, typically yields information above and beyond what can be learned from reading a candidate's résumé, the variation on offered wages is likely to be higher in reality.

In a second instance we have shown that having completed a master's degree improves employers' perceptions of a candidate's cognitive and non-cognitive traits and expertise by around 20% of a standard deviation. On the contrary, not finishing a degree significantly reduces employers' perceptions about non-cognitive traits by up to 35% of a standard deviation. A decomposition analysis unveils that these characteristics in sum are relevant and important drivers of degree effects, as they explain up to 75% of the observed variation in applicant attractiveness as indicated by interview invitation and employment probabilities. At the same time, while employer beliefs about cognitive and non-cognitive traits as well as expertise matter in the interview and hiring phase, they prove much less important when it comes to explaining wage differentials conditional on hiring, suggesting that the observed wage premium among graduates is mostly driven by opportunities to take on more attractive jobs.

Our findings hold broad significance regarding the importance of higher education credentials as a signal for potential employers. Previous studies on the importance of educational credentials for employee attractiveness mostly stems from observational data with measures of actual academic ability or exogenous variation in education curricular or years of schooling. Our results complement these findings by providing first causal evidence that completing education positively shapes employer beliefs about cognitive and non-cognitive traits, while dropping out is perceived as a negative signal about non-cognitive traits. We also show that expertise in the form of subject matter knowledge is only valued by employers when it comes in combination with obtaining a degree, indicating that the human capital part of education and degree signaling complement each other when it comes to employer perceptions.

Overall, our findings indicate that employers react strongly to educational credentials at the initial screening stage and that they adapt their beliefs about unobservable productive traits in ways that we show are consistent evidence about the actual characteristics of candidates with these credentials. Our results further suggest that, at least for business majors, more than half of the educational attractiveness effect of a master's degree is not driven by the acquired knowledge or expertise, but by ascribed intelligence and personality traits such as perseverance, commitment, conscientiousness, and emotional stability. To the extent that more attractive employers might be able to selectively choose employees along these lines, this has fundamental implications for relative firm productivity, employee incentivization, and management practices.

Appendix 2.A Questionnaire

2.A.1 Intro

Thank you for participating in our survey!

By participating, you support a research project about current labor market and hiring processes, which is conducted under the direction of XXX.

Your answers will be treated confidentially and in accordance with European data protection regulations. The results of the survey will be presented in aggregated form only.

Answering the questionnaire takes about 10 minutes.

2.A.2 General Questions

Are you currently employed?

- Yes
- No

Which of the following areas do you work in?

- Human resources development
- Personnel recruitment
- Personnel strategy/personnel planning
- Labor law
- Compensation management
- Other personnel areas
- Other areas except personnel

How many employees does your company have in Germany?

- Fewer than 10 employees

- 10 to 49
- 50 - 100
- 101 - 500
- 501 - 1000
- 1,001 - 2,000
- More than 2,000

For which of the following areas do you recruit employees in your company?

- Commercial
- Technical
- IT
- Natural sciences
- Humanities
- Other

2.A.3 Main Part

In the following, the main part of the survey begins.

Your expert opinion is of great importance for our research project. We would therefore like to ask you to give your answers as if you were conducting a real hiring process.

Imagine you are looking for a new employee for an entry-level position in your company in [controlling/project management].

In the following, we will ask you about your assessments of three applicants.

For this purpose, please assume that all of the information that we do not give you about the applicant profiles is identical between the applicants. For better comparability, we present the applicant data in a uniform and simplified form.

In the first part of the survey, we ask you to give your assessment of the candidate for each of the three candidate profiles.

By clicking on the Next button, you will see the first candidate profile.

Here, the résumé of applicant 1, 2 or 3 is shown (see appendix 2.B for examples and details).

Imagine that there is an entry-level position to be filled in the area of [controlling/project management].

How do you rate the likelihood that you would invite [name] to an interview for the described entry-level position in your company?

0% 100%

Suppose that [name] was invited for an interview. Assuming a good performance and a positive impression, how likely do you think it is that [name] would receive an offer for the described entry-level position in your company?

0% 100%

How would you rate the likelihood that [name] would accept an offer from your company?

0% 100%

What salary (annual salary in Euro for a full-time position, excluding special benefits such as bonuses) would you offer [name] for the described entry-level position in your company?

____

Based on the information from the above résumé, how would you rate [name] compared to other graduates of a business degree program in terms of the following traits. Negative numbers indicate below-average and positive numbers above-average skills.

Trainability

Much lower learning ability (-100) / Much higher learning ability (+100)

-100 +100

Intelligence

Much lower intelligence (-100) / Much higher intelligence (+100)

-100 +100

Expertise

Much lower study-specific knowledge (-100) / Much higher study-specific knowledge (+100)

-100 +100

Perseverance

Much lower perseverance (-100) / Much higher perseverance (+100)

-100 +100

Commitment

Much lower commitment (-100) / Much higher commitment (+100)

-100 +100

Conscientiousness

Much lower conscientiousness (-100) / Much higher conscientiousness (+100)

-100 +100

Emotional stability

Much lower emotional stability (-100) / Much higher emotional stability (+100)

-100 +100

Social origin

Much less privileged social origin (-100) / Much more privileged social origin (+100)

-100 +100

2.A.4 Stated Preferences

Imagine receiving an application from someone who left college during her last semester, i.e., just before earning a master's degree (but without a degree). Please write in a few words what you associate with this?

Do you prefer an applicant with a master's degree over an applicant with a bachelor's degree in the selection process for a controlling/project management position?

- Yes, a master's degree is used for pre-selection.
- Yes, there is an internal company rule that makes a master's degree a mandatory requirement in the hiring process.
- Yes, because:
- No.

What do you associate with a degree? In your view, is it more a proof of learned study content or rather a signal of certain character traits?

- Exclusively character traits
- Rather character traits
- Both equally
- Rather study content
- Exclusively study content

What do you associate with dropping out of university? Do you see it more as a lack of proof of learned study content or more as a negative signal about a candidate's character traits?

- Exclusively character traits
- Rather character traits
- Both equally
- Rather study content
- Exclusively study content

2.A.5 Background Information

Finally, we have a few statistical questions about you and your company so that we can better evaluate your answers. This information will also be treated confidentially and will only be evaluated anonymously.

How much work experience do you have?

- 0 - 5 years
- 6 - 15 years
- 16 - 25 years
- 26 - 35 years
- More than 35 years

How many applications do you receive on average for a typical entry-level controlling/project management position in your company?

What is the average salary (annual gross amount in Euro excluding individual bonuses or benefits) for an entry-level position in controlling/project management in your company with a business administration background?

Is it common practice for company to pay a performance-related bonus for an entry-level position in controlling/project management?

- Yes
- No

What is the relative share of variable salary/bonus of total salary?

Very low share of variable salary (0%) / Very high share of variable salary (100%)

0% 100%

How much leeway do you have for salary negotiations (base salary and bonuses) with applicants who receive a job offer for an entry-level position in controlling/project management in your company?

There is no room for negotiation (0%) / Free negotiations possible (100%)

0% 100%

Have selection criteria for applicants in your company changed as a result of Covid-19?

No differences compared to before Covid (0%) / Large differences compared to before Covid (100%)

0% 100%

Do you have any comments regarding this survey? Is there anything special that we should know about the hiring process at your company?

Thank you for your participation and support of our research project!

Appendix 2.B Applicant Profiles

2.B.1 Variation Of Résumé Components

For each hypothetical candidate, a one-page résumé is presented to the HR manager, comprising twenty components (see table 2.B1 for all items). The different components are randomized at the applicant level for each HR manager separately, with the exception of all time-related variables (indicated with an asterisk in table 2.B1). The résumé items that comprise a date (e.g. information on education obtained) depend on the randomly chosen accomplished university education to create a coherent and synchronized picture in one résumé. Please see below a detailed description of the variation that we include in the résumés, with the exception of the variation related to education, which is described in section 2.2.2.

Demographics - The gender of the applicant is indicated by the name on the résumé, with randomly half of the names being male and half being female. To avoid associations with socio-economic status, we make use of common German first and last names for the respective age cohort. In addition, there is a slight variation in age of the applicants, which is indicated by the birth date on the résumé. There is a maximum two-year age difference between applicants, corresponding to the different lengths of educational pathways and internship lengths. Hence, all time-related variables are adjusted to avoid gaps in the résumé. This implies – for instance – that an applicant who only obtained a bachelor degree is always slightly younger than the applicant with a master degree. Although this implies that we cannot disentangle a potential age effect from the degree effect, we believe that this résumé design is suitable for several reasons. First of all, it is the most realistic set-up, where bachelor graduates are on average younger than master graduates. Second, previous research has not shown age effects in terms of the desirability of university graduates (Piopiunik et al., 2020).

Other variation - Other variation in the résumés is related to the applicant's language skills, free-time activities, IT skills and secondary school grade. With respect to the latter, we again looked at the actual distribution of high-school GPAs and vary grades corresponding to the 10th, 50th and 90th percentile. The free-time activities are gender neutral and comprise one sport and one other activity such as drawing or playing an instrument. With respect to languages skills, all applicants are German natives and speak English fluently. Besides, they have either basic or good skills in Spanish or French as a third language. Similarly, for IT skills, each applicant is excellent in Microsoft Office and has basic knowledge of one other statistical program. It is important to note, that these individual characteristics are not the main focus of this study but rather serve the purpose of making the résumés as realistic as possible.

Table 2.B1: Overview résumé components

Component	Options			
Gender	Female	Male		
First name (male)	Lukas	Maximilian		
First name (female)	Johanna	Lena		
Last name	Schneider	Weber	Becker	Fischer
Date of birth∗	3.9.1999	12.7.1998	24.6.1997	11.8.1997
High-school degree∗	2018	2017	2016	2016
High-school GPA	1.6	2.4	3.3	
University education∗	bachelor degree	bachelor degree & master studies (30 ects)	bachelor degree & master studies (90 ects)	bachelor degree & master degree
Institution	University of Cologne	University of Frankfurt	University of Munich	
Bachelor GPA	1.5	2.3	3.2	
Master GPA	1.4	2.0	2.7	
Bachelor start & end date∗	Start: 2018; End: July 2021	Start: 2017; End: July 2020	Start: 2016; End: September 2019	Start: 2016; End: August 2019
Master start & end date∗	n.a.	Start: 2020; End: 2021	Start: 2019; End: 2021	Start: 2019; End: September 2021
Internship area	Sales	Project management	Auditing	
Internship employer	Windmoeller & Hoelscher, Lengerich	FACT, Muenster	MVI Proplant, Wolfsburg	
Internship year∗	2021	2020	2019	2019
Internship length	3 months	5 months	9 months	
Languages	German (native), English (fluent), Spanish (good)	German (native), English (fluent), French (basic)	German (native), English (fluent), Spanish (basic)	
Personal interests	Biking, choir	Swimming, drawing	Running, guitar	
IT skills	Microsoft Office (excellent), R (basic)	Microsoft Office (excellent), SPSS (basic)	Microsoft Office (excellent), Stata (basic)	

Notes: This table shows all components that are randomized on the résumés. The components marked by an ∗ are fixed within an applicant profile to ensure that there are no gaps in the timeline.

2.B.2 Examples Of Résumés

Figure 2.B1: Example of applicant with a Bsc. degree

Maximilian Becker
geb. 03/09/99

SCHULBILDUNG

Allgemeine Hochschulreife	2018
Abiturnote 3.3	

STUDIENLEISTUNGEN

Bachelorabschluss Betriebswirtschaftslehre	
180/180 ECTS erreicht	2018 - 2021
Goethe-Universität Frankfurt	
Abschlussnote: 3.2	
Abschlussdatum: Juli 2021	

ARBEITSERFAHRUNG

Praktikum Projektmanagement (9 Monate)	2021
FACT, Münster	

SPRACHEN

Deutsch: Muttersprache
Englisch: fließend
Französisch: Grundkenntnisse

IT KENNTNISSE

Microsoft Office (sehr gut), Stata (Grundkenntnisse)

PERSÖNLICHE INTERESSEN

Schwimmen, Zeichnen

Notes: Figure 2.B1 shows an example of a résumé of an hypothetical applicant with a Bsc. degree.

Figure 2.B2: Example of applicant with a Msc. degree

Maximilian Fischer

geb. 11/08/97

SCHULBILDUNG

Allgemeine Hochschulreife — **2016**

Abiturnote 3.3

STUDIENLEISTUNGEN

Masterabschluss Betriebswirtschaftslehre

120/120 ECTS erreicht — **2019 – 2021**

Goethe-Universität Frankfurt

Abschlussnote: 1.4

Abschlussdatum: September 2021

Bachelorabschluss Betriebswirtschaftslehre

180/180 ECTS erreicht — **2016 – 2019**

LMU München

Abschlussnote: 1.5

Abschlussdatum: August 2019

ARBEITSERFAHRUNG

Praktikum Sales (9 Monate) — **2019**

FACT, Münster

SPRACHEN

Deutsch: Muttersprache

Englisch: fließend

Spanisch: gut

IT KENNTNISSE

Microsoft Office (sehr gut), SPSS (Grundkenntnisse)

PERSÖNLICHE INTERESSEN

Laufen, Gitarre

Notes: Figure 2.B2 shows an example of a résumé of an hypothetical applicant with a Msc. degree.

Figure 2.B3: Example of applicant with a Bsc.+25% degree

Lukas Schneider
geb. 12/07/98

SCHULBILDUNG

Allgemeine Hochschulreife 2017
Abiturnote 2.4

STUDIENLEISTUNGEN

Bachelorabschluss Betriebswirtschaftslehre
180/180 ECTS erreicht 2017 – 2020
LMU München
Abschlussnote: 1.5
Abschlussdatum: Juli 2020

Sonstige Leistungen:

Masterstudium Betriebswirtschaftslehre (nicht abgeschlossen)
Erstes Semester (30/120 ECTS) absolviert 2020 – 2021
LMU München
Durchschnittsnote: 2.7
Exmatrikulation: 2021

ARBEITSERFAHRUNG

Praktikum Controlling (5 Monate) 2020
Windmöller & Hölscher, Lengerich

SPRACHEN

Deutsch: Muttersprache
Englisch: fließend
Spanisch: Grundkenntnisse

IT KENNTNISSE

Microsoft Office (sehr gut), R (Grundkenntnisse)

PERSÖNLICHE INTERESSEN

Laufen, Gitarre

Notes: Figure 2.B3 shows an example of a résumé of an hypothetical applicant who dropped out after attaining 25% (i.e. 30 ECTS) of a Msc. degree.

Figure 2.B4: Example of applicant with a Bsc.+75% degree

Lena Weber
geb. 24/06/97

SCHULBILDUNG

Allgemeine Hochschulreife 2016
Abiturnote 1.6

STUDIENLEISTUNGEN

Bachelorabschluss Betriebswirtschaftslehre
180/180 ECTS erreicht 2016 – 2019
Universität zu Köln
Abschlussnote: 2.3
Abschlussdatum: September 2019

Sonstige Leistungen:

Masterstudium Betriebswirtschaftslehre (nicht abgeschlossen)
Kursphase (90/120 ECTS) absolviert 2019 – 2021
Goethe-Universität Frankfurt
Durchschnittsnote: 1.4
Exmatrikulation: 2021

ARBEITSERFAHRUNG

Praktikum Sales (3 Monate) 2019
MVI Proplant, Wolfsburg

SPRACHEN

Deutsch: Muttersprache
Englisch: fließend
Spanisch: gut

IT KENNTNISSE

Microsoft Office (sehr gut), SPSS (Grundkenntnisse)

PERSÖNLICHE INTERESSEN

Rennrad, Chor

Notes: Figure 2.B4 shows an example of a résumé of an hypothetical applicant who dropped out after attaining 75% (i.e. 90 ECTS) of a Msc. degree.

Appendix 2.C Data Cleaning Procedure

When designing the survey, we bounded most answer ranges to fit feasible possibilities. For example, for the interview and hiring probabilities, it is only possible to fill in values between 0 and 100. When asking respondents the wage that they would offer the applicant, the following message shows up when they fill in an amount below €10,000 or above €99,000: please check your entry and confirm it by clicking next.[16] However, it is possible for respondents to ignore the message and fill in any amount that they deem appropriate.

As this may create noise in the answers that we observe, we clean the wage variable as follows. First, we check whether the wages that a respondent filled in are consistent across applicants. If the wage responses are not consistent with each other – in particular its value being more than twice as much in one scenario than the next – the observation is dropped. This is important for our second step, where we check whether respondents may have misunderstood the question and filled in the wage per month instead of year. For values between €1,400 and €12,500, we assume they meant monthly wages, in which case the value is multiplied by 12 (roughly 7% of the sample). Finally, we drop all respondents whose yearly wage value is below €17,000 or above €150,000. The lower bound of €17,000 originates from the minimum full-time salary mandated by German law, while the upper bound comes from an online search of the highest starting salaries in Germany.

Overall, 484 employers completed the survey. After the cleaning procedure described above, we are left with 433 respondents who answered questions about 1,299 applicants.

[16] Original text: *Bitte überprüfen Sie Ihre Eingabe und bestätigen Sie diese mit dem Weiter-Button.*

Appendix 2.D Traits Of Actual Candidates

We assess if human resource managers hold correct beliefs about the characteristics of dropout and master students as compared to bachelor students on hands of actual survey data containing information on degree completion as well as measures of conscientiousness, emotional stability, IQ, and socio-economic status. To this end we use data from the German student study 'Fachkraft 2030".[17] The original data, containing measures of personality traits, IQ and SES were collected in September 2014 and March 2015. A follow-up survey to assess final educational outcomes of these students was collected in January 2023. The data contain around 450 observations for parental socio-economic status, and around 390 observations for the measures of personality traits and IQ. 78% of the sample have completed a bachelor's degree, 13% have obtained master's degree, and 9% have dropped out from their master studies after having obtained a bachelor degree.

2.D.1 Measures

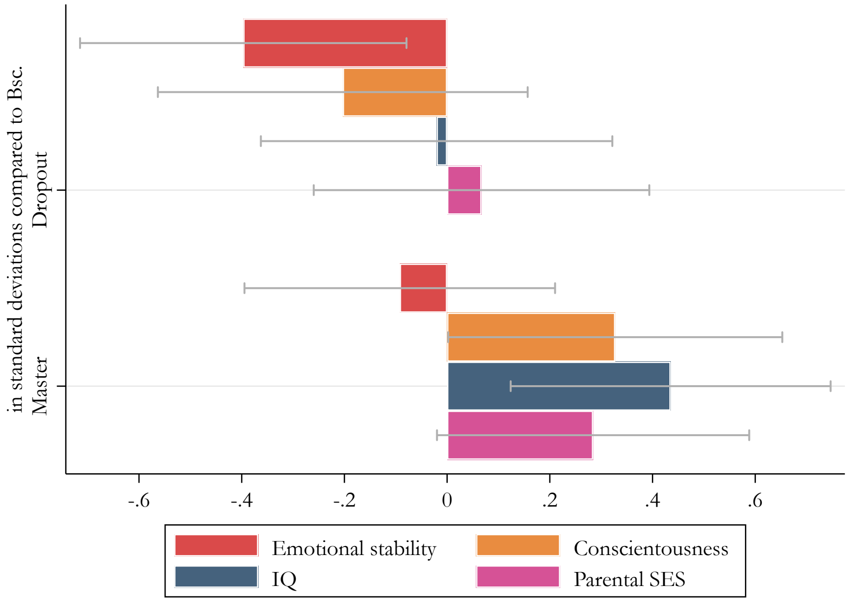
Students' conscientiousness and emotional stability were assessed using the respective parts of the 50 item IPIP test (Goldberg et al., 2006). IQ was measured based on ten items from a Raven-type matrix IQ test (Raven and Court, 1998). For socio-economic status we construct a score combining information on maternal and paternal levels of education, as well as a student's migrant status. Importantly, all measures were collected in 2014 and 2015, i.e., while students were enrolled, such that they are unaffected by later job performance or career trajectories.

