

# **Essays in Applied Microeconomics**

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# Introduction

Empirically studying human decision-making and belief formation lies at the core of applied microeconomics today. Traditionally, drawing on classical microeconomic theory, humans were assumed to be *rational* – that is, perfectly maximizing their preferences. In a more restrictive sense, it was often further assumed that individuals cared only about their own material payoffs. Moreover, research largely focused on decision-making, with no or very little attention paid to beliefs or internal cognitive processes.

With the advent of behavioral economics, this classical model of the decision-maker has been challenged both theoretically and empirically. Often drawing inspiration from psychology, economists began to take seriously the role of, for example, psychological heuristics and biases in belief updating, motivated reasoning, and memory constraints.<sup>1</sup> Among its most influential contributions, behavioral economics has advanced both theoretical and empirical understanding of phenomena such as non-hyperbolic discounting, social preferences, overconfidence, habit formation, salience, and limited attention.<sup>2</sup>

Somewhat later, and mostly distinct from developments in behavioral economics, applied microeconomics began imposing stricter demands on its empirical credibility (Angrist and Pischke, 2010). Researchers were increasingly expected to provide robust identification strategies to ensure that estimated effects were not driven by confounding factors. Among the available approaches, experimental methods – especially randomized controlled trials – are often regarded as the gold standard for establishing causal effects.

This dissertation contributes to both of these areas. Chapter 1 and Chapter 2 are clearly behavioral and experimental in that they evaluate whether non-classical explanations can improve our understanding of human behavior (the behavioral dimension) and they do so within tightly controlled experimental settings designed

1. For seminal psychological work on each of the three, see Kahneman and Tversky (1972), Kunda (1990), and Kahana (2012), respectively.

2. For seminal theoretical treatments, see Laibson (1997) and O'Donoghue and Rabin (1999) on non-hyperbolic discounting; Fehr and Schmidt (1999) on social preferences; Becker and Murphy (1988) on habit formation; Chetty, Looney, and Kroft (2009) on salience; and Sims (2003) on limited attention. For a survey of the empirical evidence in behavioral economics, see DellaVigna (2009).

to isolate causal effects (the experimental dimension). Chapter 3 also investigates belief formation and involves original survey-based data collection, but it is more closely situated at the intersection of cultural economics and experimental economics.

### **Chapter 1: Retelling and Memory**

This chapter investigates whether biased retelling of previously encountered information makes memory of the original information biased, too. In a randomized controlled experiment, participants first evaluated hypothetical products based on brief descriptions. They were then instructed to retell these descriptions either accurately or in an overly optimistic way (between-subjects treatment variation). One day later, all participants were asked to retell the original descriptions accurately and to recall their initial ratings.

Both the day-2 ratings and the day-2 retellings (as assessed by two independent human coders) revealed a large and highly significant treatment effect. That is, subjects who had retold the descriptions in an overly optimistic way on day 1 also provided exaggerated ratings and retellings on day 2 even though incentives at that point required accuracy.

A robustness experiment tested whether this bias was already present *immediately* after the first retelling, thus assessing whether the effect reflects a genuine *memory* distortion. While a statistically significant average treatment effect immediately after retelling was found, it was (i) substantially smaller in magnitude, (ii) more sensitive to outliers, and (iii) not accompanied by significant distributional differences.

This study provides causal evidence that biased retelling distorts memory, contributing a novel insight to the economics literature. The findings suggest that in contexts where people are likely to retell past experiences or information in a biased manner, even memory recalled under accuracy incentives may be systematically distorted. Such effects may be particularly relevant in financial, labor market or discrimination-related settings.

### **Chapter 2: Illusion of Control**

This chapter examines whether people believe desirable outcomes are more likely when they are more engaged in a process, even when this engagement is only pseudo-relevant. Specifically, it is tested whether the method of generating a random outcome (via a physical die vs. a computer) affects beliefs about winning a board game. To cleanly identify effects, beliefs are elicited not only from the players but also from an independent observer.

Such a tendency – known as *illusion of control* – has been studied extensively in psychology (beginning with Langer, 1975), but has received comparatively less

attention in economics. While the psychology literature consistently finds evidence for illusion of control (Presson and Benassi, 1996; Stefan and David, 2013), findings in economics have been more mixed. The results of this chapter align with the latter: in several specifications, no treatment effect for the main incentivized belief about winning the board game is observed (insignificant and point estimates close to zero), casting doubt on the robustness of the illusion-of-control findings in psychology. There is borderline significant evidence that participants who used a physical die perceived skill as more important (relative to luck), but this pattern is not limited to players and is also present among observers.

### **Chapter 3: Parents' Beliefs About Cultural Transmission**

Chapter 3 (co-authored with Paul Behler) explores parents' beliefs about cultural transmission. A growing body of research has focused on how economically relevant traits and preferences develop in children (Cunha and Heckman, 2007; Dohmen et al., 2012; Sutter, Zoller, and Glätzle-Rützler, 2019). This chapter builds on that literature by presenting a dedicated survey of parents and offering two main contributions.

The first part of the chapter presents detailed descriptive evidence on, for example, which agents or institutions parents believe matter for which traits at different ages, which traits they prefer to shape, and what goals and methods they pursue in this shaping. This addresses an important gap, as little is known about parents' beliefs and preferences concerning the non-genetic shaping of children's attitudes and character traits.

Second, the chapter engages with the literature in cultural economics by testing two key assumptions: imperfect empathy and substitutability (Bisin and Verdier, 2001, 2011). We empirically assess whether parents indeed evaluate their children's choices using their own (the parents') preferences (imperfect empathy) and whether they invest more effort in cultural transmission when their goals are misaligned with their child's social environment (substitutability).

We find robust evidence that approximately 60% of parents exhibit imperfect empathy. In contrast, we do not find support for substitutability: parents' transmission effort is not significantly affected by whether the environment is aligned, and the extremeness of their choices, if anything, moves in an anti-substitutability direction. That is, they tend to align with the perceived environment rather than compensating for it, particularly among liberal parents.

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# Chapter 1

## Retelling and Memory\*

### 1.1 Introduction

When making decisions and forming beliefs, people rely not only on present impressions but also on memory. As social beings, they also constantly retell past experiences or information, often under incentives that diverge from pure accuracy. This chapter connects these ideas and asks: Does biased retelling causally distort memory, biasing it in the same direction?

To address this question, a laboratory RCT was conducted over two consecutive days. On day 1, subjects read short descriptions of hypothetical products. Afterward, they provided a subjective but unbiased and incentivized rating of each product. Still on day 1, subjects thereafter recorded audio messages in which they retold the product description to other participants. Henceforth, these audio messages will be referred to as *retellings*. This is where treatment variation was introduced: between-subjects, subjects were either incentivized to retell *accurately* or *overly optimistically*. On day 2, the same subjects were invited again and asked to retell the descriptions once more, this time incentivized to be accurate no matter the day-1 treatment condition. Additionally, they were asked to provide another rating of the same products, under incentives identical to day 1. By providing the original product information before introducing treatment variation, it was ensured that the initial encoding of information was identical across treatment groups.

\* Valuable feedback from Roland Bénabou, Stefano DellaVigna, Thomas Dohmen, Sebastian Kube, George Loewenstein, Ulrike Malmendier, Ryan Oprea, and Florian Zimmermann, as well as from numerous BGSE students and PhD students from other institutions, is gratefully acknowledged.

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The main hypotheses posit that both in day-2 ratings and in day-2 retellings there is a treatment effect: relative to the control group that retold accurately on day 1, subjects who were asked to retell overly optimistically (i) provide upward-biased ratings on day 2, and (ii) produce retellings that are likewise overly optimistic. To assess the latter, independent coders evaluated each recording without access to the original product descriptions. Both hypotheses are strongly confirmed by highly significant and sizable treatment effects.

A potential concern is whether these effects reflect genuine memory effects, or whether they already emerge immediately after the biased retelling. To address this issue, a separate one-day robustness experiment was conducted, designed to capture immediate effects. Taken together, the two experiments allow it to distinguish immediate effects from delayed memory effects.<sup>1</sup> While a statistically significant average treatment effect was also found in the robustness experiment, it was (i) smaller in magnitude, (ii) more sensitive to outliers, and (iii) not accompanied by significant distributional differences.<sup>2</sup> These findings are taken to suggest that a sizable portion of the treatment effect observed in the main experiment reflects a genuine memory component.

To motivate why it is worthwhile to explore whether (and if so, to what extent) biased retelling affects memory, first focus on why the biasedness of retellings is an interesting and relevant starting point. More often than not, when people retell experiences, accuracy is rarely the primary, let alone the sole, goal, and reasons and motives for non-accurate retelling are abundant. To give some prominent examples, people often retell stories not only to transmit information, but also to entertain, to persuade, and to adapt to their audience – or rather, to their perception of the audience.<sup>3</sup> More broadly speaking, just as social norms shape behavior, conversational norms guide both what to talk about and what *not* to talk about in a given situation. See Section 1.2 for more details. Taking stock, it is reasonable to assume that (sufficiently often) retellings exhibit systematic deviations from originally encoded information, for example, by exaggerating or minimizing, omitting or adding.

Unsurprisingly, linking (biased) retelling to memory has received some attention in psychology. More generally focusing on retrieval rather than retelling specif-

1. In the context of this experiment, there is an important class of immediate effects which the robustness experiment in particular allows to control for: the effect just hypothesized could be driven by a notion of *self-persuasion* driving a potential treatment effect (Schwardmann and Van der Weele, 2019; Schwardmann, Tripodi, and Van der Weele, 2022). Loosely speaking, the idea would be that by retelling biasedly, subjects persuade themselves of a higher subjective quality than they just reported. Crucially, this self-persuasion-driven inflated assessment should also show up in the robustness experiment.

2. However, the difference in treatment effect estimates between the two experiments is only borderline significant under the winsorized specification (see Section 1.5).

3. See Vandierendonck and Van Damme (1988), Hyman (1994), Pasupathi, Stallworth, and Murdoch (1998), Bavelas, Coates, and Johnson (2000), Echterhoff, Higgins, and Groll (2005), Pasupathi and Rich (2005), Echterhoff et al. (2008), and Pasupathi and Hoyt (2010).

ically, Weldon and Bellinger (1997) and Blumen and Rajaram (2008), for instance, found that repeated retrieval strengthens recalled information, making it more likely to persist in memory. For a more detailed discussion of the psychology literature, see Section 1.2.

To underscore the relevance of the research question of this chapter for *economics* specifically, consider the following examples that connect the research question to themes that have been studied in seminal and highly influential publications in economics: overconfidence and political polarization.<sup>4</sup> Regarding overconfidence, suppose there is a tendency to retell successes more often than failures (e.g., in financial or labor market settings). Applying the research question of this chapter would then yield: Does such a tendency (in itself) make people’s recall of their performance exaggerated, and can thereby (partly) explain overconfident beliefs? With regard to polarization, assume the groups an individual conversationally interacts with are such that experiences that inform political preferences are differently socially acceptable to retell. To give a concrete example, take ‘personal experiences with immigrants’ and assume that people generally make both good and bad experiences with immigrants, and that these experiences and their relative likelihood affect people’s political preferences in this domain. At the same time, assume that retelling norms induce distorted retellings (either overly positive or overly negative, depending on the ideology). Through the lens of the research question of this chapter, polarized political preferences in the immigration domain could then potentially be (partly) explained by people distortedly retelling – and thereby distortedly remembering – experiences in line with the ideology of their peer group.

This chapter contributes to an emerging economics literature on memory, which has taken on an increasingly important role in behavioral economics for understanding patterns in belief formation and decision making that deviate from rational benchmarks. Broadly speaking, one can distinguish the memory literature into non-motivated memory and motivated memory.

In the non-motivated memory literature, several abstract key concepts have been established to enrich our understanding of how memory works in economically relevant contexts. These include rehearsal, similarity-based associativeness and interference, and representativeness.<sup>5</sup> For more details on these concepts, see Section 1.2.

In the literature on motivated memory, by contrast, memory biases are viewed as stemming from self-serving motives. For an overview of this literature, see Ame-

4. Seminal contributions on overconfidence include Malmendier and Tate (2005, 2008); for a broader overview, see Malmendier and Taylor (2015). On political polarization, see, e.g., Gentzkow and Shapiro (2010), Autor et al. (2020), and Boxell, Gentzkow, and Shapiro (2024).

5. See Mullainathan (2002), Gennaioli and Shleifer (2010), Bordalo et al. (2023), Bohren et al. (2024), Enke, Schwerter, and Zimmermann (2024), Gennaioli et al. (2024), Graeber, Roth, and Zimmermann (2024), Bordalo et al. (2025), and Jiang et al. (Forthcoming).

lio and Zimmermann (2023). In short, motivated memory has been shown to be linked to prosocial behavior, financial decisions, and IQ.<sup>6</sup> Furthermore, while most studies take place in laboratory settings, motivated memory has also increasingly been shown to matter in field settings.<sup>7</sup>

With regards to this distinction between non-motivated and motivated memory, the research design employed does *not* consider straightforwardly motivated contexts (e.g., using ego-relevant information) but instead utilizes ego-neutral information about hypothetical everyday products. That being said, a more subtle motivated-memory explanation might derive from a motivation to avoid cognitive dissonance.<sup>8</sup> For a stylized formal description of how such a biased-retelling memory effect might be either motivated or non-motivated, see Section 1.4. Importantly, though, instead of taking a stance as to whether an effect might be motivated or not, or empirically trying to disentangle them, the focus of this study is rather to establish clean evidence whether biased retelling has a causal effect on memory.

By employing actual verbal retelling, the study also speaks to an emerging literature using speech data (Galasso, Nannicini, and Nozza, 2024; Graeber, Noy, and Roth, 2024; Graeber, Roth, and Schesch, 2024; Haaland et al., 2024), as opposed to the traditionally used text data. This literature highlights that it is worthwhile to consider audio messages as a data type distinct from, for instance, free-text answers both because novel patterns emerge (Graeber, Noy, and Roth, 2024; Graeber, Roth, and Schesch, 2024), but also because speech data has certain methodological advantages (Haaland et al., 2024).

Despite how central this research question is given the discussion so far, to the best of my knowledge it has not been addressed in the economic literature with the exception of one very recent working paper (Conlon, 2025). In the psychology literature, on the other hand, this idea has received some attention even though still not as much as one might expect.<sup>9</sup> In Section 1.2, it is discussed in which sense this study differs from the existing literature and what the added value of this chapter is.

The remainder of the chapter is organized as follows. Section 1.2 discusses the relevant literature both in economics and related fields. Section 1.3 describes the

6. See Li (2013), Saucet and Villeval (2019), Carlson et al. (2020), and Galeotti, Saucet, and Villeval (2020) for prosocial behavior; Gödker, Jiao, and Smeets (2024) for financial decisions; and Chew, Huang, and Zhao (2020) and Zimmermann (2020) for IQ.

7. See Huffman, Raymond, and Shvets (2022), Müller (2022), Roy-Chowdhury (2024), and Sial, Sydnor, and Taubinsky (2025).

8. Separately, even if the effect at hand is non-motivated, in a wider sense it might be conceivable that it is a ‘spill-over’ from a general human tendency that was shaped motivatedly. That is, it could be that a non-motivated link between biased retelling and biased memory found in this study is an instance of a general human tendency that was itself coined in motivated contexts. To be explicit, this question is not addressed in this study.

9. See Anderson, Bjork, and Bjork (1994), Tversky and Marsh (2000), Marsh (2007), and Hirst and Echterhoff (2012).

research design of both the main and robustness experiments in detail. Section 1.4 provides a brief formalization of how the hypothesized memory effect might be thought of as either motivated or non-motivated. Section 1.5 presents the results, and Section 1.6 concludes.

Both experiments, as well as all analyses and hypotheses described above, were pre-registered under AEARCTR-0015038.

## 1.2 Literature

### 1.2.1 Economics Literature

The present study relates to two streams of the literature in economics: memory and speech data. They are discussed in turn.

#### 1.2.1.1 Memory Literature

There is a growing literature highlighting the importance of memory for economic belief and decision-making, in particular for understanding deviations from rational benchmarks in behavioral economics. In this memory literature, a general distinction between non-motivated and motivated memory exists. While the latter, roughly speaking, is concerned with cases in which imperfect memory can be explained by self-serving motives, the former is more concerned with distilling general, abstract insights into how memory operates.