2.D.2 Results

The bars displayed in figure 2.D1 display standardized differences in trait scores among dropouts and master degree holders compared to bachelor degree holders and are thus directly comparable to the results displayed in main figure 2.2. Qualitatively, the differences in actual traits observed in figure 2.D1 are surprisingly similar to the ones displayed in figure 2.2. Only emotional stability is no higher among master degree holders when compared to bachelor degree holders. Overall, however, the findings in this section indicate that employers hold on average accurate beliefs about the characteristics of the applicants.

[17] See Seegers, Philipp and Bergerhoff, Jan and Hartmann, Stephan and Knappe, Anne (2016) for more information.

Figure 2.D1: Actual trait differences by educational scenario



Notes: The figure displays standardized differences in trait scores among dropouts and master degree holders compared to bachelor degree holders, with all scores being standardized with respect the bachelor distributions. The gray bars indicate 95% confidence intervals.

Appendix 2.E Additional Figures And Tables

Figure 2.E1: Correlations between employer beliefs

Notes: The figure shows the correlation coefficients between each of the traits. The darker the color, the higher the correlation coefficient.

Table 2.E1: Descriptive statistics of employers

	Mean	St.dev.
Years of experience (sample share)		
0-5	0.12	0.32
6-15	0.45	0.50
16-25	0.26	0.44
26-35	0.15	0.36
35+	0.03	0.17
Firm size (sample share)		
10 - 49	0.09	0.28
50 - 100	0.10	0.30
101 - 500	0.33	0.47
501 - 1000	0.23	0.42
1001 - 2000	0.12	0.33
2000+	0.13	0.34
Average number of applicants	42.79	65.56
Average company starting wage (in Euro)	42740.36	17114.47
Bonus paid on top of base salary (sample share)	0.37	0.48
Change in hiring due to Covid-19 (0-100)	34.49	30.74
Observations	433	

Notes: The table shows the sample mean and standard deviation for several characteristics of HR managers and the firms for which they work.

Table 2.E2: Employment outcomes by all résumé items

	Base Bsc.		
	(1)	(2)	(3)
	Prob. to invite	Prob. to offer	Log Wage
Bsc.+25%	-0.492	0.361	-0.001
	(1.238)	(1.170)	(0.008)
Bsc.+75%	-2.302*	-0.831	0.000
	(1.267)	(1.241)	(0.008)
Msc	4.486***	3.601***	0.048***
	(1.169)	(1.179)	(0.008)
Interns. type fits	1.593*	1.497*	0.005
	(0.879)	(0.819)	(0.006)
Male	-1.041	-0.225	0.005
	(0.895)	(0.871)	(0.006)
High-school grade	2.969***	2.691***	0.012***
	(0.603)	(0.581)	(0.004)
GPA highest degree	3.569***	3.282***	0.014***
	(0.743)	(0.679)	(0.005)
Uni Munich (Bsc.)	1.510	0.131	0.005
	(1.016)	(0.921)	(0.006)
Uni Cologne (Bsc.)	-0.424	-1.629*	-0.001
	(0.986)	(0.982)	(0.007)
Uni Munich (Msc.)	1.249	2.112**	0.004
	(0.997)	(0.955)	(0.007)
Uni Cologne (Msc.)	0.201	0.005	-0.005
	(1.081)	(1.006)	(0.007)
5 months interns.	-0.033	0.760	0.006
	(1.023)	(1.003)	(0.007)
9 month interns.	0.680	0.796	0.012*
	(1.009)	(0.955)	(0.007)
Firm II	-0.143	-0.202	0.001
	(0.991)	(0.977)	(0.006)
Firm III	-0.411	-0.954	0.002
	(1.041)	(0.948)	(0.007)
Spanish basic	-0.751	0.069	0.009
	(1.032)	(0.959)	(0.007)
Spanish good	1.326	1.052	0.017**
	(1.004)	(0.957)	(0.007)
SPSS skills	0.919	0.550	0.008
	(0.958)	(0.925)	(0.007)
Stata skills	0.542	0.946	0.007
	(1.018)	(0.945)	(0.006)
Personal interests II	0.384	0.780	-0.004
	(1.040)	(0.976)	(0.007)
Personal interests III	1.827*	0.664	0.000
	(1.011)	(0.943)	(0.007)
N	1299	1299	1299
Ind. FE	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs. White robust standard errors clustered at the respondent level are displayed in parentheses. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. See table 2.B1 for the default category for each of the variables. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 2.E3: T-tests of employer beliefs by educational scenario

	Bsc.+25%		Bsc.+75%		Msc.	
	Dif. mean	P-value p	Dif. mean	P-value p	Dif. mean	P-value p
Perserverance	-0.346	0.000	-0.363	0.000	0.236	0.003
Commitment	-0.244	0.004	-0.302	0.000	0.186	0.015
Conscientiousness	-0.147	0.076	-0.244	0.004	0.170	0.030
Emotional stability	-0.175	0.039	-0.190	0.020	0.215	0.005
Trainability	-0.045	0.570	-0.112	0.152	0.243	0.001
IQ	-0.067	0.388	-0.095	0.220	0.194	0.013
Expertise	-0.049	0.541	-0.099	0.209	0.292	0.000
SES	0.002	0.982	-0.091	0.245	0.131	0.105

Notes: The table displays standardized differences in trait scores of the Bsc. +25%, Bsc. +75%, and Msc. scenarios compared to the Bsc. scenario, with all scores being standardized with respect the Bsc. distributions. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E4: Heterogeneous effects on employment outcomes by GPA

	(1)	(2)	(3)
	Prob. to invite	Prob. to offer	Log Wage
Bsc.+25%	0.411	0.437	0.003
	(1.862)	(1.782)	(0.014)
Bsc.+75%	-3.937**	-2.507	-0.003
	(1.852)	(1.694)	(0.011)
Msc	5.937***	5.817***	0.060***
	(1.862)	(1.885)	(0.012)
Bsc.+25% * High GPA	0.155	1.776	0.013
	(2.542)	(2.366)	(0.018)
Bsc.+25% * Low GPA	-4.091	-3.471	-0.036**
	(2.595)	(2.316)	(0.018)
Bsc.+75% * High GPA	1.877	1.393	0.006
	(2.663)	(2.406)	(0.017)
Bsc.+75% * Low GPA	2.706	3.153	-0.002
	(2.567)	(2.366)	(0.017)
Msc. * High GPA	1.848	-1.115	-0.021
	(2.444)	(2.424)	(0.016)
Msc. * Low GPA	-3.783	-3.318	-0.007
	(2.569)	(2.413)	(0.015)
N	1299	1299	1299
Ind. FE	Yes	Yes	Yes
Controls (other)	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. High GPA is defined as the top 10th percentile of the grade distribution, while low GPA is set at the 90th percentile, both compared to the median GPA. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E5: Heterogeneous effects on employment outcomes by job profile

	(1) Prob. to invite	(2) Prob. to offer	(3) Log Wage
Bsc.+25%	-0.907	-0.553	-0.012
	(1.782)	(1.585)	(0.013)
Bsc.+75%	-0.655	-0.723	0.006
	(1.637)	(1.608)	(0.012)
Msc	4.693***	3.796**	0.042***
	(1.630)	(1.625)	(0.011)
Bsc.+25%*Controlling	0.986	1.913	0.023
	(2.508)	(2.345)	(0.017)
Bsc.+75%*Controlling	-3.129	-0.196	-0.011
	(2.575)	(2.460)	(0.017)
Msc.*Controlling	-0.402	-0.429	0.011
	(2.335)	(2.304)	(0.015)
N	1299	1299	1299
Ind. FE	Yes	Yes	Yes
Controls (other)	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. The "controlling" dummy indicates whether the hypothesized vacancy is within the area of controlling or project management. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E6: Heterogeneous effects on employment outcomes by firm size

	(1) Prob. to invite	(2) Prob. to offer	(3) Log Wage
Bsc.+25%	-1.306	-1.058	-0.005
	(1.461)	(1.473)	(0.011)
Bsc.+75%	-1.832	-0.662	0.002
	(1.577)	(1.575)	(0.011)
Msc	3.664**	2.103	0.042***
	(1.460)	(1.516)	(0.010)
Bsc.+25%*Firm size	0.002	0.003	0.000
	(0.002)	(0.002)	(0.000)
Bsc.+75%*Firm size	-0.001	-0.000	-0.000
	(0.002)	(0.002)	(0.000)
Msc.*Firm size	0.002	0.003	0.000
	(0.002)	(0.002)	(0.000)
N	1299	1299	1299
Ind. FE	Yes	Yes	Yes
Controls (other)	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. Firm sizes measures the number of employees at the company for which the employer works. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E7: Heterogeneous effects on employer beliefs by experience

	(1) Perseverance	(2) Commitment	(3) Conscien-tiousness	(4) Emotional stability	(5) Trainability	(6) Intelligence	(7) Expertise	(8) SES
Bsc.+25%	-0.209	-0.032	0.028	-0.246	0.280	0.217	0.180	0.191
	(0.216)	(0.171)	(0.201)	(0.209)	(0.213)	(0.193)	(0.188)	(0.182)
Bsc.+75%	-0.017	-0.140	-0.111	0.052	-0.051	0.109	0.112	0.098
	(0.209)	(0.203)	(0.199)	(0.174)	(0.196)	(0.172)	(0.221)	(0.157)
Msc	0.466**	0.404**	0.439**	0.219	0.460***	0.449***	0.338	0.255
	(0.185)	(0.198)	(0.185)	(0.184)	(0.158)	(0.146)	(0.210)	(0.168)
Bsc.+25%*Median-term experience	-0.189	-0.264	-0.134	0.079	-0.281	-0.315	-0.252	-0.211
	(0.240)	(0.198)	(0.222)	(0.233)	(0.229)	(0.212)	(0.208)	(0.198)
Bsc.+25%*Long-term experience	-0.030	-0.174	-0.235	0.127	-0.277	-0.238	-0.144	-0.180
	(0.251)	(0.201)	(0.225)	(0.235)	(0.232)	(0.212)	(0.207)	(0.197)
Bsc.+75%*Median-term experience	-0.268	-0.162	0.018	-0.271	0.167	-0.079	-0.193	-0.107
	(0.238)	(0.224)	(0.221)	(0.199)	(0.213)	(0.196)	(0.241)	(0.173)
Bsc.+75%*Long-term experience	-0.369	-0.176	-0.237	-0.242	-0.120	-0.268	-0.182	-0.270
	(0.241)	(0.234)	(0.229)	(0.202)	(0.221)	(0.196)	(0.240)	(0.178)
Msc.*Median-term experience	-0.280	-0.302	-0.274	-0.111	-0.234	-0.359**	-0.121	-0.089
	(0.214)	(0.221)	(0.210)	(0.208)	(0.183)	(0.172)	(0.231)	(0.183)
Msc.*Long-term experience	-0.231	-0.276	-0.372*	0.032	-0.313*	-0.356**	-0.082	-0.258
	(0.213)	(0.221)	(0.204)	(0.208)	(0.178)	(0.168)	(0.227)	(0.184)
N	1299	1299	1299	1299	1299	1299	1299	1299
Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls (other)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. Median-term experience is defined as having worked in HR for five to fifteen years, while long-term experience is having more than fifteen years' experience, whereby both are compared to having less than five years' experience. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E8: Decomposition of differences in candidate attractiveness

	Msc.			Bsc.+75%
	(1)	(2)	(3)	(4)
	Pr. to invite	Pr. to offer	Log wage	Pr. to invite
Difference with Bsc.	4.897**	3.920**	0.070**	-3.435*
	(1.524)	(1.468)	(0.023)	(1.561)
Explained	3.484***	3.111***	0.007	-2.445*
	(0.956)	(0.935)	(0.005)	(1.089)
Unexplained	1.413	0.809	0.063**	-0.991
	(1.221)	(1.171)	(0.024)	(1.186)
EXPLAINED				
Trainability	1.332**	1.101*	-0.009	-0.694
	(0.492)	(0.440)	(0.006)	(0.498)
Intelligence	-0.064	0.611	0.005	-0.170
	(0.222)	(0.331)	(0.005)	(0.169)
Expertise	1.324**	0.446	0.008	-0.375
	(0.483)	(0.363)	(0.006)	(0.314)
Perseverance	0.677	0.822*	0.002	-1.476**
	(0.365)	(0.380)	(0.005)	(0.505)
Commitment	0.053	0.149	-0.001	-0.000
	(0.204)	(0.220)	(0.003)	(0.289)
Conscientiousness	0.183	0.128	0.002	0.315
	(0.240)	(0.200)	(0.004)	(0.263)
Emotional stability	0.039	-0.000	0.005	-0.091
	(0.252)	(0.217)	(0.004)	(0.181)
Socioeconomic background	-0.059	-0.146	-0.005	0.047
	(0.120)	(0.135)	(0.004)	(0.089)
Observations	645	645	645	642

Notes: The table shows the coefficients of the decomposition of the significant differences in candidate attractiveness shown in columns 1, 3 and 5 of table 2.1. See equation 2.3 for details on the decomposition. The t statistics are displayed in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E9: Robustness of employment outcomes by educational scenario

	(1) Main	(2) All obs.	(3) Resp. time	(4) No rule	(5) Covid: low	(6) Covid: high
PANEL A: INVITE PROBABILITY						
Bsc.+25%	-0.492	-0.496	0.019	-0.534	-1.786	0.485
	(1.238)	(1.145)	(1.397)	(1.322)	(1.811)	(1.719)
Bsc.+75%	-2.302*	-2.072*	-1.112	-2.381*	-2.105	-2.200
	(1.267)	(1.171)	(1.388)	(1.337)	(2.039)	(1.496)
Msc	4.486***	3.755***	5.186***	4.203***	4.733***	4.567***
	(1.169)	(1.123)	(1.359)	(1.295)	(1.702)	(1.645)
N	1299	1449	906	1116	645	654
PANEL B: OFFER PROBABILITY						
Bsc.+25%	0.361	0.816	0.771	0.154	-0.467	1.006
	(1.170)	(1.097)	(1.347)	(1.246)	(1.755)	(1.567)
Bsc.+75%	-0.831	-0.669	-0.021	-1.221	-1.632	0.128
	(1.241)	(1.168)	(1.337)	(1.266)	(1.891)	(1.596)
Msc	3.601***	3.790***	4.657***	3.376***	5.129***	2.485
	(1.179)	(1.136)	(1.353)	(1.262)	(1.771)	(1.595)
N	1299	1449	906	1116	645	654
PANEL C: LOG WAGE						
Bsc.+25%	-0.001	-0.008	-0.004	0.002	-0.002	0.000
	(0.008)	(0.018)	(0.010)	(0.009)	(0.011)	(0.013)
Bsc.+75%	0.000	0.019	0.005	0.006	0.000	-0.000
	(0.008)	(0.017)	(0.009)	(0.009)	(0.010)	(0.013)
Msc	0.048***	0.074**	0.052***	0.051***	0.048***	0.048***
	(0.008)	(0.029)	(0.009)	(0.008)	(0.010)	(0.012)
N	1299	1449	906	1116	645	654
Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. Column 1 shows the main specification. Column 2 includes all observations. Column 3 excludes individuals with a response time less than seven minutes. Column 4 excludes individuals whose company has a wage-setting policy favoring master degree holders. Columns 5 and 6 split the sample by beliefs of how much Covid-19 changed hiring requirements. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E10: Robustness of employer beliefs by educational scenario

	(1) Main	(2) All obs.	(3) Resp. time	(4) No rule	(5) Covid: low	(6) Covid: high
Panel A: Perseverance						
Bsc.+25%	-0.302***	-0.262***	-0.303***	-0.345***	-0.551***	-0.051
	(0.075)	(0.071)	(0.085)	(0.079)	(0.117)	(0.088)
Bsc.+75%	-0.295***	-0.273***	-0.243***	-0.370***	-0.498***	-0.102
	(0.075)	(0.072)	(0.084)	(0.077)	(0.121)	(0.088)
Msc	0.233***	0.203***	0.270***	0.227***	0.319***	0.159*
	(0.068)	(0.066)	(0.083)	(0.072)	(0.099)	(0.093)
N	1299	1449	906	1116	645	654
Panel B: Commitment						
Bsc.+25%	-0.221***	-0.193***	-0.169**	-0.231***	-0.287***	-0.168*
	(0.066)	(0.063)	(0.076)	(0.068)	(0.102)	(0.086)
Bsc.+75%	-0.282***	-0.267***	-0.210***	-0.302***	-0.302***	-0.268***
	(0.068)	(0.065)	(0.075)	(0.070)	(0.110)	(0.080)
Msc	0.144**	0.115*	0.179**	0.141**	0.187**	0.093
	(0.063)	(0.062)	(0.074)	(0.067)	(0.093)	(0.085)
N	1299	1449	906	1116	645	654
Panel C: Conscientiousness						
Bsc.+25%	-0.136**	-0.135**	-0.088	-0.153**	-0.163*	-0.128
	(0.065)	(0.061)	(0.072)	(0.067)	(0.096)	(0.088)
Bsc.+75%	-0.210***	-0.227***	-0.124	-0.259***	-0.230**	-0.188**
	(0.070)	(0.066)	(0.080)	(0.070)	(0.108)	(0.086)
Msc	0.156**	0.125**	0.248***	0.118*	0.210**	0.122
	(0.064)	(0.060)	(0.076)	(0.067)	(0.096)	(0.084)
N	1299	1449	906	1116	645	654
Panel D: Emotional stability						
Bsc.+25%	-0.150**	-0.119*	-0.126*	-0.138*	-0.189*	-0.120
	(0.068)	(0.063)	(0.076)	(0.072)	(0.104)	(0.087)
Bsc.+75%	-0.169***	-0.193***	-0.083	-0.160**	-0.096	-0.227***
	(0.064)	(0.061)	(0.070)	(0.064)	(0.099)	(0.080)
Msc	0.178***	0.147**	0.197***	0.210***	0.231**	0.131
	(0.061)	(0.059)	(0.073)	(0.064)	(0.092)	(0.082)
N	1299	1449	906	1116	645	654
Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. Column 1 shows the main specification. Column 2 includes all observations. Column 3 excludes individuals with a response time less than seven minutes. Column 4 excludes individuals whose company has a wage-setting policy favoring master degree holders. Columns 5 and 6 split the sample by beliefs of how much Covid-19 changed hiring requirements. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E10: Robustness of employer beliefs by educational scenario (ctd.)