In the non-motivated memory literature, key concepts that have been established to enrich our understanding of memory include rehearsal, similarity-based associativeness and interference, and representativeness. To give brief explanations of these terms: rehearsal refers to the repeated retrieval of a memory cue, which increases the likelihood of later recall. At the same time, when trying to access an information stored in memory, similar information might come to mind (associativeness) and may impede accurate retrieval (interference). These three concepts play central roles in several influential economic models of memory.<sup>10</sup> Representativeness formalizes the idea that representative, or stereotypical, memories are recalled more easily.<sup>11</sup>

10. Mullainathan (2002), one of the earliest models of memory, incorporates rehearsal and associativeness. Bordalo, Gennaioli, and Shleifer (2020), Bordalo et al. (2023), and Bordalo et al. (2025) use associativeness and interference to model, respectively, (i) the cueing of past experiences by choice options, (ii) probability estimation, and (iii) belief formation about novel risks. Gennaioli et al. (2024) model household inflation expectations making use of associativeness, interference and rehearsal.

11. Gennaioli and Shleifer (2010) present a model of memory-based probabilistic inference in which selective recall of information is driven by a version of Kahneman and Tversky (1972)'s representativeness heuristic. This heuristic is not as straightforward to understand, but Gennaioli and Shleifer (2010) provide an illustrative example: suppose a voter is trying to assess a candidate's quality. The candidate is either qualified or unqualified, and either familiar or unfamiliar with the

Regarding empirical evidence, Conlon (2025) shows that (biased) rehearsal of memory can generate systematic belief biases. As this working paper is the most-closely related study to this chapter, it is discussed in more detail later. Regarding further empirical evidence, Enke, Schwerter, and Zimmermann (2024) provide an experimental test of associative recall driving overreaction in expectations and market behavior. Graeber, Roth, and Zimmermann (2024) highlight that the information type (i.e., story vs. statistic) matters for selective memory, in the sense that the average impact of statistics on beliefs fades much stronger than that of stories. Moreover, Jiang et al. (Forthcoming) provide field evidence of similarity-based recall of previous experiences for a large representative sample of retail investors. Ideas of imperfect recall have also appeared in broader investigations of belief formation. For instance, Bohren et al. (2024) assess empirically how perceptions of uncertainty depend on whether learning is sequential or simultaneous, incorporating both memory and attention constraints.

Moving to motivated memory, see Amelio and Zimmermann (2023) for an overview. Theoretical treatments of motivated memory are a special case of a more general class of motivated-cognition models built around the idea that decision makers have preferences over beliefs.<sup>12</sup> At this point, it is useful to refer to the conceptualization by Bénabou and Tirole (2002) who conceive of motivated beliefs as having a *supply side* and a *demand side*. In short, the supply side addresses *how* (by what means) subjects manage to hold inaccurate beliefs, while the demand side deals with *why* (to what end) subjects hold inaccurate beliefs. Motivated memory is then one important supply-side factor in their model.<sup>13</sup>

Besides Bénabou and Tirole (2002), models of motivated memory are developed in Chew, Huang, and Zhao (2020), Hagenbach and Koessler (2022) and Fudenberg, Lanzani, and Strack (2024).

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voter's culture. The voter observes only the latter but seeks to infer the former. Under the representativeness heuristic, the voter does not update according to Bayes' rule, but exhibits "local thinking:" the voter updates as if there are only *stereotypical* candidates in the following sense. The stereotypical qualified candidate is familiar (with the voter's culture) if and only if familiar candidates are qualified with higher likelihood than unfamiliar ones – formally,  $\Pr(\text{qualified} \mid \text{familiar}) > \Pr(\text{qualified} \mid \text{unfamiliar})$  –, and likewise for the stereotypical unqualified candidate. A local thinker would then assess  $\Pr^L(\text{qualified}) = \Pr(\text{stereotypical qualified}) / (\Pr(\text{stereotypical qualified}) + \Pr(\text{stereotypical unqualified}))$ . Depending on the parameterization, this can differ starkly from the Bayesian benchmark  $\Pr(\text{qualified}) = \Pr(\text{qualified} \wedge \text{familiar}) + \Pr(\text{qualified} \wedge \text{unfamiliar})$ .

12. See Kunda (1990), Bénabou and Tirole (2002, 2006, 2011, 2016), Kőszegi (2006) for theoretical treatments of belief-based preferences. As summarized in Amelio and Zimmermann (2023), empirical evidence on motivated reasoning in general (i.e., not focussed on memory) ranges from overconfidence (Malmendier and Tate, 2005, 2008) and polarization (Kahan, 2013) to moral transgression (Babcock et al., 1995; Konow, 2000; Dana, Weber, and Kuang, 2007; Haisley and Weber, 2010; Bénabou, 2013; Di Tella et al., 2015; Gneezy et al., 2020).

13. The other supply-side factors are innate optimism and information avoidance. On the demand side, Bénabou and Tirole (2002) distinguish between the consumption, signaling, and motivational value of beliefs.

On the empirical side, Li (2013) shows that negative emotions in social interactions can lower memory accuracy, with individuals strategically forgetting unpleasant experiences to maintain self-esteem. Carlson et al. (2020) and Saucet and Villeval (2019) find that people recall their actions more generously or altruistically than they actually were, particularly when their choices conflict with personal fairness standards or social expectations. Galeotti, Saucet, and Villeval (2020) demonstrate that forgetting past unethical behavior is heightened when it serves as an excuse to avoid future responsibility. In financial contexts, Gödker, Jiao, and Smeets (2024) identify a positive memory bias among investors, leading to optimistic beliefs, overconfidence, and excessive reinvestment. Chew, Huang, and Zhao (2020) highlight how false memories of positive events enhance confidence in one's future self. Zimmermann (2020) shows that positive feedback has lasting effects on beliefs, while negative feedback fades due to recall asymmetries. Huffman, Raymond, and Shvets (2022) extend this to managerial performance, linking overly positive memories of feedback to overconfident future predictions. In life outcomes, Müller (2022) reveals that individuals misremember past fertility desires to align with current realities, influencing intergenerational preferences. Roy-Chowdhury (2024) finds students updating their self-confidence based on flattering yet inaccurate grade recollections. Finally, Sial, Sydnor, and Taubinsky (2025) demonstrate that overestimating past gym attendance reflects naivete about time inconsistency and reduces willingness to invest in future motivation.

One important difference between motivated and non-motivated conceptions of memory is the role of incentives for accuracy: none of the non-motivated-memory theories discussed mention accuracy incentives in their models, reflecting that effects are rather thought of as 'mechanical' biases that are driven by memory limitations independent of accuracy incentives.<sup>14</sup>

In the motivated-memory literature, on the contrary, accuracy incentives feature in the trade-off between the advantages and drawbacks of inaccurate beliefs (see, e.g., Zimmermann, 2020).

### 1.2.1.2 Speech Data Literature

Independent from the memory literature, an emerging body of work is beginning to utilize speech data, in contrast to the traditionally used text responses.

14. This is not to say that incentives cannot matter in theory. One might, for example, hypothesize that even in purely non-motivated settings, higher incentives can reduce memory effects by making people 'think harder.' However, this idea has not attracted much theoretical attention, and there is actually empirical evidence that very high incentives reduce other (non-memory) biases only very mildly or not at all (Enke et al., 2023). Among other things, the authors show that very high incentives (more than the monthly median income of the sample) are far from debiasing people. In the psychology literature, there is also surprisingly limited evidence of such an effect; see Murayama and Kuhbandner (2011) for evidence on memory performance specifically.

Graeber, Noy, and Roth (2024) show that word-of-mouth transmission distorts economic forecasts, with reliability information being more likely to be lost during transmission, ultimately leading to listener beliefs being influenced regardless of forecast reliability. Graeber, Roth, and Schesch (2024) find that explanations enhance the spread of truths in financial decision-making but fail to prevent the spread of falsehoods, with content quality accounting for much of this asymmetry. Galasso, Nannicini, and Nozza (2024) discuss speech data in comparison to other data types and find that oral responses to open-ended survey questions are longer, lexically simpler, and more personal than written responses, thereby capturing richer insights into individual reasoning.

Haaland et al. (2024), in a general overview of open-ended survey data, note general methodological advantages of speech data: not only is speech data able to capture spontaneity, emotional tone, and richer narratives compared to written responses, but it is also harder to fake speech responses compared to text responses (e.g., by using large language models). Furthermore, Haaland et al. (2024) write that potential drawbacks of speech data (participant self-consciousness, the complexity of analyzing non-verbal cues, and technical challenges), are often minor in practice as exemplified by Graeber, Roth, and Schesch (2024) and Graeber, Noy, and Roth (2024).

### 1.2.2 Non-Economic Literature

While memory and retelling have only recently gained sustained attention in economics, they have long been central topics in psychology and beyond. For a general overview of memory research in psychology, see Kahana (2012).

A particularly relevant insight from these non-economic literatures is that retelling is itself a form of rehearsal, but one in which communicative goals shape what is remembered. Unlike traditional views of rehearsal as a neutral mechanism for strengthening memory traces, research shows that the act of retelling is often guided by social or emotional objectives that systematically bias both what is said and what is ultimately remembered.

This idea has deep roots. Bartlett (1932)'s classic study showed that when participants repeatedly recalled a Native American folktale, unfamiliar elements were gradually transformed into culturally familiar ones. These changes were not random distortions, but structured reinterpretations based on the teller's own schemas. Later replications (e.g., Bergman and Roediger, 1999) and reviews (Gauld and Stephenson, 1967) confirmed the robustness of this effect.

More recent research has systematically shown that retellings are shaped by the speaker's goals. For instance, Dudukovic, Marsh, and Tversky (2004) instructed participants to retell a story either accurately or entertainingly. Entertaining retellings included more emotional language and distortions, and led to less accurate memory later on. Marsh (2007) similarly finds that conversational retellings often prioritize

social or emotional impact over factual precision. Dutemple and Sheldon (2022) show that socially motivated retrieval (compared to accuracy-driven recall) reduces the number of specific episodic details recalled and alters the temporal sequence of events.

In addition, retellings are shaped by the perceived audience – a phenomenon known as *audience tuning* (Higgins, 1992). Speakers adapt to what they believe the listener knows, expects, or wants to hear. For example, speakers recall more elaborate information when addressing an attentive listener (Pasupathi, Stallworth, and Murdoch, 1998), a peer rather than an experimenter (Hyman, 1994), or someone presumed to have little prior knowledge (Vandierendonck and Van Damme, 1988).

These findings align with recent theoretical perspectives, such as the Conversational Circumplex framework proposed by Yeomans, Schweitzer, and Brooks (2022), which emphasizes that speakers pursue several different goals when conversing.

Beyond psychology, sociological work has also highlighted the strategic nature of storytelling. Goffman (1959, 1981) study how social roles, expectations, and interactional contexts shape how events are narrated. In digital spaces, these pressures are amplified by what Marwick and Boyd (2011) call *context collapse*, i.e., the challenge of addressing mixed audiences with diverging expectations. See also Polletta et al. (2011) for a broader review of storytelling in sociological research.

Importantly, such socially shaped rehearsal has consequences for memory itself. Repeated retrieval tends to strengthen memory for rehearsed content (Weldon and Bellinger, 1997; Blumen and Rajaram, 2008), but selective retrieval can also induce forgetting of related but unmentioned information – a phenomenon known as *retrieval-induced forgetting* (Anderson, Bjork, and Bjork, 1994). As Marsh (2007) notes, “the perspective taken during retelling affects later ability to remember the original event in its entirety” (p. 17). This dynamic is well documented in studies of conversational remembering, where shared distortions can propagate across speakers and listeners (Cuc, Koppel, and Hirst, 2007; Coman, Manier, and Hirst, 2009). For a comprehensive review, see Hirst and Echterhoff (2012), who synthesize findings on collaborative memory, audience tuning, social contagion, and the formation of collective memory.

### 1.2.3 This Study Within These Literatures and the Added Value of This Study

Within economics, the connection between biased retelling and biased memory has not yet been systematically studied. The closest existing contribution is the working paper Conlon (2025), which examines how revisiting past experiences shapes belief formation. The two studies are now compared in more detail.

Although both studies examine how rehearsal influences subsequent beliefs, they differ in several important respects. First, Conlon (2025) does not elicit beliefs again at the end of the first day, making it impossible to disentangle immediate

effects from delayed (i.e., memory-based) effects. In contrast, this study includes a pre-registered robustness experiment that explicitly addresses this issue (see Section 1.3). Second, Conlon (2025) employs written rehearsal (via a chat interaction with a large language model), whereas the present study uses spoken retellings. Third, Conlon (2025) investigates autobiographical memory (school grades), while the present study experimentally induces memory of controlled information (hypothetical products).

While eliciting beliefs immediately after the treatment adds clear value, the second and third differences reflect complementary design choices rather than objective superiority. In this sense, beyond Conlon (2025) not eliciting immediate beliefs, the two studies are best viewed as complementary evidence on how biased rehearsal affects memory.

The use of speech data is a key feature of this study that distinguishes it not only from Conlon (2025) but from the broader economics literature on memory. As previously elaborated on, speech data is a novel data type in economics that has recently gained attention for its methodological advantages (Haaland et al., 2024), and has generated new insights in several domains (Graeber, Noy, and Roth, 2024; Graeber, Roth, and Schesch, 2024).

Methodologically, this study follows the design principles of state-of-the-art laboratory research in economics. It employs a large sample, incentivized belief elicitation at multiple points in time, randomized treatment assignment, and rigorous control conditions to ensure internal validity. This level of identification and concern for causal inference distinguishes the present study from much of the psychological literature, where sample sizes are often smaller and causal identification less emphasized. Relative to the psychological literature, the primary novelty of this study lies therefore not in the thematic focus, but in the methodological rigor with which the question is addressed.

In sum, this study applies an established experimental-economics paradigm to a research question that has not previously been studied in economics. In doing so, it utilizes a novel data type and leverages pre-registered variation and incentivized belief elicitation to causally identify the effect of biased retelling on memory.

## 1.3 Research Design and Hypotheses

### 1.3.1 Research Design

Before discussing and justifying the research design in detail, a brief overview of the experimental timeline is provided. Two experiments were conducted: a main experiment establishing the core results and testing the main hypotheses, and a robustness experiment to address an important alternative explanation concerned with *immediate* effects (as opposed to *memory* effects).

The main experiment was spread out over two consecutive days. On day 1, 550 subjects read short descriptions of three hypothetical products. After each product description, subjects were asked how they would personally rate this product on a scale from 0 to 100, thinking about the product's overall appeal and desirability. After that, but still on day 1, subjects were asked to record three audio messages – one for each product – of themselves retelling the description to another participant (each audio recording to be sent to a different participant to avoid ‘informational spill-overs’). Henceforth, these audio messages will be called ‘retellings.’ This is where treatment variation was introduced: between-subjects, subjects were either told to retell *accurately* (*Control*) or *overly optimistically* (*Treated*). Subjects were told that these retellings would be sent to other participants who did not see the product descriptions, and that it would be optimal for them if the receiver forms an accurate or overly optimistic impression (depending on the treatment condition). (For more details on the incentivization, see below.) On day 2, the same subjects were invited again. After prompting them to briefly recall their audio recordings, subjects were asked to once more record an audio message of themselves retelling the product description (without seeing the description again). Crucially, this time subjects were asked to retell accurately no matter the day-1 treatment condition. After that, subjects were asked to recall their day-1 ranking. The incentives of this rating on day 2 are exactly identical to those of day 1 (which was, just to be clear, elicited before treatment variation was introduced). The main hypotheses are that subjects who retold overly optimistically have upward biased day-2 ratings and day-2 retellings relative to the control group who retold accurately on day 1.

In the robustness experiment, 440 subjects were invited to a one-day experiment. Each subject was randomized to see (only) one of the same set of product descriptions featured in the main experiment. Besides, the experiment closely followed the main experiment: subjects provided a rating and a retelling with the same treatment variation of the main experiment (retellings should be either *accurate* or *overly optimistic*). The core difference of the main experiment was that right after the retelling subjects were asked to recall their previously given rating. That is, compared to the main experiment time delay was minimal.

Before discussing the designs in more detail, let me justify why a second robustness experiment was employed instead of incorporating it in the main experiment. The key concern speaking against simply eliciting a recall of the initial rating again at the end of the first day was that because subjects encountered *three* products in the main experiment, there might actually be an important degree of time delay already within that day-1 session between the first rating and the ‘immediate’ recall of it. Therefore, since the main goal of the robustness exercise was to identify *immediate* effects, it was essential to minimize the time delay between the first and second rating. Hence, subjects in the robustness experiment only saw one instead of three products. Otherwise, the design is virtually identical to the first date of the main experiment but including a recall elicitation of the rating at the end.