	(1) Main	(2) All obs.	(3) Resp. time	(4) No rule	(5) Covid: low	(6) Covid: high
PANEL E: TRAINABILITY						
Bsc.+25%	0.029	0.033	0.066	-0.010	-0.051	0.092
	(0.061)	(0.057)	(0.068)	(0.063)	(0.092)	(0.083)
Bsc.+75%	-0.036	-0.045	0.024	-0.079	-0.101	0.010
	(0.063)	(0.059)	(0.071)	(0.063)	(0.099)	(0.077)
Msc	0.219***	0.211***	0.216***	0.201***	0.273***	0.172**
	(0.057)	(0.054)	(0.069)	(0.060)	(0.086)	(0.078)
N	1299	1449	906	1116	645	654
PANEL F: INTELLIGENCE						
Bsc.+75%	-0.032	-0.045	-0.000	-0.022	-0.099	0.009
	(0.060)	(0.057)	(0.067)	(0.062)	(0.085)	(0.087)
Bsc.+75%	-0.049	-0.054	0.024	-0.040	-0.092	-0.004
	(0.063)	(0.059)	(0.074)	(0.062)	(0.094)	(0.082)
Msc	0.131**	0.121**	0.175***	0.140**	0.085	0.181**
	(0.057)	(0.055)	(0.065)	(0.058)	(0.079)	(0.083)
N	1299	1449	906	1116	645	654
PANEL G: EXPERTISE						
Bsc.+25%	0.001	0.005	0.018	-0.007	-0.029	0.009
	(0.065)	(0.061)	(0.069)	(0.068)	(0.095)	(0.086)
Bsc.+75%	-0.055	-0.063	0.028	-0.064	-0.074	-0.044
	(0.066)	(0.063)	(0.072)	(0.069)	(0.104)	(0.080)
Msc	0.245***	0.224***	0.302***	0.236***	0.293***	0.210***
	(0.061)	(0.057)	(0.070)	(0.067)	(0.090)	(0.080)
N	1299	1449	906	1116	645	654
PANEL H: SES						
Bsc.+25%	0.016	-0.000	0.063	0.023	-0.019	0.050
	(0.056)	(0.054)	(0.058)	(0.061)	(0.078)	(0.078)
Bsc.+75%	-0.070	-0.078	-0.001	-0.075	-0.070	-0.058
	(0.052)	(0.051)	(0.055)	(0.056)	(0.079)	(0.066)
Msc	0.101**	0.087*	0.167***	0.107**	0.155**	0.062
	(0.049)	(0.048)	(0.056)	(0.052)	(0.065)	(0.071)
N	1299	1449	906	1116	645	654
Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns show coefficients that are estimates from a linear regression, including employer FEs and control variables. White robust standard errors clustered at the respondent level are displayed in parentheses. Column 1 shows the main specification. Column 2 includes all observations. Column 3 excludes individuals with a response time less than seven minutes. Column 4 excludes individuals whose company has a wage-setting policy favoring master degree holders. Columns 5 and 6 split the sample by beliefs of how much Covid-19 changed hiring requirements. The data are unbalanced as employers randomly receive and assess résumés corresponding to three out of the four scenarios. The Bsc. scenario serves as a baseline estimate. Control variables comprise the randomized résumé elements: gender, age, secondary school grade, university, bachelor grade, internship fit to job vacancy, internship firm, internship duration, languages, personal interests and IT skills. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E11: Robustness of decomposition of candidate attractiveness

	Msc. - Prob. to invite						Msc. - Prob. to offer					
	(1) Main	(2) All obs	(3) Resp. time	(4) No rule	(5) Covid: low	(6) Covid: high	(7) Main	(8) All obs	(9) Resp. time	(10) No rule	(11) Covid: low	(12) Covid: high
Difference with Bsc.	4.897***	4.798***	5.525***	4.415***	6.286***	3.560*	3.920***	4.784***	4.842***	3.260**	5.138**	2.772
	(1.524)	(1.444)	(1.814)	(1.640)	(2.351)	(1.931)	(1.468)	(1.412)	(1.740)	(1.581)	(2.177)	(1.957)
Explained	3.484***	3.506***	3.260***	2.872***	5.232***	2.066	3.111***	3.202***	2.772**	2.745***	4.932***	2.197*
	(0.956)	(0.917)	(1.187)	(1.009)	(1.524)	(1.268)	(0.935)	(0.906)	(1.137)	(0.964)	(1.466)	(1.264)
Unexplained	1.413	1.292	2.265	1.543	1.054	1.494	0.809	1.583	2.070	0.515	0.206	0.575
	(1.221)	(1.149)	(1.410)	(1.345)	(2.029)	(1.461)	(1.171)	(1.118)	(1.352)	(1.297)	(1.778)	(1.542)
Explained												
Train.	1.332***	1.206***	1.427**	1.045**	1.363	0.837	1.101**	0.955**	1.475**	0.981**	1.870*	0.589
	(0.492)	(0.428)	(0.672)	(0.485)	(0.926)	(0.642)	(0.440)	(0.378)	(0.698)	(0.467)	(0.975)	(0.471)
IQ	-0.064	0.114	0.235	-0.143	-0.008	0.081	0.611*	0.618**	0.563	0.346	-0.091	1.136*
	(0.222)	(0.202)	(0.266)	(0.241)	(0.438)	(0.247)	(0.331)	(0.312)	(0.369)	(0.296)	(0.397)	(0.686)
Expert.	1.324***	1.295***	1.146**	1.450***	1.727*	0.775	0.446	0.563*	-0.060	0.572	0.903	0.199
	(0.483)	(0.444)	(0.525)	(0.553)	(0.923)	(0.497)	(0.363)	(0.339)	(0.369)	(0.384)	(0.784)	(0.300)
Persev.	0.677*	0.794**	0.541	0.701*	2.537**	-0.031	0.822**	0.794**	0.868*	0.741*	2.170**	0.034
	(0.365)	(0.369)	(0.385)	(0.400)	(1.043)	(0.151)	(0.380)	(0.350)	(0.464)	(0.393)	(0.903)	(0.135)
Comm.	0.053	0.130	-0.145	0.010	-0.218	0.146	0.149	0.197	-0.038	0.197	0.214	0.079
	(0.204)	(0.196)	(0.240)	(0.187)	(0.485)	(0.200)	(0.220)	(0.206)	(0.233)	(0.223)	(0.503)	(0.172)
Consc.	0.183	0.052	0.084	0.075	0.400	0.088	0.128	0.118	0.024	0.026	0.238	0.052
	(0.240)	(0.196)	(0.317)	(0.171)	(0.647)	(0.208)	(0.200)	(0.186)	(0.295)	(0.165)	(0.577)	(0.131)
Emot.	0.039	-0.007	-0.068	-0.379	-0.622	0.222	-0.000	0.124	0.053	-0.008	-0.190	0.155
	(0.252)	(0.225)	(0.256)	(0.283)	(0.562)	(0.287)	(0.217)	(0.202)	(0.274)	(0.272)	(0.488)	(0.212)
SES	-0.059	-0.077	0.039	0.113	0.053	-0.051	-0.146	-0.166	-0.114	-0.108	-0.181	-0.047
	(0.120)	(0.122)	(0.139)	(0.138)	(0.265)	(0.146)	(0.135)	(0.138)	(0.146)	(0.130)	(0.244)	(0.134)
N	645	724	438	551	322	323	645	724	438	551	322	323

Notes: The table shows the coefficients of the decomposition of the significant differences in candidate attractiveness shown in columns 1 and 3 of table 2.1. Columns 1 and 7 show the main specification. Columns 2 and 8 include all observations. Columns 3 and 9 exclude individuals with a response time less than seven minutes. Columns 4 and 10 exclude individuals whose company has a wage-setting policy favoring master degree holders. Columns 5, 6, 11 and 12 split the sample by beliefs of how much Covid-19 changed hiring requirements. See equation 2.3 for details on the decomposition. The t statistics are displayed in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 2.E11: Robustness of decomposition of candidate attractiveness (ctd.)

	Msc. - Log wage						Bsc.+75% - Prob. to invite					
	(1) Main	(2) All obs	(3) Resp. time	(4) No rule	(5) Covid: low	(6) Covid: high	(7) Main	(8) All obs	(9) Resp. time	(10) No rule	(11) Covid: low	(12) Covid: high
Difference with Bsc.	0.070***	0.112*	0.077***	0.077***	0.059**	0.078**	-3.435**	-2.531*	-2.432	-4.047**	-3.322	-3.494*
	(0.023)	(0.067)	(0.029)	(0.024)	(0.027)	(0.038)	(1.561)	(1.491)	(1.852)	(1.683)	(2.556)	(1.836)
Explained	0.007	0.038	0.009	0.008	0.016*	0.006	-2.445**	-2.189**	-1.748	-3.081***	-3.995**	-1.710
	(0.005)	(0.024)	(0.006)	(0.005)	(0.009)	(0.008)	(1.089)	(1.045)	(1.294)	(1.160)	(1.845)	(1.295)
Unexplained	0.063***	0.075	0.068**	0.069***	0.043	0.072*	-0.991	-0.343	-0.684	-0.966	0.673	-1.784
	(0.024)	(0.065)	(0.029)	(0.025)	(0.027)	(0.038)	(1.186)	(1.122)	(1.394)	(1.303)	(1.913)	(1.461)
Explained												
Train.	-0.009	-0.017	-0.006	-0.004	-0.010	-0.004	-0.694	-0.589	-0.741	-1.119**	-0.869	-0.556
	(0.006)	(0.020)	(0.006)	(0.005)	(0.010)	(0.005)	(0.498)	(0.462)	(0.738)	(0.557)	(0.784)	(0.657)
IQ	0.005	0.010	0.004	0.001	-0.002	0.008	-0.170	-0.151	-0.111	-0.161	-0.409	-0.043
	(0.005)	(0.017)	(0.005)	(0.004)	(0.006)	(0.007)	(0.169)	(0.169)	(0.187)	(0.177)	(0.473)	(0.113)
Expert.	0.008	0.001	0.007	0.010*	0.015	0.005	-0.375	-0.289	-0.163	-0.475	-0.376	-0.302
	(0.006)	(0.021)	(0.007)	(0.006)	(0.010)	(0.006)	(0.314)	(0.272)	(0.256)	(0.354)	(0.443)	(0.359)
Persev.	0.002	0.016	0.001	-0.001	0.005	0.002	-1.476***	-1.371***	-1.208**	-1.814***	-3.226***	-0.659
	(0.005)	(0.030)	(0.006)	(0.005)	(0.009)	(0.003)	(0.505)	(0.464)	(0.527)	(0.620)	(1.149)	(0.435)
Comm.	-0.001	0.044	-0.005	-0.000	0.006	-0.004	-0.000	-0.162	0.579	-0.039	0.486	-0.351
	(0.003)	(0.029)	(0.005)	(0.003)	(0.007)	(0.005)	(0.289)	(0.242)	(0.367)	(0.331)	(0.530)	(0.375)
Consc.	0.002	-0.029	0.008	0.001	0.004	0.000	0.315	0.414*	-0.020	0.553	0.539	0.176
	(0.004)	(0.020)	(0.007)	(0.003)	(0.009)	(0.002)	(0.263)	(0.251)	(0.241)	(0.356)	(0.495)	(0.304)
Emot.	0.005	0.030	0.001	0.005	0.005	0.002	-0.091	-0.081	-0.082	0.010	-0.162	0.095
	(0.004)	(0.022)	(0.005)	(0.005)	(0.007)	(0.004)	(0.181)	(0.157)	(0.149)	(0.187)	(0.248)	(0.321)
SES	-0.005	-0.018	-0.003	-0.003	-0.006	-0.002	0.047	0.041	-0.002	-0.037	0.022	-0.070
	(0.004)	(0.014)	(0.003)	(0.003)	(0.004)	(0.005)	(0.089)	(0.080)	(0.053)	(0.109)	(0.236)	(0.177)
N	645	724	438	551	322	323	642	714	454	554	315	327

Notes: The table shows the coefficients of the decomposition of the significant differences in candidate attractiveness shown in columns 1 and 5 of table 2.1. Columns 1 and 7 show the main specification. Columns 2 and 8 include all observations. Columns 3 and 9 exclude individuals with a response time less than seven minutes. Columns 4 and 10 exclude individuals whose company has a wage-setting policy favoring master degree holders. Columns 5, 6, 11 and 12 split the sample by beliefs of how much Covid-19 changed hiring requirements. See equation 2.3 for details on the decomposition. The t statistics are displayed in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

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Chapter 3

The Monetary Value Of Freedom Of Choice

Joint with Hannah Schildberg-Hörisch

3.1 Introduction

Paternalistic policies have a bearing on many dimensions of our society and affect our everyday life in various ways. While (legal) paternalism and its effectiveness has a particular relevance for society, its ethical justification is a matter of an ongoing contentious debate.[1] The question if paternalistic interventions are justified and when individuals' freedom of choice is to be respected is a persistent controversy not alone in the economic literature. Papers arguing in favor of the implementation of paternalistic policies point out that paternalism is needed, in order to remedy behavioral biases and help individuals to make better choices. There is a large literature examining behavioral biases and proposing counteracting paternalistic policies partly discussing the welfare implications of the intervention itself and partly not (see e.g. O'Donoghue and Rabin, 1999; Madrian and Shea, 2001; Thaler and Benartzi, 2004; Carroll et al., 2009; Karlan et al., 2016; O'Donoghue and Rabin, 2003; Camerer et al., 2003; Zamir, 1998). In the early 2000s, libertarian paternalism or nudging came in the spotlight of both public and academic debate.[2] Thaler and Sunstein (2003) base their argument in favor of libertarian paternalism on the notion that paternalistic action is inevitable in many contexts, hence the

[1]Paternalism is usually defined as "[...] the interference with a person's liberty of action justified by reasons referring exclusively to the welfare, good, happiness, needs, interests or values of the person being coerced" (Dworkin, 1972). We define *legal* paternalism as a paternalistic intervention enforced by a public institution.

[2]Libertarian paternalism is defined as a paternalistic intervention that helps people to improve their decision making, but does not involve coercion. The mechanism behind nudging draws on more subtle ways to affect (or manipulate) behavior (some influential publications on nudging are e.g. Thaler and Sunstein, 2003; Sunstein, 2015; Sunstein and Thaler, 2003; Carroll et al., 2009; Chetty, 2015; Allcott and Kessler, 2019).

question should concentrate on how to be paternalistic and not if at all.[3]

On the other side, there are plenty of studies opposing or criticizing (libertarian) paternalism. The argument against paternalism builds on protecting autonomy and liberal principles and preserving freedom of choice, suggesting alternative ways to foster good decision-making (e.g. Schmidt and Engelen, 2020; Grüne-Yanoff, 2012; Gigerenzer, 2015; Mitchell, 2004; Hausman and Welch, 2010; Rebonato, 2014).[4] Another concern that emerges in the analysis of paternalistic action, is that welfare implications can not always be identified, e.g. in case preferences or behavioral patterns are not fully known (examples for studies discussing various welfare concepts, Thunström, 2019; Gruber and Mullainathan, 2005; Bernheim and Rangel, 2009; O'Donoghue and Rabin, 2003; Gul and Pesendorfer, 2008; Kőszegi and Rabin, 2008). Moreover, it is criticized that the analysis of paternalistic policies often underlies a consequentalist approach, i.e. a regulation is judged by the sum of utility or well-being it generates ex-post. Although some studies suggest that individuals indeed sometimes have a consequentalist view on a situation (e.g. Johansson-Stenman, 2012), they often care about the implementation and the intention behind a regulation as well (e.g. Falk et al., 2008).
Interestingly, it appears that most studies about the ethical perspective on paternalism assume that restricting people's autonomy in a paternalistic context should bother individuals intrinsically. Camerer et al. (2003) write in their paper on asymmetric paternalism: "We also echo the *common intuition* that people may have an intrinsic taste for free choice, and many of the policies we discuss may be worse than described if people believe that they encroach on their freedom." But while the general refusal of paternalism is usually implicitly presumed in the debate about the justification of public interference, there is surprisingly little conclusive evidence about it, other than the one following from a philosophical argument.[5] Yet, a substantiated understanding of people's attitudes towards paternalism would not only be beneficial for the welfare analysis of policies and the subsequent policy evaluation. It would also provide an important groundwork for the discussion about the ethical justification of paternalism. This is why our paper concentrates on this less reviewed angle of the discourse, namely the individual's *perception* of an imposed legal paternalistic action.

Our work provides experimental evidence on the (incentivized) reaction to an imposed paternalistic restriction and disentangles the instrumental from the intrinsic value of freedom of choice. To achieve this, we measure the perception towards paternalism

[3] Thaler and Sunstein (2003) argue that in many cases a policy maker needs to decide about the choice design and paternalistic influence is inevitable.

[4] See the essay by Sen (1988) for an extensive discussion on conceptual aspects concerning the characterization and evaluation of freedom of choice in economics.

[5] Hurka (1987), a philosopher, elaborates in his article about the question why people might value autonomy and writes "An opposing view retains the idea that autonomy is intrinsically good, but denies that this needs any justification. [...] This view is not inane, and it may represent a fall-back position. But it would be defeatist to adopt it from the start. We are challenged to explain why autonomy is good, and the challenge is not obviously inapposite."

in a standard decision situation under risk (lottery choices), that compares closely to conditions involving legal paternalism. More specifically we elicit a monetary valuation for removing a paternalistic restriction and then distinguish between an instrumental motive for this willingness to pay and an intrinsic one. Besides, we use different situational specifications, to test if the intrinsic value of freedom of choice depends on the context or if it is a more general concept. Our findings can be summarized in four main results. First, in almost half of all situations, individuals reject the paternalistic constraint, that was imposed on them, and indicate a positive willingness to pay to remove the restriction. Second, we observe a positive intrinsic value of freedom of choice for approximately 30% of the decision situations and therefore can confirm, that individuals value their autonomy beyond the instrumental benefit that it provides. Third, the value of freedom of choice varies substantially across lottery specifications, which indicates that the valuation of a free choice set is dependent on the situational context. Lastly, most participants show a positive monetary value of freedom of choice in some situations, but not in all and we find individual level heterogeneities with respect to occurrence and magnitude of the value of freedom of choice.

To our knowledge, there are only two studies using an incentivized experiment to analyse the perception of paternalism, making use of a principal-agent set-up. Both studies are strongly related to our work, as they are concerned with the reaction of an agent that is subject to a paternalistic restriction by a principal. Kataria et al. (2014) examine experimentally if the reaction towards a paternalist, who restricts their freedom of choice, is driven by consequentialism or grounded on an intrinsic valuation for freedom of choice. The authors find, that individuals take up a *consequentalist position* facing a paternalistic restriction and seem to only show an *instrumental* value for freedom of choice, as they refrain from punishing the paternalist, unless they are constraint in their preferred action. The results of this study contrast the intuitive notion that freedom of choice should be intrinsically rooted. The second, more recently published study by Lübbecke and Schnedler (2020) uses a principal-agent set up as well to examine the agent's preference for authorship. They find that one in five participants insist on using their own solution to a problem and reject help if offered by the principal (paternalist), even though the proposed solution is similar to theirs and rejection comes at significant monetary costs. The authors suggest that individuals oppose interference in order to show their own abilities and determine their own fate. This result supports the notion of an intrinsic valuation for autonomy.

While both papers contribute to the understanding of the attitude towards paternalism, there is one concern about the validity of these results for the understanding of *legal* paternalistic action. The studies are based on a principal-agent set up, where one participant randomly gets power over the actions of another, which entails that the reaction of the agent is (at least partially) driven by social preferences, instead of the intrinsic valuation of freedom of choice. In the case of legal paternalism, which is the prime focus

of most of the literature, a policy is usually not imposed by a specific person, but rather by the government, the legal system or an organization. Therefore, the reaction to a legal paternalistic intervention might differ from the "face-to-face" principal-agent set up used by the two papers before.
Our paper also relates to a stream of literature that provides evidence about the intrinsic value of decision rights. Bartling et al. (2014); Fehr et al. (2013); Bobadilla-Suarez et al. (2017); Ferreira et al. (2020); Neri and Rommeswinkel (2017) explore preferences for decision rights in a delegation game. Contrary to Kataria et al. (2014) they find that most individuals have a substantial intrinsic valuation for freedom of choice that also significantly exceeds the instrumental value of decision rights, mostly driven by a bias to retain authority (Fehr et al., 2013) and a preference for self-reliance (Ferreira et al., 2020). These studies demonstrate that individuals show a substantial intrinsic valuation for their decision rights even if that decreases their payment. These insights, however, build on a specific delegation set up, where the payoff of both parties is affected by the decisions made. It is unapparent if this body of literature can be transferred to a context of legal paternalism. Our paper enhances the existing experimental evidence on the intrinsic value of freedom of choice by two aspects. First, our experimental design resembles a context involving paternalism and second we refrain from a bilateral set up to minimize the impact of social preferences.

More recently the literature specific to the perception of paternalism grows, though almost entirely concentrating on nudging and libertarian paternalism. In addition, this body of evidence relies on survey and vignette studies, while evidence from experiments or the field is mostly lacking. We complement this body of literature in both respects, using an incentivized experiment that examines a strict form of paternalism. The findings of the survey evidence indicate, that overall individuals frequently support paternalistic nudges. However, the studies suggest that policies with low-level of intrusion, e.g. education, are preferred over policies that function unconsciously. Policies with high-level of intrusion, like taxation, are least accepted (see e.g. Konrad and Simon, 2021; Petrescu et al., 2016; Arad and Rubinstein, 2018; Loewenstein et al., 2015; Felsen et al., 2013; Jung and Mellers, 2016; Sunstein, 2016; Casal et al., 2019; Bruns et al., 2018; Yan and Yates, 2019).[6] Further, the perception of a paternalistic policy seems to be connected to the decision context (Reisch and Sunstein, 2016; Hedlin and Sunstein, 2016; Hagman et al., 2015, e.g.) and the individuals' traits and preferences (Jung and Mellers, 2016; Hagman et al., 2015; Tannenbaum et al., 2017; Sunstein et al., 2019), opposing the idea that one universal valuation is attributed to autonomy. Surprisingly, a relation between little self control and the support for paternalism could not be established (Pedersen

[6]One can distinguish between System I nudges that affect the decision processes subconsciously and System II nudges that interfere with deliberative decision making, e.g. through information. If people feel the risk that they are nudged to an undesired outcome driven by unknown behavioral biases, they are more likely to disapprove an intervention.

et al., 2014). On the contrary, Pedersen et al. (2014) find a counter-intuitive relation between good self control and support for rigid paternalism. The large majority of this literature, however, considers libertarian policies only. One exception is the study by Hagman et al. (2022), who include both a strict form of paternalism (legislation) and no regulation at all as reference points for the comparison with a nudge in their survey and find that the acceptability of a nudge does not increase, if compared to a strict form of paternalism. Participants even report a higher perceived intrusiveness to freedom of choice of nudging if the reference is strict paternalism compared to no policy at all.

Altogether, the evidence about the perception on paternalism is mixed. While it seems intuitive to assume that individuals have an intrinsic value of decision rights facing a paternalistic action, the literature only partly supports this. A large body of literature building on survey- and vignette studies is informative about influential factors of the acceptability of paternalistic nudges. However, in the light of a more thorough understanding of the perception of paternalism, the body of literature lacks the examination of rigid paternalistic interventions and builds on survey data without the use of incentives. Other streams of literature use incentivized experiments to study the intrinsic value of decision rights, but the experimental designs build on a principal-agent set up evoking social preferences, which is only to a limited extent comparable to the reaction to paternalistically motivated regulations. Our paper, enhances the understanding of the perception of paternalism in multiple ways, using an *incentivized* experiment that examines a *strict* form of paternalism and that does *not involve* a bilateral setting to minimize the impact of social interaction. We provides first incentivized evidence that individuals are willing to give up money - exceeding the instrumental benefit of a full choice set - to protect their freedom of choice in a paternalistic context.

The paper is structured as followed: we start with outlining the experimental design and implementation procedure. In section 3.3, we explain how we identify the monetary value of freedom of choice. Then we analyse the results, subdivided into general findings, an analysis of the impact of situational specifications on the value of freedom of choice and an individual level analysis in section **??**. At the end, we discuss the results and conclude.

3.2 Experiment

Our experiment is designed to answer two questions in an incentivized manner: First, do people attribute an intrinsic value to freedom of choice? Second, does the intrinsic value of freedom of choice depend on the situational context (such as stake sizes involved, gain versus loss domain) and individual characteristics (like attitude towards risk or self-control)?

More specifically, we investigate if individuals are willing to give up money to regain a full choice set after it was restricted by a paternalistic intervention. In order to answer our research questions, it is key to separately identify the two motives that can drive a willingness to pay to regain freedom of choice. On the one hand, a full choice set has an instrumental value. If a constraint makes the preferred option of a choice set unavailable, regaining the favored option increases utility. On the other hand, individuals might additionally attribute an intrinsic value to freedom of choice on top of the instrumental value, if they dislike being restricted in their choices per se. To be able to identify a possible intrinsic value of freedom of choice, we need to observe the willingness to pay to remove an imposed paternalistic restriction beyond the individual's instrumental value of a free decision. For this purpose, we chose a standard decision situation involving risk, in which individuals choose between a safe payment and a lottery. In this setting, we can elicit the individual instrumental value of a free decision by measuring the individual's certainty equivalent of the lottery. Next, we compare the willingness to pay to regain a full choice set in a restricted situation, where the lottery cannot be selected anymore, to the certainty equivalent and the safe payment that individuals are compelled to choose. Hence, this design enables us to disentangle the instrumental value of removing a restriction from the intrinsic value of freedom of choice and to quantify both.
We study people's reaction to paternalistic policies in a context involving risk for two reasons. First, lottery choices are well studied situations, that allow us to determine the instrumental value of a free choice set (i.e. the certainty equivalent). Second, paternalistic policies are typically applied to situations that either involve risk or externalities. Thus, using a situation that simulates a gambling decision is relatively close to a real world situation.[7]

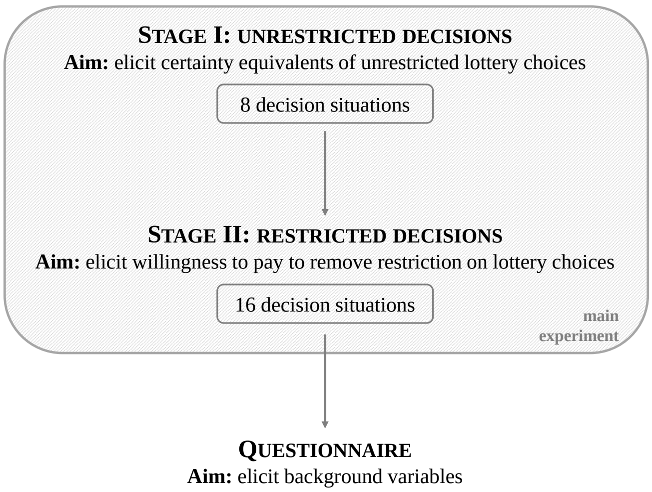
3.2.1 Design

The main experiment consists of two stages containing a total of 24 decision situations, 8 in stage I and 16 in stage II. Figure 3.1 provides a graphical overview of the within-subject design. We elicit two variables that are needed to compute the intrinsic value of freedom of choice (VoF): first, the individual certainty equivalent for each lottery in stage I and second, the willingness to pay to remove a paternalistic restriction in stage II.

Stage I: Unrestricted decisions
In stage I, we elicit the individual valuation of the 8 different lotteries, i.e., their certainty equivalents $ce_{i,lot}$ for each individual i and lottery lot. We use four lottery types, combined with two different stake sizes each. The expected value of the lotteries ranges between -0.4€ and 12€ . We provide further details on the 8 lotteries below.

[7]Examples for paternalistic policies in risky situations include, among many others, the duty to wear a seat belt in cars, to take out insurances, applying extra taxes on cigarettes or unhealthy food, or subsidies for retirement savings.

Figure 3.1: Overview of experimental design



Notes: Figure 3.1 shows the experimental procedure. The main experiment consists of two stages. Individuals face 8 decision situations in stage I and 16 decision situations in stage II. After the main experiment a questionnaire follows.

For each lottery, participants decide for which amount of money they are indifferent between a risky lottery and a safe payment with the help of a choice list. Each choice list contains multiple rows, offering safe payments between 0 and a fixed maximum amount of money, max_list_{lot}, that varies across lotteries. Table 3.D1 in the appendix provides detailed information on all choice lists, including max_list_{lot} and the increments with which the safe payment increases or decreases. The safe payment is either increasing from one row to the next or decreasing, which is determined at random for each of the 8 decisions. After filling in the main choice list, a second, more detailed choice list appears to elicit the certainty equivalent even more fine-grained. It ranges from the highest safe payment for which an individual still preferred the lottery to the next higher safe payment which is the lowest safe payment that the individual preferred over the lottery, i.e., the switching point. This procedure allows us to measure all certainty equivalents with a 10ct precision. Figures 3.B1 and 3.B2 in the appendix show screenshots from the experiment with an increasing choice list and a detailed choice list.
We use the information on individual certainty equivalents for each of the 8 lotteries to identify each participant's individual *instrumental* value of removing an imposed restriction.

Stage II: Restricted decisions

In the second stage, participants are restricted in their freedom of choice. We observe their reaction towards the imposed paternalistic constraint by eliciting individuals' willingness to pay for removing the imposed restriction and regaining the full choice set.

We use the same lotteries as in stage I, but vary the nature of the imposed restriction in order to achieve a variation in the participants' perception of the constraint. Restrictions are either neutral or unfavorable. As a result, stage II consists of 16 decision situations s (8 lotteries with 2 kinds of restrictions). In each decision situation, individuals face a comparison between one of the lotteries and a safe payment. In contrast to the decisions in stage I, participants face only *one* fixed amount of money p_s (i.e., only one row of the full choice list in stage I) as alternative to playing the lottery lot in each decision situation. Moreover, participants are initially restricted to choosing the safe payment in stage II, but they have the option to remove this restriction, if they are willing to pay for it.

The exact set-up is as follows: First, the upcoming choice between the lottery and the safe payment p_s is described. On the same screen, participants are informed that they are restricted to choose the safe payment, which is additionally visualized as in figure 3.B3 in the appendix. While both alternatives are still displayed on the screen, the choice is fixed to be the safe payment and cannot be changed. To give the constraint a paternalistic foundation, participants also get a brief explanation why their choice set is restricted and why the lottery is not available to them. The text states that taking the lottery comes at the risk of not winning anything (or even losing money), which is why they are restricted to a safe option to prevent them from getting nothing.[8] Participants can react in two ways to the constraint, in a conformist and non-conformist way. If participants want to conform to the policy and take the safe payment, they can directly accept the restriction. In this case their willingness to pay for removing the restriction, $wtp_{i,s}$, is zero. If participants do not want to conform to the restriction, they have the possibility to state a willingness to pay to remove the restriction that is elicited by means of a choice list. Similar to the unrestricted decision situations, we also use a second choice list to elicit the willingness to pay in more detail (10ct steps) after observing the choice in the main choice list. The actual price for abolishing the restriction is determined at random and participants are aware of that. Only if their willingness to pay is larger or equal to the random price, they regain freedom of choice in the specific decision situation after paying the randomly determined price.[9] All participants are asked afterwards to indicate what they would have choosen, given they could have decided freely between the lottery and the safe payment, even if the participant accepted the restriction directly. The elicited willingness to pay, together with the certainty equivalents from stage I, enables us to disentangle the instrumental value of removing the restriction from the value of freedom of choice (see section 3.3).

[8]The wording for the restricted situation with a 50/50 lottery corresponds to: "*Humans often make decisions that they regret afterwards. Taking this investment opportunity, you might end up getting 0€ with a probability of 50%. This is why your freedom of choice has been restricted in this decision situation. The following binding choice had been made for you: you get the safe payment of 2.70€ and you do not have the opportunity to chose the investment option.*"

[9]This preference revealing mechanisma is similar to the one used by Becker et al. (1964).

Lotteries

As a standard decision situation involving risk, we use lotteries. In order to allow for a systematic investigation if behavioral responses to paternalism are sensitive to changes within the decision context under risk, we use four different types of lotteries and additionally vary their stake size. Table 3.1 provides an overview of all specifications of lotteries and restrictions we use.

Table 3.1: Overview lottery specifications

Lottery	**Specifications**				
	winning prob. & amount	losing prob. & amount	E(lot)	safe payment p_s (restriction) neutral	safe payment p_s (restriction) unfavorable
50/50 low	50% 11€	50% 0€	**5.5€**	6€	2.7€
50/50 high	50% 22€	50% 0€	**11€**	12€	5.4€
LS low	10% 60€	90% 0€	**6€**	6.6€	3€
LS high	10% 120€	90% 0€	**12€**	13.2€	6€
RLS low	90% 6€	10% 0€	**5.4€**	5.9€	2.7€
RLS high	90% 12€	10% 0€	**10.8€**	11.8€	5.4€
LOSS low	90% 0€	10% -4€	**-0.4€**	-0.36€	-0.8€
LOSS high	90% 0€	10% -8€	**-0.8€**	- 0.72€	-1.6€

Notes: Table 3.1 displays all lotteries. Column (2) and (3) show the winning (losing) probabilities and the amount of money participants can win (lose), followed by the expected value of the lottery in column (4). The last two columns show the safe payment that is offered as alternative to the lottery in the restricted situations, whereby the neutral safe payment is always higher than the unfavorable payment.

All lotteries have two possible outcomes, one of them is always earning 0€. Lotteries vary both with regard to the second payoff and the probabilities attached to both outcomes. In particular, we use the following four lottery types: a 50/50 lottery (further denoted by 50/50), a long-shot lottery with a small probability of a large gain and a large probability of earning nothing (further denoted by LS), a reversed long-shot with

a large probability of a gain and a small probability of earning nothing (further denoted by RLS), and a loss lottery (LOSS). Each of the four lottery types has two versions: their stake size is either low or high. In the high stake version, the amount that can be won (lost) is twice as high as with the low stake size.
In the restricted choices in stage II, we additionally vary the amount of money offered as the safe option, p_s, that people are predetermined to choose. To ensure that participants perceive some paternalistic constraints as more restrictive than others, we systematically vary p_s to be either a neutral (p_s^n) or an unfavorable (p_s^u) amount of money, with $p_s^n > p_s^u$. The neutral safe payment p_s^n is set to a value that is approximately 10% higher than the expected value of the lottery, such that for risk-averse, risk-neutral and slightly risk-seeking individuals the neutral safe payment should be more attractive than playing the lottery. The unfavorable safe payment p_s^u is set to about half of the expected value of the lottery, which should induce a strictly positive instrumental value of abolishing the restriction for almost all participants, except for extremely risk-averse ones. Overall, this results into 16 restricted lottery situations (4 lottery types $\times$ 2 stake sizes $\times$ neutral and unfavorable restriction).

3.2.2 Questionnaire

After the main experiment, participants answered a questionnaire regarding individual characteristics, preferences, personality traits, and background information. In order, to keep the questionnaire as short as possible, we use the survey module from Falk et al. (2023) to capture risk attitudes, trust, altruisum, and negative and positive reciprocity. We further elicit a self-control measure based on the 13-item Brief Self-Control Scale by Tangney et al. (2004), a standard locus of control measure based on the scale from the German Socio-Economic Panel study (see Richter et al., 2017), and participants' personality traits using a validated brief Big Five survey scale, the BFI-10 (Rammstedt and John, 2007). At last, participants are asked several questions about their general attitudes towards the experiment and background information is inquired.[10]
Moreover, the questionnaire includes two incentivized decisions to study participants' reaction to defaults. The aim of these tasks is to investigate the individuals' reaction to a libertarian paternalistic policy in comparison to the value of freedom of choice in reaction to a rigid paternalist policy.[11]

3.2.3 Procedural Details And Implementation

Before the start of the main experiment, participants got sufficient time to read the instructions that explained the experiment and payment scheme. Appendix figure 3.A1

[10] We inquire the following background information: gender, age, number of siblings, disposable income per month, student status, if working more than 10h/week, study subject, last math grade, high-school average grade, experience with experiments, life satisfaction.
[11] A description of the default decisions and a short overview over the results can be found in appendix seciton 3.C.

provides the translated instructions of the experiment. Before the start of the experiment participants answered a set of control questions and could ask clarification questions. The main experiment was followed by the questionnaire.

The experiment was run in four sessions at the *BonnEconLab* at the University of Bonn, Germany, in December 2019 using the software *ztree* (Fischbacher, 2007). 94 subjects participated, 35 males and 59 females.[12] The median age was 23 years old (participants' age ranged from 18 to 61 with only 3 participants older than 40). The vast majority of participants were students (97.9%), majoring in different kind of subjects, except economics, which was the only exclusion restriction for the experiment.

Each of the 94 participants faced 24 decision situations (8 unrestricted in stage I and 16 restricted in stage II). We therefore observe 752 certainty equivalents for 8 different lotteries and 1504 reactions to paternalistic restrictions (either acceptance or rejection and eventually a willingness to pay to remove the restriction). We thus collected 1504 measures of the intrinsic value of freedom of choice from 94 independent observations.

The order of the 24 decision situations was randomized at two levels. First, we randomized whether stage I or stage II was played first. Second, within each stage, the order of the different decision situations was randomized. The randomization was implemented at the individual level, with the exception of the 50/50 low stakes lottery, which was always shown first for every participant. This guarantees that the 50/50 decision is always free of potential order effects.

Only one of the 24 decision situations was randomly chosen at the individual level to be payoff-relevant. The total payment consisted of the outcome of the chosen decision situation, a general show up fee of 4€, a flat payment for answering the questionnaire of 10€ and the profit from the investment task in the questionnaire. Participants were only informed about the payoff-relevant decision situation and their total payment at the end of the experiment. The average payment was 24.8€.

3.3 Identification Of Value Of Freedom Of Choice

We use the two main variables elicited in stage I and II of the experiment to identify the intrinsic value of freedom of choice: the certainty equivalent $ce_{i,lot}$ and the willingness to pay to remove the imposed restriction $wtp_{i,s}$. As paying to remove the paternalistic constraint in stage II can have two motives, we need to disentangle the instrumental value from the intrinsic value of removing the restriction. By comparing the individual

[12] The results presented in this paper are based on a rather small number of participants, as the underlying experimental sessions were initially planned to be a pilot study, that should be complemented by further data collections. And while the pilot study worked out as expected, the subsequent experimental sessions could not be conducted as the Covid-19 pandemic started shortly afterwards and the *BonnEconLab* was temporarily closed. The following months were characterized by the corona measures. These extraordinarily strict and far reaching paternalistic policies affected everyday life heavily. As subsequent experimental sessions on the perception of paternalism would not be comparable to the pilot study, additional data collection were unfeasible. This is why, we need to rely on a small sample only.

valuation of the lottery $ce_{i,lot}$ to the value of the pre-selected option p_s in the restricted situations s:, we identify the instrumental value $iv_{i,s}$ of removing the constraint of individual i in s:

$$iv_{i,s} = \begin{cases} ce_{i,lot} - p_s & ce_{i,lot} > p_s \\ 0 & ce_{i,lot} \leq p_s \end{cases} \tag{3.1}$$

In the restricted situations, individuals initially cannot choose the lottery, but are restricted to receive the fixed payment p_s. If the individual's valuation of the lottery exceeds the fixed payment offered ($ce_{i,lot} > p_s$), the difference between the individual certainty equivalent of the lottery and the safe payment corresponds to the instrumental value of abolishing the restriction. If the certainty equivalent is lower than the offered fixed payment ($ce_{i,lot} \leq p_s$), the safe payment should be the preferred option and abolishing the restriction has no instrumental value.

To identify a potential intrinsic value of freedom of choice, we compare the stated willingness to pay $wtp_{i,s}$ to the instrumental value $iv_{i,s}$.

$$VoF_{i,s} = \begin{cases} wtp_{i,s} & wtp_{i,s} > 0, \quad iv_{i,s} = 0 \\ wtp_{i,s} - iv_{i,s} & wtp_{i,s} > iv_{i,s} > 0 \\ 0 & iv_{i,s} \geq wtp_{i,s} > 0 \\ 0 & wtp_{i,s} = 0 \end{cases} \tag{3.2}$$

Equation 3.2 depicts the four relevant cases for the calculation of the individual intrinsic VoF in a given decision situation. The first three cases depict non-conformist behavior, i.e. when individuals reject the restriction and state a willingness to pay larger than zero. First, if the restriction does not hinder participants from taking their preferred choice, hence $iv_{i,s} = 0$, the stated willingness to pay corresponds to the value of freedom of choice: $VoF_{i,s} = wtp_{i,s}$. Second, if removing the restriction has an instrumental value, the VoF can be either positive, if the willingness to pay exceeds the instrumental value $wtp_{i,s} > iv_{i,s}$, or third, it can be zero, if the willingness to pay is positive, but the instrumental value is not fully covered ($wtp_{i,s} \leq iv_{i,s}$). Fourth, if participants conform a restricted situation, i.e., they accept the restriction and their willingness to pay is zero, the intrinsic value of freedom of choice is 0, independent of the instrumental value of a full choice set. These four cases are also depicted in section 3.4.2 figure 3.3 that provides additional information how common the four cases are in our data.[13]

3.4 Results

In the following, we present the results of the experiment. We start with discussing the elicited certainty equivalents from stage I in light of the related literature in section

[13] Note that both the instrumental and intrinsic value of freedom of choice can only take on (weakly) positive values as both $wtp_{i,s} \geq 0$ and $iv_{i,s} \geq 0$ by definition.