Both experiments were programmed in Qualtrics and distributed on Prolific. Subjects were pre-screened to be 50% male and female each. See Section 1.B.2 for the exact instructions.

The following section discusses the research design and provides a more detailed justification of the design choices.

### 1.3.1.1 Main Experiment

When moving from the motivation to the implementation, the research design was developed to meet several key requirements. First, to identify causal effects, it was necessary to introduce exogenous variation in retelling motives. Second, a multi-day structure was required to identify memory effects. Third, the design aimed to ensure a natural and realistic environment, particularly with respect to the authenticity of retelling behavior.

Taken together, these requirements suggested an experimental research design as it yields an unparalleled degree of control for cleanly identifying effects through the introduction of exogenous variation. Within the domain of experimental research designs, an online survey, rather than a laboratory study, was chosen to facilitate retelling (by preventing participants from overhearing one another) and for practical reasons, such as enabling larger sample sizes for a given monetary and organizational budget. In particular, the survey provider Prolific was selected not only because it is widely accepted in the experimental economics literature as a source of high-quality data, but also because it allowed for multi-day data collection and enables researchers to inform participants in advance about the need to use a microphone and speakers.

More specifically, with regard to the research design, it is common in the memory literature to conduct experiments across two sessions in order to identify memory effects. While Amelio and Zimmermann (2023) note that no consensus has emerged on the optimal delay between sessions, a one-day delay was chosen for two main reasons. First, a shorter delay helps to reduce attrition. Second, unlike laboratory studies, online surveys are likely to leave less vivid impressions – partly due to interference from other surveys participants may complete in the meantime. As a result, holding all else constant, an online subject pool suggests a shorter interval between sessions compared to a laboratory setting.

The next key design choice concerned the question: memory of what? The psychology literature has traditionally relied either on real, often autobiographical memories elicited prior to the experiment, or on highly abstract, experimentally induced memories such as word lists.

This study, however, took a different approach by presenting short descriptions of hypothetical products for two main reasons: (i) to induce the same content within the experiment for all subjects, and (ii) to provide a natural and realistic setting. Reason (i) increases experimental control and reduces noise, thereby enhancing in-

**Dream Glow**

A smart ceiling projector that can also simulate sunrise.

Perfect for **relaxation**, a **romantic night** or even to **educate about astronomy**.

Additionally, it has a sunrise feature and can **gently wake you up with a simulated sunrise, improving wake-up routine**.

However, it **needs a constant power source** for placement.

The **ceiling projector has limited accuracy in depicting real constellations**.

It also **requires a darkened room, limiting daytime use**.



**Figure 1.1.** Product Description *Dream Glow*

**Echo Scent**

A personal fragrance device that can capture and replay scents from your favorite memories.

The Echo Scent device brings back memories through **personalized scents**.

It **reduces stress** and **enhances emotional connections**.

It's **portable** and **eco-friendly**.

But it has **limited scent storage** and **can struggle with complex scents**.

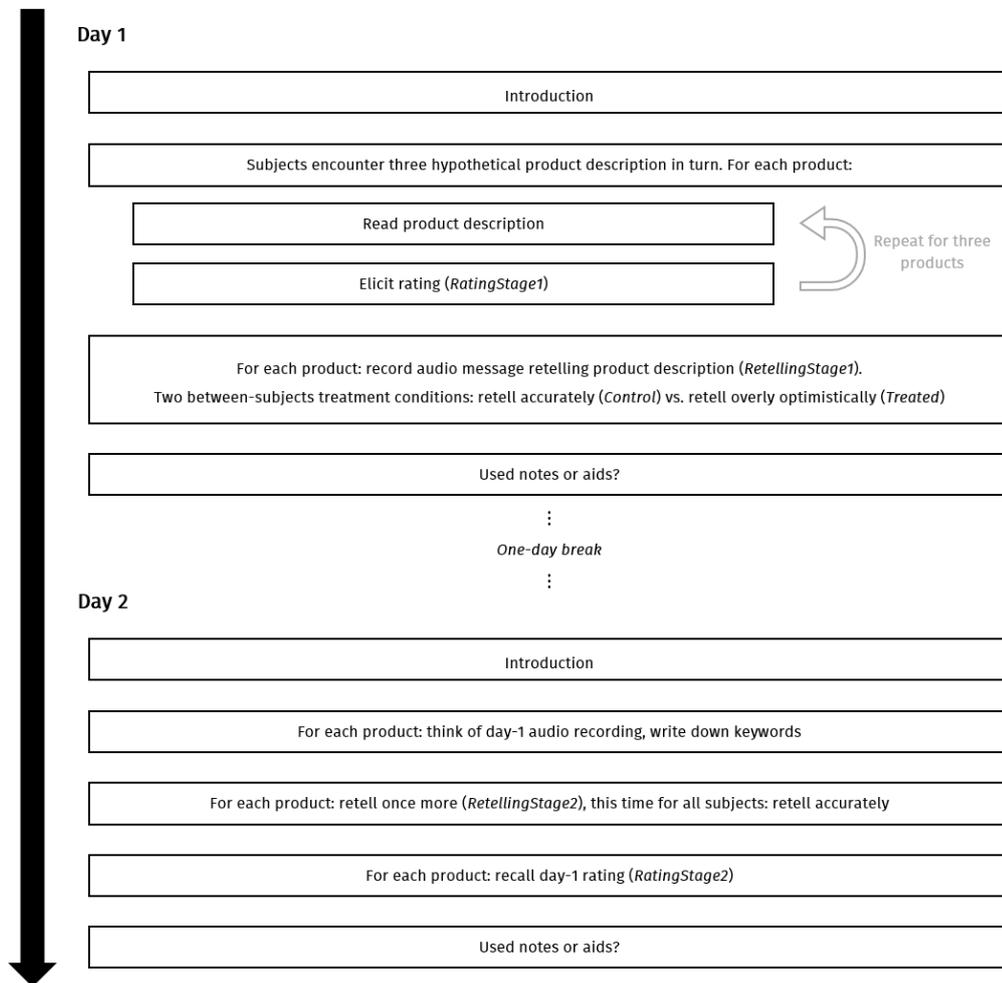
**Technical issues could lead to scent loss** and **overuse may reduce effectiveness**.



**Figure 1.2.** Product Description *Echo Scent*

ternal validity and statistical power. Reason (ii), by contrast, addresses a common and often valid criticism of experimental studies – namely, their lack of external validity. Having settled on the use of hypothetical products, these were intentionally chosen to be somewhat exotic, for two further reasons. First, exotic products are more likely to be memorable due to reduced interference with existing memories. Second, they help minimize cross-subject noise by avoiding varied personal associations with more familiar products (see the discussion of interference and association effects in Section 1.2). Each product description included both positive and negative attributes, along with a representative photo (AI-generated). To increase statistical power while holding the budget constant, each subject was shown three products. For robustness, there were two distinct sets of three products; participants were randomly assigned to either set 1 or set 2. This randomization was orthogonal to all other aspects of the design, in particular to treatment assignment.

For two of the products featured in the study, see Figure 1.1 and Figure 1.2. For the full list of all products, see Section 1.B.1.



**Figure 1.3.** Retelling and Memory: Timeline

*Note:* Product ratings were incentivized using a binarized scoring rule based on proximity to the average rating. Retellings were limited to 45 seconds and incentivized differently across conditions on day 1 (accurate vs. overly positive), while on day 2 all participants retold accurately.

For a timeline of an experimental session, see Figure 1.3.

The research design is now described in more detail. On the first day, as part of the administrative, logistic and legal introduction (informed consent, Prolific ID elicitation and microphone check), subjects also completed attention checks. In line with the policy of the survey provider, subjects were directly screened out, and immediately replaced, if (and only if) they failed both attention checks.<sup>15</sup>

Afterward, participants encountered the first product description and were required to stay on the page for at least 30 seconds. On the next page, they were asked:

15. Participants were also replaced if they chose to return the survey for any reason, such as technical issues with their microphone.

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

**Dream Glow**  
A smart ceiling projector that can also simulate sunrise.



Worst rating  Best rating

Please click on the slider.

This answer can be relevant to determine your bonus payment.