3.4.1. In section 3.4.2, we characterize participants' behavior in response to paternalistic restrictions. First, we report rejection rates of the restriction and investigate whether they differ by situational context. Then, we document the existence of a positive intrinsic value of freedom of choice and how frequently it occurs. We conclude by analysing the role of context and individual characteristics as drivers of the observed intrinsic value of freedom of choice in sections 3.4.3 and 3.4.4.

3.4.1 Certainty Equivalents

The aim of the unrestricted lottery decisions is to elicit the certainty equivalents for all participants for each of the 8 lotteries. The certainty equivalent is the safe payment that makes a participant indifferent between the lottery and the respective safe payment. In table 3.2, we report mean and median of observed certainty equivalents $ce_{i,lot}$ as well as the share of participants who act risk seeking, i.e., for whom $ce_{i,lot} > E(lot)$.

The overall patterns turn out as expected. The average certainty equivalent lies below the expected value for all lotteries, implying that most participants act risk averse. Higher stake sizes seem to induce stronger risk aversion, as both the certainty equivalents become relatively lower and the share of risk seeking behavior decreases (except for the loss lottery). In line with previous studies (see e.g. Kachelmeier and Shehata, 1992), the overall share of risk seeking behavior is rather high in the long-shot lotteries with a small probability of a large gain. The loss lotteries show slightly different patterns. The certainty equivalent for the low stakes loss lottery is relatively small, implying higher risk aversion in the loss domain with low losing probabilities, which is in line with the prospect theory of Tversky and Kahneman (1992). However, in the loss lotteries, individuals seem to become slightly less risk averse with higher stakes, despite a low probability of losing.[14]

In table 3.D2 in the appendix, we regress the elicited certainty equivalents on individual characteristics that we elicited in the survey (risk attitudes, gender, math grade, age, positive and negative reciprocity and disposable income). As expected, a higher willingness to take risks is predictive for a higher certainty equivalent (see column 1 in table 3.D2). Besides, in column 2 of table 3.D2, we check if individual characteristics are related to the measured certainty equivalent in an expected way. Comparing our results to the work of Falk et al. (2018), we find similar correlations between individual risk taking (measured by the certainty equivalent) and gender, math grade and positive and negative reciprocity. However, only negative reciprocity is statistically significantly

[14] A higher fraction of risk seeking behavior would have been expected for higher probabilities of losing (Tversky and Kahneman, 1992). However, Harbaugh et al. (2010) find in their experimental study, that when using a choice based elicitation method - similar to what we use - risk attitudes might not always follow the fourfold pattern of risk attitudes that is suggested by Tversky and Kahneman (1992). Thus, it might not be too surprising that our results deviate slightly from the implications of the prospect theory in one lottery.

related to the certainty equivalent.[15] Overall, the results of stage I are in line with previous evidence on risk attitudes and indicate that our sample follows common patterns of risk taking behavior.

Table 3.2: Elicited certainty equivalents (mean, median, share risk seeking)

Lottery		**Certainty equivalent**		
	E(lot)	Mean	Median	% risk seeking
50/50 low	**5.5€**	5.0	5.4	31.9%
50/50 high	**11.0€**	8.6	9.3	17.0%
LS low	**6.0€**	5.6	4.9	30.9%
LS high	**12.0€**	9.7	9.0	25.5%
RLS low	**5.4€**	4.5	4.9	11.7%
RLS high	**10.8€**	8.2	9.4	8.5%
LOSS low	**-0.4€**	-0.9	-0.9	20.2%
LOSS high	**-0.8€**	-1.1	-1.5	26.6%

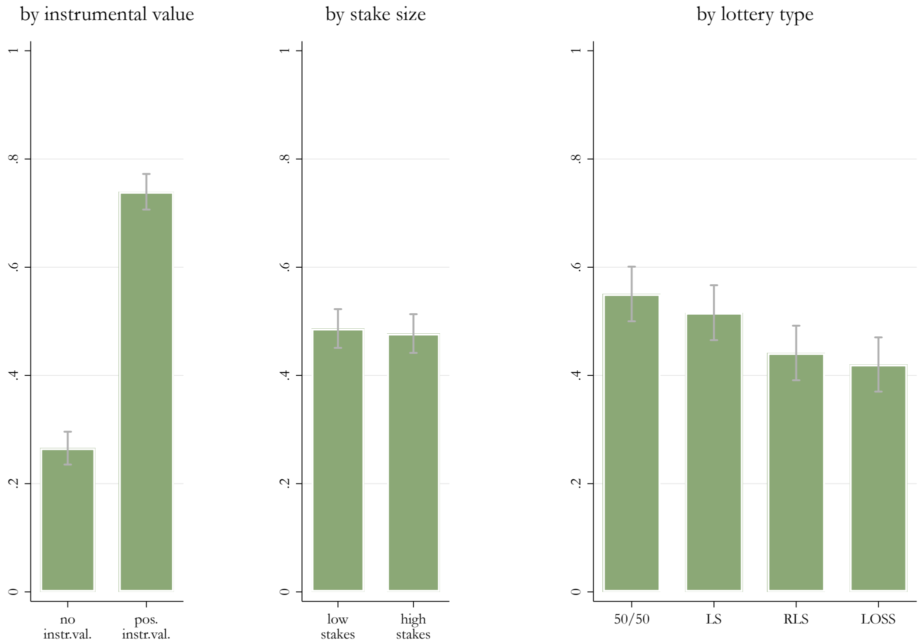
Notes: Table 3.2 displays the mean and median of elicited certainty equivalents by lottery type and stake size. It also shows the fraction of risk seeking individuals who indicate a certainty equivalent that exceeds the expected value of the lottery ($ce_{i,lot} > E(lot)$) in a specific situation.

3.4.2 Intrinsic Value Of Freedom Of Choice

As a first step towards understanding how we identify the intrinsic value of freedom of choice, we examine participants' reactions to the imposed restrictions in stage II. Overall, participants reject the pre-selected option in 48.2% of the 1,504 restricted decision situations by stating $wtp_{i,s} > 0$. As expected, rejection rates are substantially higher if the instrumental value is positive, i.e., when the paternalistic intervention restricts access to the most preferred option, as against when the favored alternative is still available. On average, individuals reject restrictions in 73.9% of situations involving a positive instrumental value of rejecting as opposed to 26.6% rejections in situations where the preferred option is still available, see first column of figure 3.2. A Fisher exact test that tests for equality of rejection rates in situations with positive and zero instrumental value, but pools them across lottery types and stake sizes yields $p = 0.000$. This observation reveals three important insights. First and not surprising, participants are sensitive to the effect of the imposed restriction on the instrumental value of the choice set. Second and more

[15]Being male, better math skills and negative reciprocity are correlated with higher risk taking, while positive reciprocity is associated with less risk taking.

Figure 3.2: Average rejection rate by instrumental value, stake size and lottery type



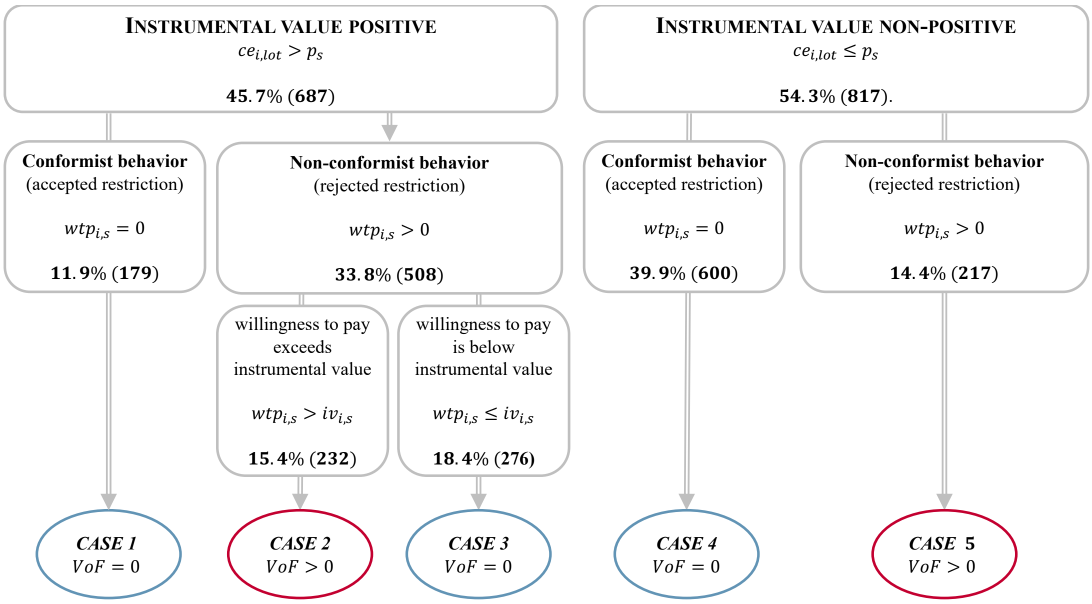
Notes: Figure 3.2 shows the average rejection rate by instrumental value, stake size and lottery type, including 95% confidence intervals. The rejection rate indicates the fraction of individuals who are willing to pay a strictly positive amount of money in order to abolish the restriction. The average rejection rate across all situations when removing the restriction has no instrumental value is 26.6% and 73.9% when removing the restriction has a positive instrumental value. For low (high) stake sizes the average rejection rate is 48.7% (47.7%). By lottery types the average rejection rates are: 55.1% (50/50), 51.6% (LS), 44.1% (RLS), 42.0% (LOSS).

unanticipated, even if a restriction hinders participants from choosing their favorite option, in 26.1% of these situations participants nevertheless conform to the pre-selected option. Third and most interesting, in more than one out of four cases individuals reject a paternalistic constraint by stating $wtp_{i,s} > 0$, although there is *no* instrumental value of removing the restriction, i.e. the preferred option was still available. In these cases, the intrinsic value of freedom of choice has to be positive.
Figure 3.2 further shows the average rejection rate by stake size and lottery type. There is no significant difference in rejection rate by stake size, when pooling over situations with different lottery types and instrumental value. Pairwise comparisons of average rejection rates by lottery type reveals that the rejection rate is significantly lower for the reversed long-shot and the loss lotteries compared to the 50/50 and long-shot lotteries.[16]

Along the lines of section 3.3, figure 3.3 depicts how combinations of instrumental value and willingness to pay allow for identifying the share of situations which reveal a positive intrinsic value of freedom of choice. In 51.8% of all 1,504 situations individuals

[16]Pairwise Fisher exact tests yield: 50/50 vs RLS: $p = 0.004$; 50/50 vs LOSS: $p = 0.000$; LS vs RLS: $p = 0.049$; LS vs LOSS: $p = 0.010$. The other pairwise comparisons yield $p > 0.05$.

Figure 3.3: Identifying value of freedom of choice



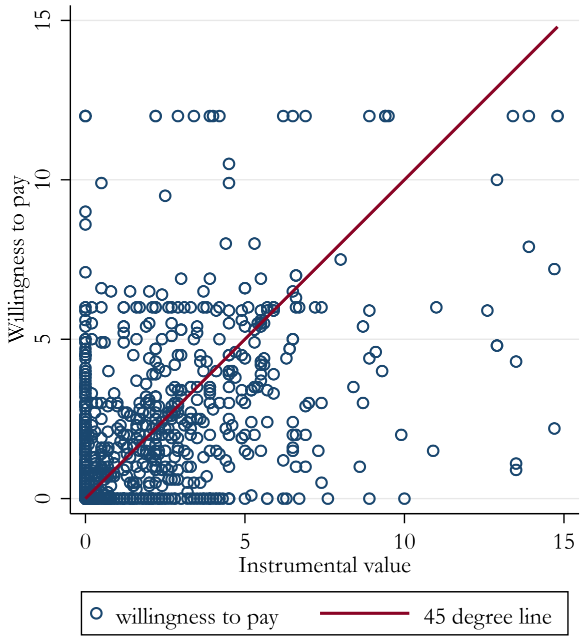
Notes: Figure 3.3 shows an overview over the different possible combinations that can arise in the experiment in stage II. Each case consists of a combination of the existence of an instrumental value of removing the restriction and the reaction to the latter. The graph starts with indicating the number of situations in which the individual instrumental value of removing a restriction is positive or (weakly) negative. In the next row, the behavior towards the restriction is depicted: either conformist ($wtp_{i,s} = 0$) or non-conformist ($wtp_{i,s} > 0$). The last row distinguishes between cases where the willingness to pay (if positive) exceeds or is smaller than the instrumental value. For all paths of the flow chart, we indicate the absolute and relative number of observed decisions. Shares always refer to the total of 1504 situations.

accept the restriction, i.e. their willingness to pay and thus their intrinsic value of freedom of choice are zero: $wtp_{i,s} = VoF_{i,s} = 0$ (see first and forth case in figure 3.3). In the remaining 48.2% of situations, participants state a $wtp_{i,s} > 0$ for removing the restriction (non-conformist behavior). In about one third of those cases (14.4% of all situations), individuals are willing to pay a positive amount of money, although the instrumental value of removing a restriction is zero indicating a positive intrinsic value the freedom of choice (last case in figure 3.3). In these cases, the intrinsic value of freedom of choice equals their willingness to pay. In situations with a positive instrumental value and a positive willingness to pay to regain both options, the willingness to pay exceeds the instrumental value in 45.7% of cases, corresponding to a share of 15.4% of all situations (case two in figure 3.3). In sum, in about 30% of situations individuals show a positive, intrinsic value of freedom of choice.

To quantify the value of freedom of choice, we need to set the individuals' willingness to pay into context of the instrumental value in one situation. Figure 3.4 illustrates the interaction between the latter two variables in a scatter plot.[17] The 45 degree

[17] Figure 3.D1 in the appendix shows the distribution of the observed willingness to pay.

Figure 3.4: Relationship between willingness to pay and instrumental value

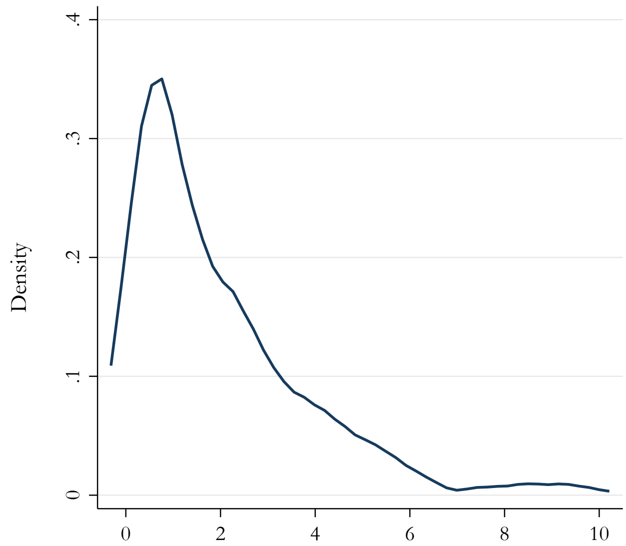


Notes: Figure 3.4 is a scatter plot displaying the indicated willingness to pay in relation to the instrumental value of removing a restriction for each of the 1504 decision situations in stage II. The red line shows the 45 degree line and therefore indicates cases where individuals are willing to pay exactly the instrumental value of removing the restriction.

line separates cases that lead to a positive $VoF_{i,s}$ (above the line) from the cases with $VoF_{i,s} = 0$ (below the line). In a substantial share of situations, the willingness to pay exceeds the instrumental value noticeably. Figure 3.4 also speaks against the notion that individuals behave in a purely profit maximizing way and invalidates the concern that the behavior could be driven by a preference for consistency (in the sense of stating a willingness to pay that equals the instrumental value). If profit maximization or a preference for consistency were the predominant drivers of the stated willingness to pay, the willingness to pay should be concentrated around the 45 degree line, as individuals should be willing to pay an amount close to the instrumental value to regain the full choice set.

Calculating the VoF based on equation 3.2, we find that the average VoF amounts to 0.6€ (std. dev. 1.47) and raises to 2.0€ (std. dev. 2.07) conditional on being positive. Figure 3.5 plots the density of the observed VoFs conditional on being positive, while appendix figure 3.D2 presents a histogram showing the observed VoF including values of zero. The median positive VoF is 1.4€ and more than half of the observed values (63%) of freedom of choice are below 2.0€. Some individuals, however, have a substantially higher VoF in some situations, with the highest decile of VoFs ranging from 4.5€ to a maximum of 12€. To make these numbers more tangible, we set the VoF in relation to the expected value of each lottery, $VoF^{rel}_{i,s} = \frac{VoF_{i,s}}{E(lot)}$. On average, the

Figure 3.5: Density of positive, intrinsic values of freedom of choice



Notes: Figure 3.5 shows the kernel density of the observed value of freedom of choice excluding values of zero and values above 10 for the sake of readability. In 0.27% of situations (4 out of 1504) the VoF exceeds 10, more specifically in all of the 4 situations the VoF is exactly 12. See figure 3.D2 in the appendix for a histogram including also values zero values for the VoF.

relative VoF amounts to 60.5% of the expected value of the lotteries, only including the positive VoFs. Including zero VoFs as well, the average is 18.1%. Figure 3.6 shows the share of observed, positive relative VoFs. While most VoFs are lower than the expected value of the lottery, 13.4% of all positive VoFs even exceed the expected value of the respective lottery ($VoF_{i,s}^{rel} > 1$), indicating a substantial intrinsic value of freedom of choice compared to the amount at stake.

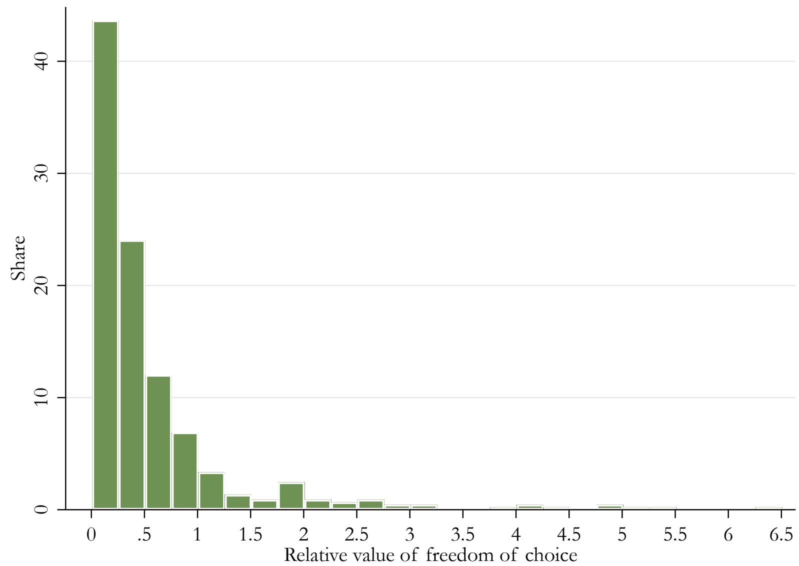
3.4.3 Impact Of Situational Specifications

Many papers suggested that both context related factors and individual characteristics matter for the individual's perspective on paternalism (among others Jung and Mellers, 2016; Hagman et al., 2015; Bartling et al., 2014).[18] As a consequence the literature suggests that there is no "universal" value of freedom of choice, but the valuation of a free decision rather depends on various factors, including the nature of the situation in which the restriction is imposed, the context, the kind of policy and others.

In our experiment, we systematically vary two situational factors – lottery type and stake size – in order to examine if they affect occurrence and size of the intrinsic value of freedom of choice. Moreover, we can investigate whether the intrinsic value of freedom of

[18] Bartling et al. (2014) find in their experiment that the intrinsic value of choice is not a fixed amount of money, but it is context specific and increases in stake sizes and decreases in the degree of conflict between principal and agent.

Figure 3.6: Relative intrinsic values of freedom of choice



Notes: Figure 3.6 displays the share of the observed relative, intrinsic values of freedom of choice. The relative VoF evaluates the absolute individual VoF in relation to the expected value of the respective lottery, $VoF_{i,s}^{rel} = \frac{VoF_{i,s}}{E(lot)}$.

choice varies with the existence of a positive instrumental value of removing a restriction.

We estimate the following model:

$$VoF_{i,s} = \beta_0 + \beta_{LS}LS_s + \beta_{RLS}RLS_s + \beta_{LOSS}LOSS_s + \beta_X X_i + \epsilon_i \tag{3.3}$$

with

$$VoF_{i,s} = max(0, wtp_{i,s} - iv_{i,s}) \tag{3.4}$$

Equation 3.4 is a condensed version of equation 3.2 that makes clear that our dependent variable $VoF_{i,s}$ is censored at 0. This is why, we use a tobit model to regress $VoF_{i,s}$ on the following variables: binary variables for each lottery type LS_s, RLS_s, and $LOSS_s$, using the 50/50 lottery as baseline and X_i a vector of individual level control variables. The results are presented in column 1 of table 3.3. The VoF differs substantially between the lottery types, with the RLS and the LOSS lottery having significantly lower VoFs compared to the 50/50 lottery, which is in line with the lower average rejection rate for these lottery types. In the second column, we include a dummy variable indicating a higher stake size and find that the VoF increases significantly with higher amounts at stake, in line with the findings of Bartling et al. (2014). Column 3 includes a dummy variable indicating, if in the situation at hand abolishing the restriction has a positive instrumental value. While the effect is positive, it is not statistically significant.[19] In the fourth column, we compare all situational specifications together, also accounting for interaction effects between stake size and lottery type. While the stake size dummy itself does not have a significant effect on the VoF anymore, the interaction effects between the reversed long shot/ the loss lotteries and high stake sizes turn significant. This results suggests, that the stake size especially matters in the RLS lottery and the LOSS lottery, while for the other lottery types the stake size differences are less pronounced.[20] The overall negative effects of the RLS and LOSS lottery remain. For a graphical representation of the mean VoF by lotteries and stake size, also see appendix figure 3.D3. The last model includes individual characteristic and background controls and we see that the results remain similar.

Next to the effects on the magnitude of the VoF, we checked if the above findings hold also for the *occurrence* of a positive VoF. Therefore, we repeat the analysis of table 3.3, but instead of the continuous VoF, we use a binary variable indicating a positive value of freedom of choice as outcome and apply a logit model. Table 3.D3 in the appendix presents the estimations. Overall, the results show similar patterns as before with some slight deviations. First, we see in column 1 that although the measured VoF is smaller for the loss lottery type, the VoF does not occur less often for the loss lottery. Second, the effect of the high stakes dummy on the presence of the VoF is neglectable in contrast to the significant positive impact on the size. That is, higher stakes lead to a higher VoF in magnitude, but do not trigger an intrinsic value of freedom of choice more often. This means that the effect of the high stakes seem to be rather mechanical than intrinsically motivated. Last, we see that the dummy indicating an instrumental value of freedom of choice turned significant on a 10% level. This can be interpreted that if abolishing a restriction has a positive instrumental value, a VoF is triggered more often, but given that there is a positive VoF the size is not significantly different.

Taking everything together, there are two main take-aways: first, both the size and the occurrence of a positive VoF substantially vary between different situations, even though we only vary the situational context with the decision context under risk and

[19] If the instrumental value itself is included instead of the dummy, the effect turns negative. Although this seems unintuitive at first, it is allegeable. By definition, the size of the instrumental value negatively influences the VoF, see equation 3.2. Intuitively, if no/only a small instrumental value is involved and individuals nevertheless rejected the restriction, this is a stronger signal about their valuation of freedom of choice compared to rejecting the restriction when there is a higher positive instrumental value of removing the constraint. Hence, the lower the instrumental value of removing the restriction, the higher the measured VoF with respect to a similar willigness to pay. However, if we only consider if there was an instrumental value or not, we observe that the presence of an instrumental value positively influences the VoF.

[20] A Wilcoxon signrank test comparing the VoF between stake sizes (for all situations separately) reveals: in situations with a reversed long shot or a loss lottery both with the unfavorable fixed payment and the neutral fixed payment, the VoF varies significantly between stake sizes ($p<0.05$), whereas in all other situations the VoF does not differ significantly between stake sizes.

Table 3.3: Effect of situational specifications on the VoF (tobit model)

	Tobit - VoF				
	(1)	(2)	(3)	(4)	(5)
LS	-0.164			-0.215	-0.183
	(0.307)			(0.339)	(0.335)
RLS	-0.754***			-1.168***	-1.099***
	(0.280)			(0.375)	(0.376)
LOSS	-0.732**			-1.107**	-1.054**
	(0.352)			(0.432)	(0.426)
High Stakes		0.674***		0.323	0.357
		(0.199)		(0.372)	(0.360)
Instr. Value (dum)			0.471	0.437	0.320
			(0.334)	(0.333)	(0.299)
LS*high stakes				0.113	0.048
				(0.589)	(0.581)
RLS*high stakes				0.762*	0.712
				(0.459)	(0.451)
LOSS*high stakes				0.755*	0.706
				(0.456)	(0.451)
N	1504	1504	1504	1504	1504
Controls (background)	No	No	No	No	Yes
Controls (ind. characteristics)	No	No	No	No	Yes

Notes: Table 3.3 shows the effects of situational characteristics on the observed VoF for 94 individuals in 16 situations each. In column 5 we include the following background characteristic: gender, math grade, age, disposable money in 100 €, experience with prior experiments and the following individual characteristics: risk attitudes, measures for self control, trust, locus of control, positive and negative reciprocity and Big Five personality traits. The individual characteristics and the math grade are standardized with mean 0 and standard deviation 1. Experience with experiments is a dummy indicating if someone has participated in more than 2 experiments. We use a tobit model with clustered standard errors at the individual level. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

hence in a relatively marginally way.[21] Second, it seems that in situations, where individuals are less willing to take risk, like in the reversed long shot lottery and the loss lottery, individuals show a lower rejection rate and also a lower VoF. This indicates that

[21] Figure 3.D4 in the appendix also supports this finding. The graph shows the share of individuals who indicated in the questionnaire if they thought that paternalistic action was acceptable for different situations (drugs, driving, environment, compulsory education, smoking, gambling, cannabis, health, meat consumption and saving). The acceptance rate varies considerably between 91% acceptance for paternalistic actions regarding drugs to only 39% acceptance regarding savings. This supports the finding that the perception of paternalistic policies is dependent on the decision context.

individuals feel less troubled to be restricted to a safe payment in these situations. This is also reflected by the positive impact of the presence of the instrumental vale on the occurrence of the VoF. Our findings are in line with the literature concerning the sensitivity of the intrinsic value for free choice to the decision context (e.g. Bartling et al., 2014). Although we find some variation with respect to the stake sizes as suggested by the literature, the evidence does not allow for a coherent conclusion in that respect.

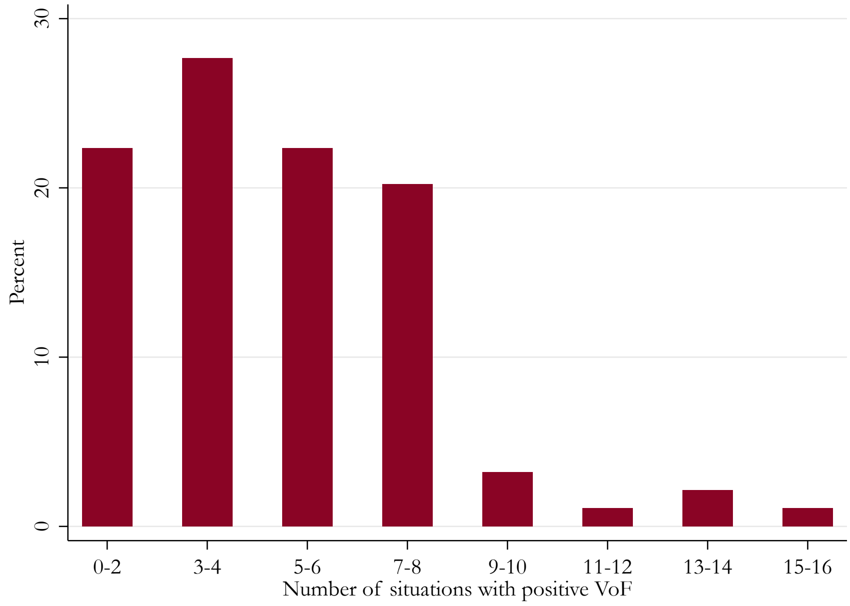
3.4.4 Individual Level Analysis

Similarly to the notion that a universal value of freedom of choice does not exists, the literature also suggests that there is no "one policy fits all" approach. Multiple studies suggested that the acceptance of a policy depends on individual characteristics and preferences (e.g. Pedersen et al., 2014; Jung and Mellers, 2016; Hagman et al., 2015; Sunstein et al., 2019). In this section, we will explore if our data confirms the finding, that the perception of a paternalistic policy is not only context specific, but also driven by individual characteristics.

To investigate this, we first analyse observed heterogeneity with respect to the frequency of situations in which individuals reveal a positive VoF. So far, we find that in a substantial fraction of situations (roughly 30%) a positive VoF occurs. This could be driven by some individuals who generally show a VoF in most of the decisions or by a larger fraction of individuals that only sometimes show a value of freedom of choice. Given that section 3.4.3 demonstrates that the VoF occurs context specific, we assume the latter will be true. We start with checking if we observe individuals that always (never) behave (non-)conformist. Overall, there are only a few individuals who act similar in *all* decision situations. Three participants (3.2%) accept the restriction in all situations and never state a positive willingness to pay, while four (4.3%) individuals always behave non-conformist and reject the restriction in all situations. With respect to the VoF, only one participant always shows a positive value of freedom of choice and the decisions of nine (9.6%) individuals never lead to a positive VoF. The vast majority of individuals must therefore show different kind of behavior across the decision situations. The average number of situations in which an individual reveals a positive value of freedom of choice is 4.8 times (out of 16 decisions in stage II of which around 45% involve an instrumental value). Figure 3.7 shows the distribution of how frequently one individual showed a positive VoF. It is apparent, that most individuals *sometimes* reveal a positive value of freedom of choice, but not always, and most participants showed a value of freedom of choice in less than half of the situations in stage II. Still, we observe quite some heterogeneity between individuals with respect to the frequency of decisions, where a positive VoF was found.

Next, we investigate how individual characteristics shape the attitude towards a paternalistic restriction. On that account, we explore the correlation between the VoF and individual self-control, trust, risk attitudes, positive and negative reciprocity and

Figure 3.7: Share of individuals, who show a positive VoF a certain amount of times



Notes: Figure 3.7 displays the fraction of individuals who showed a positive value of freedom of choice in a specific number of decisions. Each of the 94 individuals made 16 restricted decisions, so we observe a minimum of 0 positive VoFs across all situations and a maximum of 16 positive VoFs.

some background characteristics like gender, age or GPA.

Table 3.4 presents the results from a tobit model similar to the one described by equation 3.3. In column 1 and 2, we pool all decisions together and cluster standard errors at the individual level, varying the set of control variables.[22] Unlike Pedersen et al. (2014), we do not find any gender differences for the value of freedom of choice. Instead, we find that older individuals have on average a lower VoF and thus seem to suffer less from being restricted in their choices.[23] Further participants' last math grade is negatively correlated with the size of the VoF.[24] One explanation could be that participants with a higher math grade are better in calculating the instrumental value of abolishing a restriction and have a stronger preference for consistency, trying to adjust their behavior between the two stages by stating a $wtp_{i,s}$ closer to $iv_{i,s}$. This would imply that the positive VoF we observe can rather be seen as a deviation of a consistent behavior, rather than a conscious decision. However, if we revisit the scatter plot of figure 3.4 including only students in the highest quartile of the math grade

[22] In the first column, we control for lottery type, stake size and kind of restriction. In the second column, we additionally control for the Big Five personality traits, a locus of control measure, disposable income and if the participants is experienced with economic experiments (measured by a dummy variable with value 1 if the subject participated in more than two experiments).

[23] Although, there is some heterogeneity in age in our sample, 90% of participants are between 18 and 30 years old. Therefore, it would be helpful to revisit the effect of age on the intrinsic value of freedom of choice with a more diverse sample.

[24] Grades are coded as such that higher grades imply better performance.

Table 3.4: Effect of individual characteristics on the VoF (tobit model)

	Tobit - VoF		Tobit - mean VoF
	(1)	(2)	(3)
Male	0.189	0.413	0.036
	(0.466)	(0.443)	(0.162)
Age	-0.100***	-0.101***	-0.030***
	(0.028)	(0.030)	(0.010)
Math grade	-0.409**	-0.382**	-0.131*
	(0.208)	(0.191)	(0.067)
Risk attitude	0.310	0.275	0.110
	(0.226)	(0.222)	(0.077)
Self control	-0.054	-0.439	-0.169
	(0.218)	(0.331)	(0.114)
Trust	0.170	0.274	0.055
	(0.222)	(0.330)	(0.118)
Negative reciprocity	0.089	0.059	-0.004
	(0.205)	(0.203)	(0.070)
Positive reciprocity	-0.130	-0.204	-0.041
	(0.166)	(0.149)	(0.060)
N	1504	1504	94
Controls (Big Five; LOC)	No	Yes	Yes
Controls (experience, income)	No	Yes	Yes
Controls (lottery specifications)	Yes	Yes	N/A

Notes: Table 3.4 shows the effects of individual characteristics on the observed VoF (column 1 to 2) and the overall mean VoF (column 3) for each participant for 94 individuals in 16 situations each. We use a tobit model with clustered standard errors at the individual level. In the first model we control for lottery type, stake size and kind of restriction. In the second and third model we control for measures of the Big 5 personality traits, a measure of locus of control, experience with experiments and disposable income in 100 €. The individual characteristics were elicited in the survey subsequent to the experiment. The variables math grade, risk attitude, self control, trust, locus of control, negative and positive reciprocity, extraversion, agreeableness, conscientiousness, neuroticism and openness are standardized with mean 0 and standard deviation 1. Experience with experiments is a dummy indicating if someone has participated in more than 2 experiments already. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

distribution (see figure 3.D5 in appendix), we do not see a more consistent pattern of the eclicited willingness to pay in comparison to the instrumental value. Another possible interpretation for this finding is suggested by Hagman et al. (2015). They find, that individuals perceive paternalistic nudges as less intrusive to freedom of choice, if they are more prone to analytical thinking. This effect might be explained by the fact, that individuals with a more analytical way of thinking are less affected by paternalistic policies and hence see nudges less as a threat to their freedom of choice. If we presume that higher math grades are correlated to the tendency to think in an analytical way, a comparable mechanism could explain our finding, assuming that the effect also holds true for rigid paternalism.

Additionally, we look at risk preferences, self-control, trust and negative and positive reciprocity.[25] We hypothesize that people who are more risk-seeking might not only have a higher instrumental value from abolishing a restriction, but also in general feel more constraint in their autonomy facing a restriction, that stops them from taking risk. We indeed find a positive correlation between the willingness to take risks and the VoF. Further, we find that higher self-control leads to a lower VoF. However, both effects are not statistically significant. The negative correlation between self control and the VoF is in line with the finding of Pedersen et al. (2014) showing that individuals with good self-control are more favorable towards rigid paternalism presumably because of fairness concerns. While people with high self-control can resist temptation better and therefore are less affected by paternalistic policies, they want individuals with lower self-control to be stopped from imposing externalities on others as well. Next, we examine if a measure of trust is correlated with the elicited VoF and find no coherent correlation. Sunstein et al. (2019) find that trust in public institutions can enhance approval for nudging, but other forms of trust (social trust or trust in other people) do not influence approval. As we only elicit a general trust measure, we cannot investigate this relationship in more detail. Lastly, we check if reciprocity correlates with the VoF. A connection between reciprocity and the VoF could indicate if - contrary to the intention of our experimental design - social preferences between the participants and the experimenter (i.e. the paternalist) play a role in the participants' behavior. In that case, we would expect that especially a higher tendency for negative reciprocity positively influences the VoF, as a sign of reactance. Though, we do not find coherent evidence for a correlation between reciprocity and the VoF. Including additional controls (Big Five measures, locus of control, experience with laboratory experiments and income) in the second column does not change the results.

In the last column, we test if results also hold for the individual mean VoF over all situations. As the observed 1504 situations are not independent, but stem from 94 individuals that act in 16 situations each, we use the last specification to check, if we overestimate the individual level correlations. The results are qualitatively similar but smaller in size.

We again repeat a similar analysis with a dummy indicating the occurrence of a positive VoF as dependent variable and a logit specification. Results can be found in table 3.D4 in appendix. The results show a very similar pattern compared to the

[25] The following variables from table 3.4 are standardized with mean 0 and standard deviation 1: math grade, risk attitude, self control, trust, negative and positive reciprocity.

tobit specification of table 3.4 with the exception that the occurrence of the VoF is not statistically significantly affected by the math grade, contrary to the size.

Overall, the individual level analysis reveals some heterogeneity in the size and existence of the intrinsic value of freedom of choice by individual characteristics. However, the small sample size and a following deficiency to detect smaller effects limit the interpretation of the results. Nevertheless, the results allow for two conclusions: First, we see that - in line with the literature on nudging - individual characteristics seem to play a role for an incentivized reaction towards a strict paternalistic policy. We find similar relations compared to the literature, e.g. a negative effect of self-control on the acceptance of strict paternalism, suggesting that the perception of strict and libertarian policies are related. Third, our design did not trigger a reaction driven by social preferences towards the experimenter (negative reciprocity). As we intended to set up an experimental environment to measure the value of freedom of choice independent of social preferences, that confirms the validity of our design and framing.

3.5 Conclusion

Paternalistic policies are an important and widely used tool for governments and institutions to guide individuals towards better decision making. So far, the literature revolves mainly around the two questions: Is it ethically justified that the government encroaches the individual's freedom of choice to remedy behavioral biases? And: Which policies are effective in manipulating behavior in a desired way? We aim to fill a gap in the existing literature by investigating if individuals value their freedom of choice in the context of a paternalistic constraint and examine if they are also willing to give up money in order to retain a full choice set.

For this purpose, we use a decision situation involving risk to test how individuals react to a paternalistic intervention, that restricts their freedom of choice. The use of a standard decision situation allows us to disentangle the individuals' instrumental value from the intrinsic value of a free choice set. We show that in a substantial fraction of situations, individuals reveal a positive intrinsic value of freedom of choice, that exceeds the instrumental value of removing a restriction. We derive four main results: First, individuals reject a paternalistic restriction in a substantial fraction of situations (48.2%) by giving up money in order to regain a full choice set. In 14.4% of situations, individuals do this even though it does not come at an instrumental value. Second, in approximately one third of all situations individuals show a willingness to pay that exceeds the instrumental value. Thus, we attest that individuals show an intrinsic valuation of freedom of choice, which in this setting amounts to around 2€ on average per decision situation. Third, both the occurrence and the magnitude of the intrinsic value of freedom of choice is context specific and varies in lottery types. Fourth, the vast majority of individuals shows an intrinsic value of freedom of choice in at least some situations. At the same time only a small fraction of participants shows a similar behavior in reaction to the restriction in all situations.