Details:  
If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

**Figure 1.4.** Day-1 Rating Elicitation (*Dream Glow*)

“On a scale from 0 to 100, how would you personally rate this product? Think about the product’s appeal and desirability in terms of its features, design, and anything else that might be relevant to you.” This rating was elicited using a slider.

On this page, participants were reminded of the product’s name, shown the photo, and given a short one-sentence description of the product category. However, the full original description – that is, the longer pro and con statements – was not shown again. See Figure 1.4 for a screenshot how this elicitation looked like.

By providing the original product information before introducing treatment variation, it was ensured that the initial encoding of information was identical across treatment groups.

For future reference, refer to this condensed version as the *product prompt* (consisting of name, photo, and summary sentence), in contrast to the initial *product description* (which included the full description with pros and cons). The *product prompt* was used consistently throughout the remainder of the survey whenever participants were asked questions about a product. The full product description was only presented once, at the beginning.

This combination of information provision and rating elicitation was then repeated for the remaining two products. All three rating questions were incentivized: participants maximized their chances of earning a bonus by providing a rating close to the average rating for that specific product among all day-1 participants (used as

a proxy for an ‘accuracy benchmark’ in the absence of a better alternative). Subjects knew this, but were not given the exact scoring formula.<sup>16</sup>

Behind the scenes, a binarized scoring rule was determining the probability of winning a bonus (Hossain and Okui, 2013). For financial and organizational reasons, only one bonus-eligible question from a randomly selected participant out of every 100 was chosen for payment. Participants were informed of this rule during the initial instructions at the start of the experiment.

Afterward, participants were told that they would now be recording audio messages for each product, which would be shown to another participant who had not seen the original product description. This was implemented, but only for the one-in-a-hundred randomly selected participants who were eligible for the bonus. That is, for these participants the retellings were actually transmitted to a recipient in a separate Prolific survey, whose participants were screened to ensure they had not taken part in either the main or robustness experiments. To avoid ‘spill-over contamination’ across retellings, senders were (truthfully) informed that no receiver was assigned more than one retelling of the same product or from the same sender.

In these retellings, between-subjects treatment variation was introduced: participants were instructed to provide either an *accurate (Control)* or an *overly positive (Treated)* retelling of the product. Beyond a plain-language explanation, this treatment variation was also incentivized.<sup>17</sup>

One concern was that participants might not produce a natural retelling of their impressions, but instead simply attempt to signal the rating they wanted the receiver to report. To prevent this, participants were explicitly warned: “Important: You must not communicate a numerical rating. Instead, you should retell the product description. If you communicate a specific rating number, you may be excluded from the bonus payment.”

The treatment variation was implemented between subjects (rather than within-subjects across products) to avoid confusion and to maximize internal validity.

After the page explaining the treatment variation in detail, participants proceeded to the audio-recording interface. They saw the same three products again in the same order as before and were shown the product prompt accompanied by a reminder of their retelling incentives as well as an instruction how to submit an audio recording. Participants were informed that their recording could not exceed 45 seconds and were reminded that each of their recordings will be sent to different

16. This design choice reflects recent insights by Danz, Vesterlund, and Wilson (2022) suggesting that – somewhat ironically – explaining incentives too precisely can distort participants’ beliefs.

17. Again using a binarized scoring rule, a reteller’s probability of receiving the bonus decreased quadratically with the deviation between the receiver’s inferred rating and a treatment-specific benchmark. In the *Treated* condition, the benchmark was 100; that is, retellers had the highest chance of receiving a bonus if the receiver inferred a rating of 100. In the *Control* condition, the benchmark matched the sender’s accuracy benchmark – i.e., the average day-1 rating across all participants.

receivers each of whom had neither seen the original product description nor received any other recording for that product. (As before, this was truthful under the condition that the sender was selected as the one-in-a-hundred participant whose recordings were actually transmitted.)

The first day of the experiment concluded with a question about whether participants had used any notes or other aids during the study. To reassure them, it was emphasized that answering “Yes” would have no negative consequences whatsoever.<sup>18</sup> While note-taking would likely bias treatment effect estimates toward zero (by improving memory and thus reducing the observable impact of retelling motives), it remains desirable that only a negligible number of participants used such aids. As pre-registered, subjects who answered “Yes, I used aids.” were excluded from data analysis. However, to uphold the integrity of the stated instructions, they were still invited to participate in the second part of the study and remained eligible for bonus payments.

The day-1 survey ended with a reminder to return for the day-2 session.

For the full verbatim instructions of the day-1 session, see Section 1.B.2.

On the following day, all participants who had successfully completed the first part were invited to take part in the second part of the study. In practice, a survey titled “SECOND PART survey (first part yesterday)” appeared on their Prolific dashboard alongside other available studies. To maximize participation, three reminder messages were sent throughout the day, encouraging all day-1 participants to complete the second session.

After the usual administrative logistics – Prolific ID elicitation and an attention check (which was not enforced in the design but could be used in the data analysis) –, subjects were asked to think about their previous audio recordings. For each product, they were shown the product prompt and asked to write down a few keywords that come to mind about their respective retelling. This task was repeated for all three products, with participants required to remain on each page for at least ten seconds. The purpose was to bring their retellings back to top of mind.

Thereafter, subjects were asked to retell the product descriptions once again, this time to a new set of recipients. It was clearly stated that these recipients were entirely different from those who had received their earlier retellings, and that – just as before – no recipient would ever receive more than one retelling from the same sender or more than one retelling of the same product.

Importantly, regardless of the treatment condition on the first day, all participants were now instructed to convey an accurate impression of the products. To en-

18. Participants were asked verbatim: “Did you use any aids during the study? For example, did you take notes on product descriptions using your phone or a sheet of paper? Or did you take screenshots of the product descriptions? There are no negative consequences whatsoever if you now answer ‘Yes,’ but it would greatly help us in evaluating the responses. Please be honest, as it would truly help us a lot.”

sure participants attended to this instruction, the message “In your voice recording, you want to convey an accurate impression of the products to the other participant” was displayed in bold red font, accompanied by an explanation of how this would affect bonus eligibility.

Apart from this change in the instructions, all other aspects of the procedure remained identical to the first day.

After participants completed the day-2 retellings for all products, they were reminded that they had also provided a product rating during the first session and were now asked to recall that rating. For each product, the rating elicitation interface was identical to that used on day 1, with one minor variation in the wording of the incentive description. It now read: “If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants from the first session who saw the exact same product description as you did in the first session.”<sup>19</sup>

As the final question, participants were once again asked whether they had used notes or other aids – worded exactly as on day 1.

For the full verbatim instructions of the day-2 session, see Section 1.B.2.3.

### 1.3.1.2 Robustness Experiment

The robustness experiment closely mirrored the day-1 session of the main experiment, with two key differences. First, immediately after the retelling, participants were asked to recall and report their previous rating. Second, each participant was exposed to only one randomly selected product (drawn from the same pool of six products used in the main experiment), thereby minimizing the time delay between the initial and follow-up rating.

In all other respects – particularly the retelling interface and treatment variation – the procedure was identical to day 1 of the main experiment. For the full verbatim instructions of the robustness experiment, see Section 1.B.2.4.

### 1.3.2 Classification of Audio Recordings

All data necessary for the analysis was collected via the surveys, with the exception of the audio recordings, which required manual coding.<sup>20</sup>

19. On day 1, the corresponding message read: “If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.”

20. At the time of writing, AI-based solutions were not yet sufficiently advanced for this task. Implementing them would have required a two-step process: first transcribing the recordings, then analyzing the resulting text – similar to the approach taken by, e.g., Graeber, Roth, and Schesch (2024) using speech data. In the context of this study, however, such a two-step method was deemed inferior, as the impression of a product’s quality conveyed in an audio recording arguably contains richer information than can be captured by text alone (e.g., emotional undertones, pitch, et cetera).

Two human coders independently listened to all recordings of the main experiment. For each recording, the coders assessed:

**Q1:** Is the audio quality of the recording sufficiently good?

**Q2:** Is there sufficient information in the audio recording to answer Q3 and Q4?

Only if the answers to Q1 and Q2 were ‘Yes’:

**Q3:** Coders categorically assess whether the recording was overly optimistic or accurate (or whether they do not know).