This study contributes to the literature about the perception of paternalism in two important ways. On the one hand, this is one of only few *incentivized* studies examining the reaction towards a paternalistic intervention and as such complements the survey literature with experimental evidence. On the other hand, this is - to our best knowledge - the only study that analyses the (monetary) intrinsic value of freedom of choice in a setting that does not involve a bilateral set up or any kind of social interaction. The latter is important as social preferences have been shown to be a strong driver for the refusal of giving away autonomy (see e.g. Fehr et al., 2013; Ferreira et al., 2020).

Our study also paves the way for further studies investigating the context dependency of the perception of paternalism, as well as the research on how individual characteristics shape the reaction to paternalism. Our study confirms previous work on libertarian paternalism (e.g. Arad and Rubinstein, 2018; Loewenstein et al., 2015; Felsen et al., 2013) in that the situational context is crucial for the assessment of the attitude towards strict paternalism as well. In our experiment, some comparatively minor variations in situational specifications already affected the reaction towards the policy significantly. The same holds true for the results on individual level differences, that show similar patters as previous studies on libertarian paternalism (e.g. Pedersen et al., 2014; Hagman et al., 2015). Prospectively, it is important to broaden both the incentivized analysis and the analysis of *strict* paternalism to different kind of decision contexts, policy designs and target groups.

Overall, our findings also have broader implications for policy design. First and foremost, we can back up the intuitive notion and underlying assumption of the discourse about paternalism that an intrinsic value for freedom of choice exists. Further, we can confirm that the intrinsic value of free choice is not only an abstract concept, but indeed has a monetary representation. Moreover, the pecuniary value of freedom of choice seems to be of substantial importance, as it amounts to 60.5% of the expected value of the decisions under risk on average (based on positive VoFs) already in an experimental set up. This implies that paternalistic policies come at indirect costs, that need to be taken into account when assessing the consequences of a paternalistic constraint. Besides, the fact that the reaction to a paternalistic policy is sensitive to both contextual and individual factors, should raise awareness that policy design should take both into account. Further research along these lines can eventually help policy makers to find the right intervention based on the situation at hand and the target group. In short, our findings can serve as basis for further research on the perception of paternalism and informs the debate on the ethical justification of paternalistic policies.

Appendix 3.A Instructions

General Explanations

Welcome to today's economics experiment.

During this experiment you have the chance to earn a substantial amount of money. How much money you earn, depends to a large degree on your own decisions. Please read the following instructions carefully! In case of questions, please raise your hand out of the cubicle – we will come to your seat.

During the experiment it is not permitted to talk to other participants, use mobile devices or run any other software on the computer. If you do not comply with these rules, you will be excluded from the experiment and all payments.

At the end of the experiment you are paid according to your decisions in cash.

On the following pages, the experimental procedure is explained in detail.

The experiment: 24 decision situations

In the experiment you will face 24 different decision situations. In general, you have to decide between two alternatives in all of the 24 decisions: alternative A is always an investment opportunity, alternative B is always a safe payment.

This is an example for an investment opportunity:

With 50% probability you will get 5€ and with 50% probability you will get 0€.

The winning probability and the amount of money (in €), that can be won, vary between the different decision situations. We use decision tables to present the decision in every situation. The table either consists of **multiple rows (type A)** or **only one row (type B).** The order in which the decision situations appear are randomly chosen. However, all participants take the same decisions.

To determine your payoff, one of the 24 decision situations is randomly chosen at the end of the experiment. Your actions in this specific decision situation will be decisive for your payment.

1

Figure 3.A1: Experimental instructions (translated)

Decision situations - type A

In decision situations of type A you have to make multiple decisions that are documented with the help of a decision table. In each row of the table, you can choose between two alternatives: alternative A (an investment opportunity) and alternative B (a safe payment). The safe payment varies for each decision, hence each row.

Decision situation type A, example:

1 von 24

Alternative A: Investitionsgelegenheit		Alternative B: Sichere Auszahlung
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	0 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	0.5 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	1.0 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	1.5 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	2.0 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	2.5 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ◉ ○ Alternative B	3.0 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ○ ◉ Alternative B	3.5 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ○ ◉ Alternative B	4.0 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ○ ◉ Alternative B	4.5 €
5 € mit 50 % oder 0 € mit 50 %	Alternative A ○ ◉ Alternative B	5.0 €

Figure 1

After deciding for each row in the table, you will see a second, smaller decision table. The aim of the smaller table is to measure your preferences more precisely. The smaller table follows the same concept as the bigger decision table.

Every single decision in the table is important and might be relevant for your payment.

The computer randomly chooses one of the rows from the decision table, in case a decision situation of type A is chosen for your payment. Your decision in the respective row will then be implemented. This means:

- If you chose the **safe payment** in the randomly selected row, you will get paid the respective safe amount.
- If you chose the **investment opportunity** in the randomly selected row, the respective lottery will be drawn by the computer and the result of the lottery draw determines your payment.

2

Figure A1 (ctd.): Experimental instructions (translated)

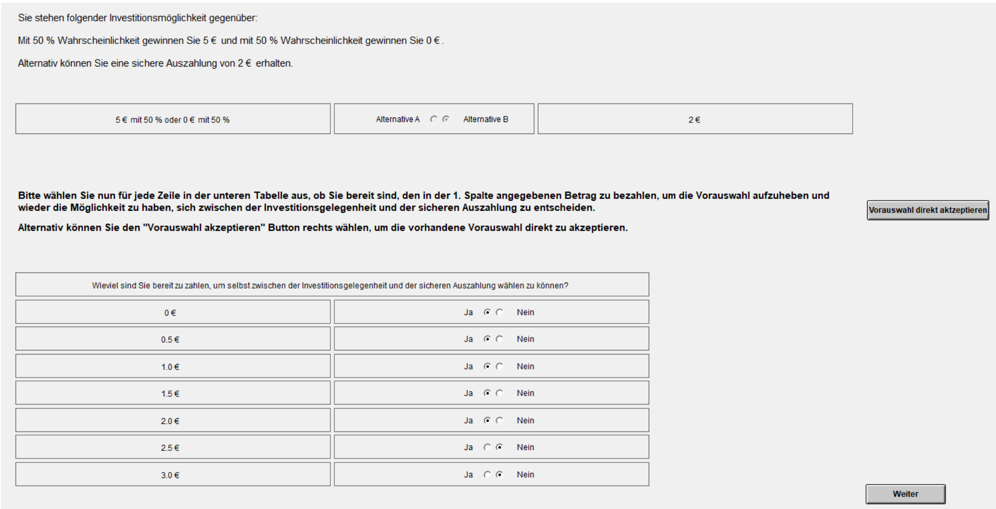
Decision situations - type B

In decision situations of type B you are facing a decision between an investment opportunity and **one** safe payment (hence, a decision table with only one row). Besides, one of the options is already pre-selected for you and implemented. In this case, you have to options:

Either you directly accept the pre-selected option and click the respective button that leads you to the next decision situation.

Alternatively, you can indicate your willingness to pay to being able to take the decision yourself, with the help of another decision table. As before, you will need to decide for each table row whether you are willing to pay a certain amount of money to regain the capability to choose yourself in this decision situation.

Decision situation type B, example:


Sie stehen folgender Investitionsmöglichkeit gegenüber:

Mit 50 % Wahrscheinlichkeit gewinnen Sie 5 € und mit 50 % Wahrscheinlichkeit gewinnen Sie 0 € .

Alternativ können Sie eine sichere Auszahlung von 2 € erhalten.

5 € mit 50 % oder 0 € mit 50 %	Alternative A ◯ ◉ Alternative B	2 €

Bitte wählen Sie nun für jede Zeile in der unteren Tabelle aus, ob Sie bereit sind, den in der 1. Spalte angegebenen Betrag zu bezahlen, um die Vorauswahl aufzuheben und wieder die Möglichkeit zu haben, sich zwischen der Investitionsgelegenheit und der sicheren Auszahlung zu entscheiden.

Alternativ können Sie den "Vorauswahl akzeptieren" Button rechts wählen, um die vorhandene Vorauswahl direkt zu akzeptieren.

Vorauswahl direkt aktzeptieren

Wieviel sind Sie bereit zu zahlen, um selbst zwischen der Investitionsgelegenheit und der sicheren Auszahlung wählen zu können?	
0 €	Ja ◉ ◯ Nein
0.5 €	Ja ◉ ◯ Nein
1.0 €	Ja ◉ ◯ Nein
1.5 €	Ja ◉ ◯ Nein
2.0 €	Ja ◉ ◯ Nein
2.5 €	Ja ◯ ◉ Nein
3.0 €	Ja ◯ ◉ Nein

Weiter

Figure 1

Every single decision is important and might be relevant for your payment.

In case, a decision situation of type B is chosen for your payment and you have directly accepted the pre-selected option in this situation, the pre-selected option will be implemented. In the example above, you would receive a safe payment of 2€.

In case, a decision situation of type B is chosen for your payment and you have **not** accepted the pre-selected option in this situation, the computer randomly chooses a row of the lower decision table. Your decision in the respective row will be implemented.

3

Figure A1 (ctd.): Experimental instructions (translated)

This means:

- If you answered **„yes“** in the respective row, you will be able to choose yourself between the investment opportunity and the safe payment. The price for getting rid of the pre-selected choice is determined by the row chosen and will be deducted as costs from your payment.
- If you answered **„no“** in the respective row, you indicated that you are not willing to pay the costs to choose between the investment opportunity and the safe payment yourself. Hence, the pre-selected choice will be implemented.

Your decision in the lower table determines if the pre-selected choice is implemented or if you can choose yourself between the two options.

Summary and general remarks

You will face 24 decision situations in total. At the end of the experiment, **one** decision situation will be randomly selected by the computer. This decision situation will then determine your payment.

Hint: You should approach every decision in all rows of every table just as if that would be the only decision you are making, as every single decision can be relevant for your payment.

In general, you can still change your decisions as long as you haven't clicked the "next"-button during the experiment. As soon as you have completed all decisions in the decision table, press the "next"-button at the lower right corner of the screen. This will lead you to the next screen with a new decision situation.

At the end of the experiment – after the 24 decision situations – there will be a couple of screens with further questions, right before you will receive your payment for today's experiment. If you answer all questions of the survey carefully, you will receive 10€ on top of the payment, that you earned during the decision situations of the main experiment.

After filling in the survey, you will be informed about the decision situation that is relevant for your payment and the amount you earned.

Excercises and comprehesion questions

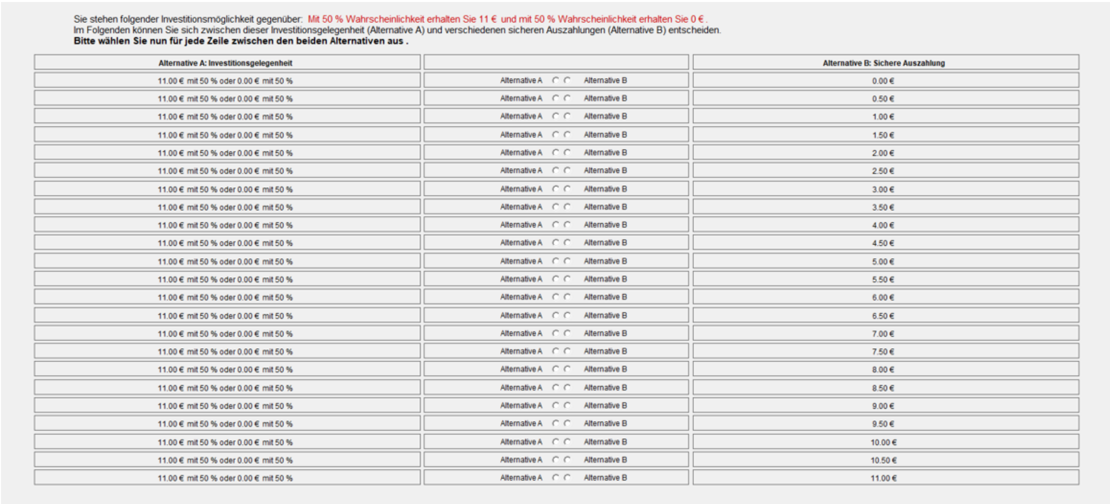
Before the main experiments starts with the 24 decision situations, we ask you to answer some exercise questions about the decision situations. Going through these questions should help you to get familiar with the decision situations.

In case of questions – now or during the exercises – please raise your hand out of the cubicle. One of the experimenters will come to your seat and answer your questions.

4

Figure A1 (ctd.): Experimental instructions (translated)

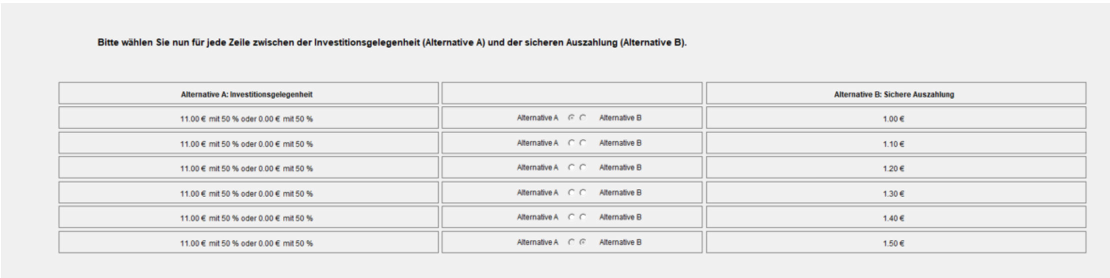
Appendix 3.B Screenshot Experiment



Sie stehen folgender Investitionsmöglichkeit gegenüber: Mit 50 % Wahrscheinlichkeit erhalten Sie 11 € und mit 50 % Wahrscheinlichkeit erhalten Sie 0 € .
Im Folgenden können Sie sich zwischen dieser Investitionsgelegenheit (Alternative A) und verschiedenen sicheren Auszahlungen (Alternative B) entscheiden.
Bitte wählen Sie nun für jede Zeile zwischen den beiden Alternativen aus .

Alternative A: Investitionsgelegenheit		Alternative B: Sichere Auszahlung
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	0.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	0.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	2.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	2.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	3.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	3.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	4.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	4.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	5.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	5.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	6.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	6.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	7.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	7.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	8.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	8.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	9.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	9.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	10.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	10.50 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	11.00 €

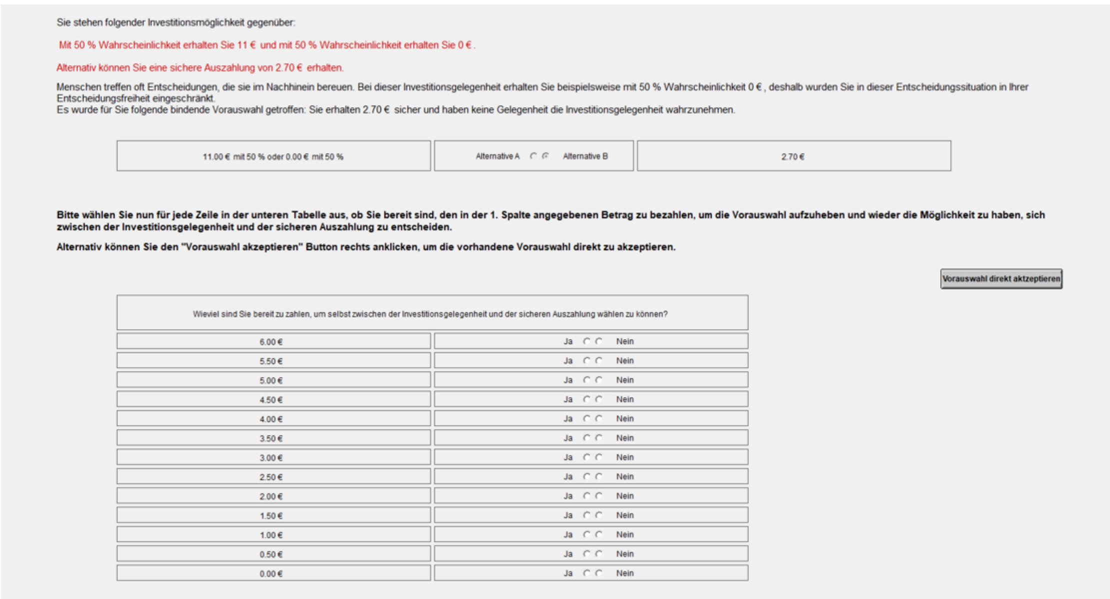
Figure 3.B1: Unrestricted decision situation, increasing safe option



Bitte wählen Sie nun für jede Zeile zwischen der Investitionsgelegenheit (Alternative A) und der sicheren Auszahlung (Alternative B).

Alternative A: Investitionsgelegenheit		Alternative B: Sichere Auszahlung
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.00 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.10 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.20 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.30 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.40 €
11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	1.50 €

Figure 3.B2: Unrestricted decision situation, detail table



Sie stehen folgender Investitionsmöglichkeit gegenüber:

Mit 50 % Wahrscheinlichkeit erhalten Sie 11 € und mit 50 % Wahrscheinlichkeit erhalten Sie 0 € .

Alternativ können Sie eine sichere Auszahlung von 2.70 € erhalten.

Menschen treffen oft Entscheidungen, die sie im Nachhinein bereuen. Bei dieser Investitionsgelegenheit erhalten Sie beispielsweise mit 50 % Wahrscheinlichkeit 0 € , deshalb wurden Sie in dieser Entscheidungssituation in Ihrer Entscheidungsfreiheit eingeschränkt.
Es wurde für Sie folgende bindende Vorauswahl getroffen: Sie erhalten 2.70 € sicher und haben keine Gelegenheit die Investitionsgelegenheit wahrzunehmen.

11.00 € mit 50 % oder 0.00 € mit 50 %	Alternative A Alternative B	2.70 €

Bitte wählen Sie nun für jede Zeile in der unteren Tabelle aus, ob Sie bereit sind, den in der 1. Spalte angegebenen Betrag zu bezahlen, um die Vorauswahl aufzuheben und wieder die Möglichkeit zu haben, sich zwischen der Investitionsgelegenheit und der sicheren Auszahlung zu entscheiden.

Alternativ können Sie den "Vorauswahl akzeptieren" Button rechts anklicken, um die vorhandene Vorauswahl direkt zu akzeptieren.

Vorauswahl direkt aktzeptieren

Wieviel sind Sie bereit zu zahlen, um selbst zwischen der Investitionsgelegenheit und der sicheren Auszahlung wählen zu können?	
6.00 €	Ja Nein
5.50 €	Ja Nein
5.00 €	Ja Nein
4.50 €	Ja Nein
4.00 €	Ja Nein
3.50 €	Ja Nein
3.00 €	Ja Nein
2.50 €	Ja Nein
2.00 €	Ja Nein
1.50 €	Ja Nein
1.00 €	Ja Nein
0.50 €	Ja Nein
0.00 €	Ja Nein

Figure 3.B3: Restricted decision situation

Appendix 3.C Default Analysis

In the questionnaire, we included two tasks, where participants faced a default making a decision. The idea is to analyse individuals reactions to defaults in the context of the main experiment to gain first insights to which extent the individual's reaction to libertarian and strict paternalistic interventions are aligned. We study two different situations involving a default: a risky investment decision comparable to the ones used by Gneezy and Potters (1997) and a choice between two pens of different color. The first task is the one of main interest, as it is designed to study behavioral reactions to a default in an investment situation involving risk, similar to the decision situations of the main experiment. In particular, participants get 2 € of endowment and could choose to invest an amount of their choice in an investment opportunity. The investment yields 2.5 cents for every cent invested with 2/3 probability and 0 cents otherwise, thus the expected value of this lottery is positive. The default is set to 0.5€ investment for all participants and could be changed without any costs.

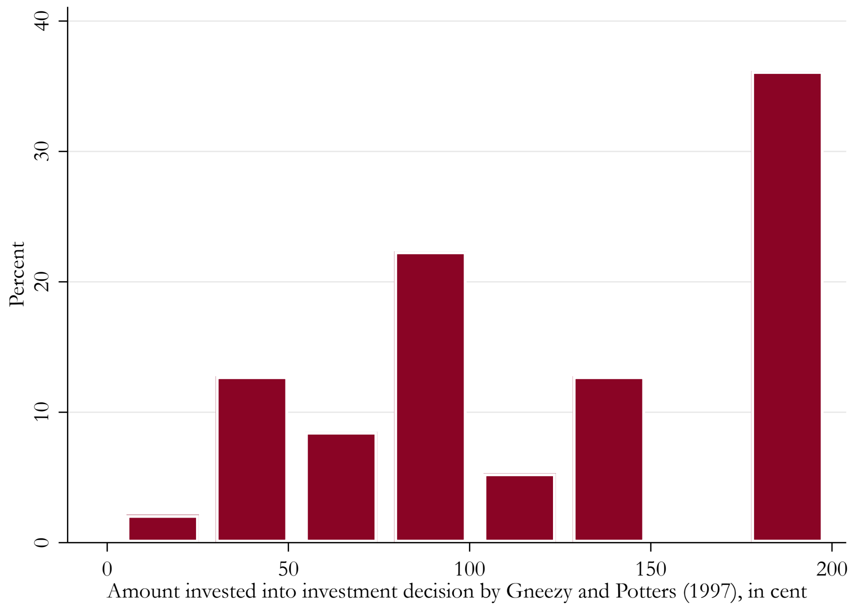
The second decision is unrelated to the decision situations of the main experiment and designed to be perceived as neutral as possible, trying to evoke only minimum preferences between the two options. Individuals are offered a pen at the end of the experiment and the pen's outside color was randomly chosen to be either blue or green (independent of the outside color both pens have a blue reservoir). The task is incentivized by providing the chosen pen at the end of the experiment. Individuals could decide to keep the pen offered or get one of the other color.

In both situations a default is set (a certain amount of investment and a pen color) and we observe whether participants stick to the default option (or not). Figure 3.C1 shows the distribution of the invested amount in the investment task in the questionnaire. The average amount invested was 1,33 €. 12 individuals sticked to the default of 0,5 €, while 2 invested less than the default and 80 individuals invested more than the default. As the investment opportunity has a positive expected value this behavior was expected. In table 3.C1 we repeat the individual level analysis with pooled situations over all individuals and lottery types and the VoF as outcome variable (see section 3.4.4). We include the absolute difference between the invested amount and the default setting of the investment task as independent variable. Results are shown in column 1 of 3.C1. The size of the deviation from the default is positively correlated to the mean individual VoF, suggesting that individuals with a higher intrinsic valuation of freedom of choice might also be less susceptible to defaults.

The results look different for the other default setting, offering a pen. 31% of individuals change the default color and choose a different pen. In the second column of table 3.C1, we also include a dummy indicating if the individual changed the pen offered by default to a different color. We do not find a positive association between changing the default and the VoF, on the contrary the correlation is negative.

Overall, our pilot study yields some first hints that individual behavior might be consistent over different types of policies, at least if a similar situation context is examined. For the neutral and unrelated default setting we however find opposite effects.

Figure 3.C1: Distribution of investment in decision under risk by Gneezy and Potters (1997)



Notes: The figure shows the distribution of invested amounts in cent in the decision situation under risk by Gneezy and Potters (1997). The default option was set to 50cent investment.

Table 3.C1: Correlation between VoF and behavior in default decisions

	Tobit - VoF	
	(1)	(2)
Abs. Diff. in €	0.720*	
	(0.379)	
Changed Default Pen		-1.245***
		(0.427)
Male	0.171	0.582
	(0.425)	(0.434)
Age	-0.104***	-0.093***
	(0.030)	(0.033)
Math grade	-0.338*	-0.346*
	(0.180)	(0.179)
Risk attitude	0.179	0.283
	(0.230)	(0.208)
Self control	-0.441	-0.433
	(0.319)	(0.302)
Trust	0.258	0.428
	(0.311)	(0.319)
Negative reciprocity	-0.017	0.069
	(0.196)	(0.185)
Positive reciprocity	-0.218	-0.227*
	(0.142)	(0.130)
N	1504	1504
Controls (Big Five; LOC)	Yes	Yes
Controls (experience, income)	Yes	Yes
Controls (lottery specifications)	Yes	Yes

Notes: Table 3.C1 shows the coorelation between the VoF and the behavior in the default situations. In the first model, the absolute difference of invested euros compared to the default of 0,5 € (the maximum possible investment was 2 €) in the investment task by Gneezy and Potters (1997) is included. In the second model, a binary variable, indicating if a participant decided for the pen, which was **not** the default option is included. Situations are pooled over 16 decision situations of 94 participants each. We use a tobit model with clustered standard errors at the individual level. In all columns we control for measures of the Big 5 personality traits, a measure of locus of control, experience with experiments, disposable income in 100 € stake size, lottery type and kind of restriction. The variables math grade, risk attitude, self control, trust, locus of control, negative and positive recirpocity, extraversion, agreeableness, conscientiousness, neuroticism and openness are standardized with mean 0 and standard deviation 1. Experience with experiments is a dummy indicating if someone has participated in more than 2 experiments already. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Appendix 3.D Additional Figures And Tables

Table 3.D1: Choice list specifications for all lotteries

		Choice list		
		minimum	maximum	increment
50/50	low	0	11	0.5
	high	0	22	1
Long-shot	low	0	14	0.5
	high	0	28	1
Reversed long-shot	low	0	6	0.2
	high	0	12	0.4
Loss	low	-4	0	0.2
	high	-8	0	0.4

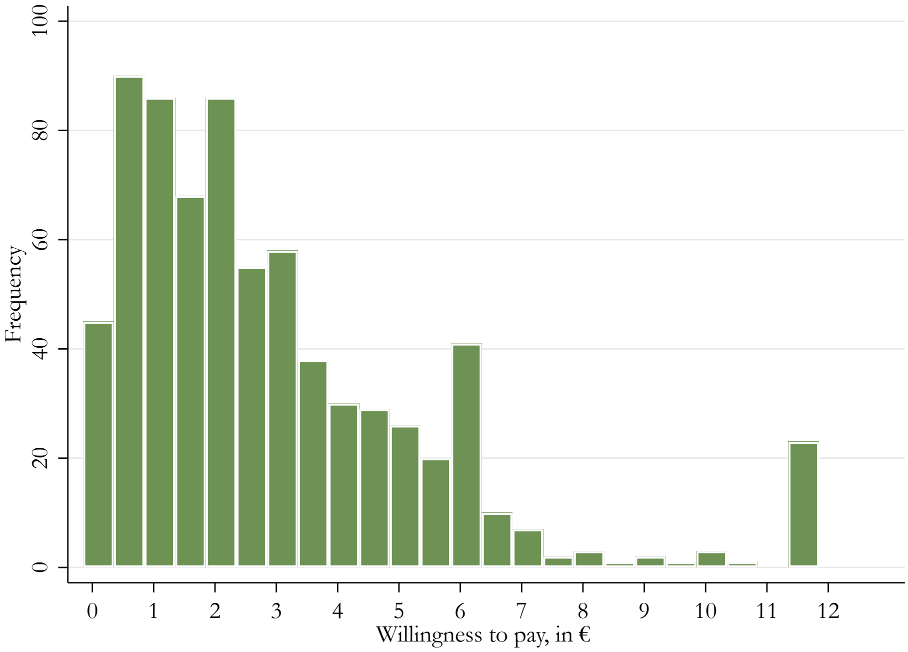
Notes: The table displays the specifications of the choice lists used in the experiment, including their minimum and maximum values and the increment between the different rows of a choice list.

Table 3.D2: Certainty Equivalent by individual characteristics

	Certainty Equivalent	
	(1)	(2)
Risk Attitude	0.716***	
	(0.174)	
Male		0.364
		(0.393)
Math grade		0.101
		(0.160)
Age		-0.000
		(0.031)
Positive reciprocity		-0.031
		(0.197)
Negative reciprocity		0.385**
		(0.184)
Disposable income, in 100 €		0.061
		(0.051)
N	1504	1504
Controls (Situation FE)	Yes	Yes

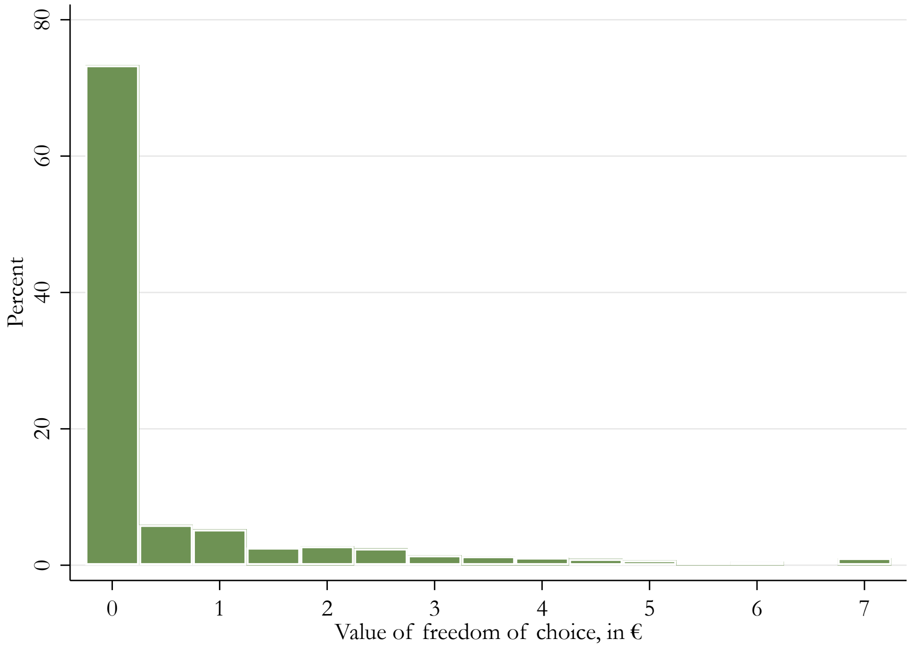
Notes: The table shows results of an OLS regression of elicited certainty equivalents on individual characteristics. Disposable income is measure in hundreds of € and math grade captures the last math grade at school (a higher grade corresponds to better mathematics skills). The variables risk attitudes, math grade, positive and negative reciprocity are standardized. In both columns, we use situation fixed effects to account for differences by lottery type and stake size. Standard errors are clustered at the individual level. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Figure 3.D1: Distribution of willingness to pay conditional on $wtp_{i,s} > 0$



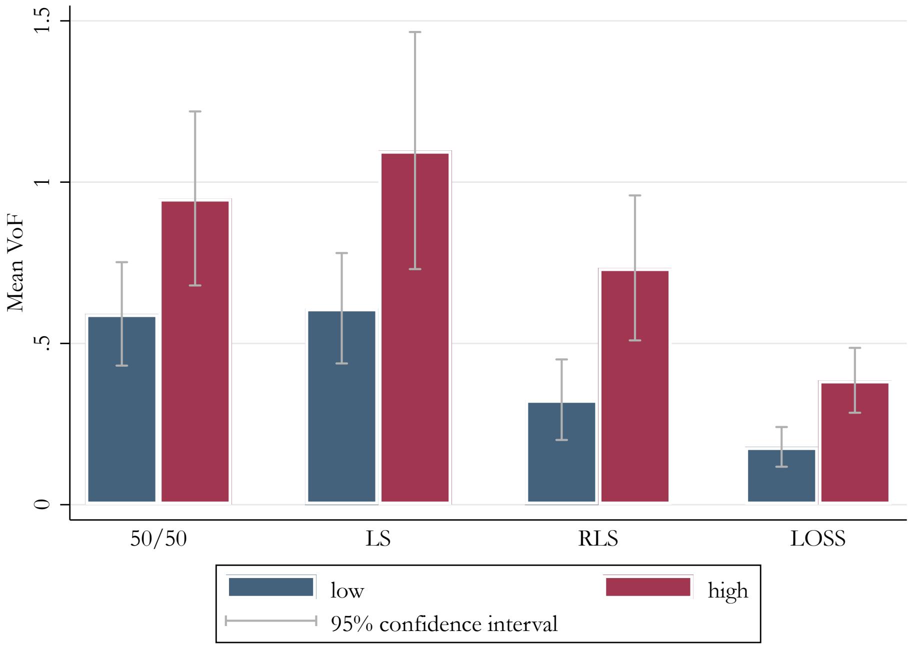
Notes: Figure 3.D1 displays the frequency of observed positive amounts of money that participants are willing to pay to abolish the restriction (in €). In 779 out of 1504 decision situations (51.8%) the willingness to pay was 0. For better readability, the graph only depicts the 48.2% of decision situations with $wtp_{i,s} > 0$.

Figure 3.D2: Distribution of VoF (highest percentile bundled at 7)



Notes: Figure 3.D2 displays the relative frequency of observed values of freedom of choice (in €). The highest percentile of observations is bundled at 7 to improve readability. In total, we observe a VoF higher than 7 in 15 (out of 1504) decision situations with the highest, observed VoF being 12.

Figure 3.D3: Average VoF by lottery and stake size



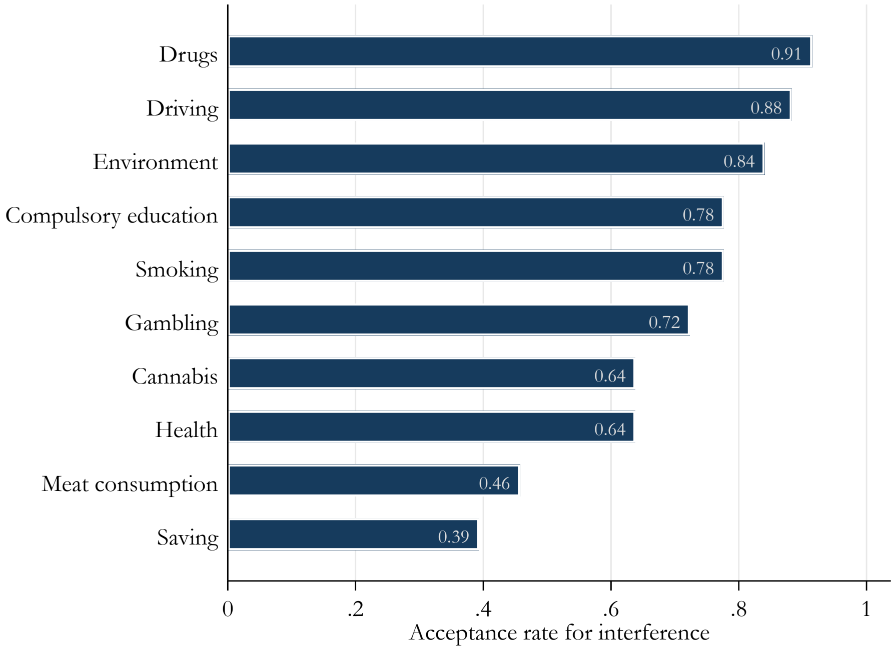
Notes: Figure 3.D3 compares the mean VoF for each lottery type by stake size (in €). The gray lines indicate the 95% confidence interval per lottery stake size combination.

Table 3.D3: Effect of situational specifications on the occurrence of the VoF (logit model)

	Logit - VoF dummy				
	(1)	(2)	(3)	(4)	(5)
LS	-0.266*			-0.219	-0.229
	(0.141)			(0.192)	(0.199)
RLS	-0.334**			-0.607***	-0.631***
	(0.151)			(0.223)	(0.231)
LOSS	0.000			-0.382	-0.406
	(0.195)			(0.256)	(0.265)
High Stakes		0.095		-0.201	-0.216
		(0.089)		(0.179)	(0.187)
Instr. Value (dum)			0.343*	0.335*	0.295*
			(0.176)	(0.179)	(0.176)
LS*high stakes				-0.118	-0.118
				(0.282)	(0.291)
RLS*high stakes				0.548**	0.573**
				(0.251)	(0.262)
LOSS*high stakes				0.800***	0.843***
				(0.253)	(0.264)
N	1504	1504	1504	1504	1504
Controls (background)	No	No	No	No	Yes
Controls (risk, self control)	No	No	No	No	Yes

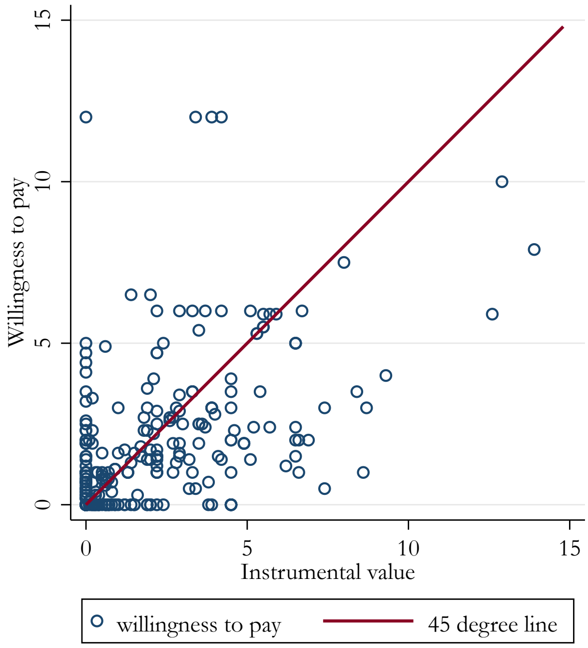
Notes: Table 3.D3 shows the effects of situational specifications on the VoF dummy, indicating if someone showed a positive VoF or not, for 94 individuals in 16 situations each. As the outcome variable is binary, we use a logit model, standard errors are clustered at the individual level. In column 1, we regress the VoF dummy on a dummy for each lottery with the 50/50 lottery as a baseline and a dummy indicating high stakes. In column 2, interaction effects between stake sizes and lottery types are added. In column 3, we include a dummy variable indicating if removing the restriction included an instrumental value for the individual. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Figure 3.D4: Acceptance rate for paternalistic intervention in different situations



Notes: Figure 3.D4 displays the results of a survey question, where participants were asked if they found that in the specific subjects paternalistic action is acceptable. The bars depict the share of participants who find paternalistic action acceptable for the respective matters.

Figure 3.D5: Relationship between willingness to pay and instrumental value (best quartile of math grade distribution)



Notes: Figure 3.4 is a scatter plot displaying the indicated willingness to pay in relation to the instrumental value of removing a restriction for all participants belonging to the best quartile with respect of the math grade. The plot show each of the 384 decision situations in stage II of the respective sample. The red line shows the 45 degree line and therefore indicates cases where individuals are willing to pay exactly the instrumental value of removing the restriction.

Table 3.D4: Effect of individual characteristics on the occurrence of the VoF (logit model)

	Logit - VoF dummy	
	(1)	(2)
Male	0.210	0.367
	(0.241)	(0.239)
Age	-0.047***	-0.053***
	(0.015)	(0.016)
Math grade	-0.164	-0.173
	(0.113)	(0.110)
Risk attitude	0.098	0.113
	(0.115)	(0.116)
Self control	-0.029	-0.126
	(0.113)	(0.172)
Trust	0.152	0.222
	(0.116)	(0.179)
Negative reciprocity	0.084	0.049
	(0.115)	(0.111)
Positive reciprocity	-0.118	-0.159*
	(0.094)	(0.088)
N	1504	1504
Controls (Big Five; LOC)	No	Yes
Controls (experience, income)	No	Yes
Controls (lottery specifications)	Yes	Yes

Notes: Table 3.D4 shows the effects of individual characteristics on the observed occurrence of the VoF by lottery type (column 1 to 4) for 94 individuals in 4 situations each. We use a logit model with clustered standard errors at individual level. In all columns we control for measures of the Big 5 personality traits, a measure of locus of control, experience with experiments, disposable income in 100 €and stake size. The variables math grade, risk attitude, self control, trust, locus of control, negative and positive reciprocity, extraversion, agreeableness, conscientiousness, neuroticism and openness are standardized with mean 0 and standard deviation 1. Experience with experiments is a dummy indicating if someone has participated in more than 2 experiments already. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

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