**Q4:** Coders provide a (subjective) numerical rating of the product’s quality solely based on this audio recording.

Coders were provided with the product prompts but not the full product descriptions, and they were explicitly instructed to evaluate each recording in isolation. Coding was implemented via four identical Qualtrics surveys, corresponding to each day (day 1 and day 2) and each of the two product sets. This design was chosen to introduce natural breaks into the coding task, as otherwise coders would have been required to process an excessive number of recordings consecutively. During these surveys, coders were blind to subjects’ treatment assignment, and *Treated* and *Control* recordings were presented in random order. Coders could pause at any point within a survey, ensuring flexibility in completion. For the exact coding instructions, see Section 1.B.2.5.

The coders’ answers to questions Q3 and Q4 serve as the dependent variables in the analysis of the retellings. As pre-registered, all recordings that were judged to have poor audio quality (Q1) or insufficient information to support an assessment (Q2) were excluded from the retelling analysis.

### 1.3.3 Hypotheses

The following three hypotheses were pre-registered. For the main experiment, there are two hypotheses:

**Hypothesis Rating:** In the day-2 ratings, subjects in *Treated* provide a significantly higher rating than subjects in *Control*.

**Hypothesis Retelling:** In the day-2 retellings, as assessed by the human coders, (i) a higher fraction of recordings is assessed to be overly optimistic (vs. accurate) in *Treated* than in *Control*, and (ii) the ratings provided by the human coders are significantly higher in *Treated* than in *Control*.

The hypothesis of the robustness experiment is:

**Hypothesis Robustness:** In the recalled ratings, there is no significant treatment effect between *Treated* and *Control* subjects for the subjects in the robustness experiment.

Note that **Hypothesis Robustness** is stronger than necessary to support the existence of a genuine memory effect, since even a smaller positive effect in the robustness experiment would suffice to support such a conclusion.

As stated also in the pre-registration, **Hypothesis Rating** and **Hypothesis Retelling** constitute the main hypotheses, while **Hypothesis Robustness** is an auxiliary hypothesis.

### 1.3.4 Exclusion Criteria

In both the day-1 main experiment and the robustness experiment, participants were required to pass attention checks. In line with the platform's policy, participants were excluded only if they failed *both* attention checks. In that case, they were immediately screened out of the survey and replaced by new participants.

On day 2 of the main experiment, a single attention check was included, but participants were not excluded based on their response. The rationale was to preserve sample size, with the day-2 attention check used only as a potential covariate in robustness analyses.

Otherwise, as long as a subject did not drop out or time out (i.e., exceed a maximum response time set by the platform), their submission was considered a complete response.<sup>21</sup>

As noted above, at the end of each day in both experiments, participants were asked whether they had used notes or other aids. In line with the pre-registration, participants who answered 'Yes' were excluded from all analyses.

All responses that passed these criteria – that is, participants who (i) submitted a complete response on each day, (ii) passed the attention checks as described, (iii) indicated that they did not use any aids – are included in the data analysis. For the analysis of the retellings, all recordings with poor audio quality or insufficient information for assessment (as assessed by the human coders) were additionally excluded.

## 1.4 Formalization

This section presents a formal framework for conceptualizing the effect of biased retelling on memory as either non-motivated or motivated. However, it is important to explicitly emphasize that this is not meant to be a realistic model, but rather a formal organization of ideas as to how an effect of biased retelling on memory might be thought of as non-motivated or motivated.

21. The time-out threshold was set at more than four times the intended median completion time. For example, for day-1 sessions, the intended median was 12 minutes, and the time-out threshold was 49 minutes.

First, note that the environment of this study lacks the typical features used to induce *motivated* memory (e.g., ego-relevant information). However, one could still conceptualize an effect of biased retelling on memory via motives to reduce cognitive dissonance between the biased retelling and the subsequent unbiased recalling (see below for more details).

In either formalization, take the perspective of some individual agent and denote by  $x$  the original information cue (the product description).<sup>22</sup> It is natural to think of  $x$  as a vector of the product's properties,  $x = (x_1, \dots, x_k)$ . For ease of presentation, however, simplify to the one-dimensional case and denote by  $x \in \mathbb{R}$  the product's overall quality. Extensions to the multi-dimensional case are straightforward.

When formalizing retelling and how it can be biased, in the more realistic multi-dimensional setting one could distinguish between the omission or invention of properties (extensive margin), on the one hand, and distortion of existing properties (intensive margin) on the other hand. In the context of this one-dimensional formalization, assume retelling to be a function of the original information  $x$  and the retelling incentives  $\nu$ , with  $r = r(x, \nu)$  and  $r : (\mathbb{R} \times \{\text{Treated}, \text{Control}\}) \rightarrow \mathbb{R}$ .

In what follows, also assume that retelling is payoff-maximizing given incentives.

Therefore, in the context of this specific research design, retelling is given by:

$$r(x, \nu) = \begin{cases} x & \text{if } \nu = \text{Control} \\ 100 & \text{if } \nu = \text{Treated} \end{cases}$$

For the *Control* treatment condition, memory should under any formalization (motivated or non-motivated memory) simply be accurate and not exhibit any systematic bias. Therefore, in the following focus on *Treated* and in turn describe two very basic formalizations – one non-motivated and one motivated – illustrating how each of them could explain a link between biased retelling and biased memory.

#### 1.4.1 Non-Motivated Memory Formalization

In this conceptualization, the link between biased retelling and biased memory is mechanical in the sense that retrieval of the original information is affected by similarity-based interference. Biased retelling  $r$  of the original information  $x$  introduces a separate cue, and when trying to retrieve  $x$ , subjects find it hard to cleanly separate  $x$  from  $r$  as they are very similar. In other words, when trying to retrieve  $x$ , the cue introduced by  $r$  interferes. For this idea of similarity-based interference, see for example Bordalo, Gennaioli, and Shleifer (2020), Bordalo et al. (2023), and Bordalo et al. (2025).

22. To simplify notation and since there is no interaction between agents, refrain from indexing by  $i$ .

Formalize this in the following reduced-form sense: Denote by  $m$  the recalled product quality and by  $\delta$  the degree of similarity-based interference from the retelling  $r$  when trying to retrieve the original information  $x$ :

$$m = (1 - \delta)x + \delta r$$

where  $\delta \in [0, 1]$  denotes the degree of interference.  $\delta$  might contain both subjective and objective components of similarity, but in the context of this exposition simply assume it to be exogenous and fixed.

Importantly, in this description, there would *not* be a trade-off between the interference effect and accuracy incentives. As stated in Section 1.2, this is in line with the theoretical literature in non-motivated memory.

#### 1.4.2 Motivated Memory Formalization

Regarding the motivated-memory formalization, subjects are assumed to incur cognitive-dissonance costs from retelling one way (biasedly) and then recalling another way (unbiasedly) later.

That is, while a certain valence of the information itself is not more or less self-serving, subjects are assumed to be motivated to avoid internal inconsistencies between past retelling and present recall.

Denote by  $\lambda \in \mathbb{R}_+$  the cognitive-dissonance cost (in other words, the strength of the motivation to align memory with retelling), and by  $\kappa \in \mathbb{R}_+$  the incentive for memory accuracy.

Assume quadratic cost functions for both the deviation of memory from the retelling (cognitive-dissonance cost) and of memory from the original cue (incentive cost).<sup>23</sup> Memory  $m$  then optimally solves the trade-off of both forces:

$$m = \arg \max_m \{-\lambda(m - r)^2 - \kappa(m - x)^2\}$$

$\Leftrightarrow$

$$m = \frac{\lambda r + \kappa x}{\lambda + \kappa}$$

As special cases, when  $\lambda = 0$  (no cognitive-dissonance cost), memory fully aligns with the original information:  $m = x$ ; and as  $\lambda \rightarrow \infty$  (infinitely high cognitive-dissonance cost), memory converges to the retelling:  $m \rightarrow r$ .

23. Quadratic cost functions are chosen purely for tractability. Any strictly convex loss function would yield the same qualitative comparative statics; the quadratic form simply delivers closed-form solutions and transparent weights on  $x$  and  $r$ .

### 1.4.3 Discussion

This section illustrated that the effect of biased retelling on memory can be understood as stemming from both non-motivated and motivated processes. Just to reiterate, this section was not meant to present a thoroughly sophisticated model.

A crucial difference between the two formalizations concerns whether the strength of the incentives to retrieve the original information ( $\kappa$  in the motivated formalization) matters. In the non-motivated set-up, these incentives are irrelevant as retrieval is mechanically affected by the interfering retelling. In the motivated set-up, by contrast, incentives explicitly enter the trade-off: The higher the incentives for accuracy  $\kappa$  (holding the cognitive-dissonance cost  $\lambda$  constant), the smaller the deviation of  $m$  from  $x$ .

Furthermore, note that since the two formalizations are not mutually exclusive, it is possible that both dynamics operate simultaneously in a given situation. In this case, various ways to model the interaction of the two are conceivable, e.g., that they happen sequentially or nested in a specific way. As an exploratory illustration, one can model the combined effect as a weighted average of the non-motivated and motivated memory outcomes, with weights  $\alpha$  and  $1 - \alpha$ , respectively.

Memory  $m$  would then be given by:

$$m_{both} = \alpha \underbrace{((1 - \delta)x + \delta r)}_{=:m_{non-mot}} + (1 - \alpha) \underbrace{\frac{\lambda r + \kappa x}{\lambda + \kappa}}_{=:m_{mot}}$$

where  $m_{non-mot}$  and  $m_{mot}$  are the respective non-motivated and motivated memory terms from before.

Since accuracy incentives  $\kappa$  are arguably the most directly manipulable variable from the experimenter side, a promising empirical strategy to disentangle the two would be to examine how memory changes with changing incentives for accurate retelling. This idea has indeed been employed in the literature, see, e.g., Zimmermann (2020). In the context of the present discussion, Figure 1.5 plots  $m$  as a function of  $\kappa$ .

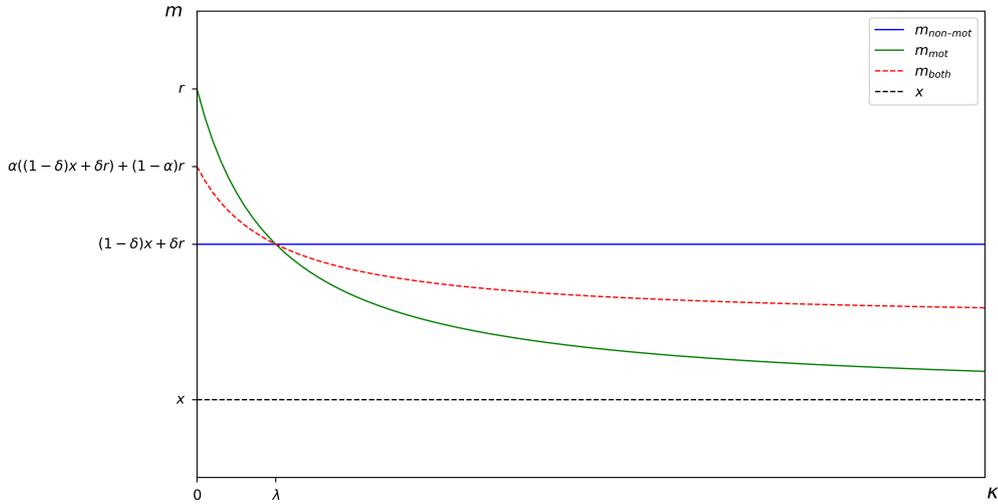
To assess which memory bias dominates for given  $\kappa$  – i.e., whether  $|m_{non-mot} - x|$  or  $|m_{mot} - x|$  is larger – it is straightforward to derive:

$$|m_{non-mot} - x| > |m_{mot} - x|$$

$$\Leftrightarrow$$

$$\kappa < \lambda$$

From an empirical perspective, the following distinction may be useful. If  $m$  (or alternative memory bias  $m - x$ ), remains unchanged when the incentive to retrieve



**Figure 1.5.** Memory as a Function of Accuracy Incentives

*Note:* This figure plots memory  $m$  as a function of accuracy incentives  $\kappa$ . The blue line depicts memory under purely non-motivated memory and is constant at  $(1 - \delta)x + \delta r$ . The green line depicts memory under purely motivated memory.  $m_{mot} = \frac{\lambda r + \kappa x}{\lambda + \kappa}$  is decreasing in  $\kappa$ , with decreasing (absolute) slope ( $|\frac{\partial m_{mot}}{\partial \kappa}| < 0$ ) and converges to  $x$  (black dotted line) for  $\kappa \rightarrow \infty$ . The memory bias  $|m - x|$  is larger for non-motivated memory vs. motivated memory if and only if  $\kappa < \lambda$ . The red dotted line depicts a weighted average of non-motivated and motivated memory for some exemplary weights ( $\alpha = 0.5$ ).

accurately is varied ceteris paribus, this suggests the effect is purely non-motivated. Conversely, if  $m$  does change with  $\kappa$  and converges to  $x$  if  $\kappa$  becomes very large, this suggests a purely motivated effect, with  $m = r$  for  $\kappa = 0$ . If both are at work (and we assume a simple weighted average),  $x - m$  converges to  $\alpha\delta(r - x)$  for  $\kappa \rightarrow \infty$ .

To conclude this discussion, it is important to reiterate that the aim of this study is not to empirically distinguish between motivated and non-motivated mechanisms. Rather, the primary objective is to cleanly identify whether biased retelling causally affects memory. The purpose of the formalization presented here is to illustrate, in simple terms, how both types of mechanisms could plausibly underlie such an effect.

## 1.5 Results

Next, the results of the main experiment are presented first, followed by those of the robustness experiment.

In what follows, *Treated* (or *Treatment = 1*) refers to subjects who were instructed to provide an overly optimistic retelling on day 1, while *Control* (or *Treatment = 0*) refers to those who were asked to retell the information accurately.

## 1.5.1 Main Experiment

### 1.5.1.1 Data

For the main experiment, 550 subjects were successfully recruited for the day-1 session on Prolific. The sample was stratified by gender, with equal numbers of male and female participants. Product sets were randomized such that half of the subjects were shown *Products1* (Dream Glow, Echo Scent, Wonder Weave), and the other half *Products2* (Vibe Vest, Zen Pod, Brain Boost).<sup>24</sup>

Subjects who failed the attention check, did not consent to data protection, dropped out, or timed out during the day-1 session were immediately replaced. As a result, the 550 participants referenced here reflect only those who passed the initial screening and submitted a valid day-1 response.

An exceptional 512 subjects (93.1%) returned for the second session and 491 (89.3%) subjects submitted a valid day-2 response.<sup>25</sup>

As pre-registered and previously mentioned, the sample for data analysis excludes from these 491 complete observations all subjects who reported having used notes or other aids. This results in a final analysis sample of 428 subjects (87% of the complete responses), all of whom indicated on both days that they did not use any such aids.

The analysis begins with the rating data (i.e., the ratings provided by the subjects themselves), followed by the retelling data (i.e., the retellings recorded by the subjects as evaluated by human coders).

### 1.5.1.2 Rating Data Results

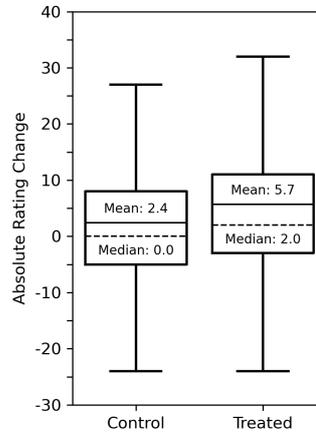
The pre-registered analyses are OLS regressions of the outcome of interest – in this case the day-2 rating – on a binary treatment variable and product fixed effects. The posited effects concern the coefficient on the binary treatment variable.

Before turning to the regression analysis, consider the following plots to provide a first impression of the data. Figure 1.6 shows a box plot of the absolute difference between a subject's day-2 rating and day-1 rating, separately for *Treated* and *Control* subjects. This difference is higher in *Treated* compared to *Control* not only for the mean and median, but also for both the 0.25 and 0.75 quartiles. Notably, the median in the control group is exactly zero, consistent with a perfect-memory, profit-maximizing benchmark.

Additional descriptive evidence is presented in Figure 1.7, which comprises three panels. The top panel displays the mean difference in ratings (Rating2 minus Rating1) for both treatments, with 95% confidence intervals. The middle panel

24. The gender-based randomization was implemented via Prolific, while the product-set randomization occurred within Qualtrics.

25. The difference between 512 and 491 is due to 15 subjects returning the day-2 survey after initially starting it, 5 time-outs, and 1 subject falsely claiming completion.



**Figure 1.6.** Main Experiment: Rating2 Minus Rating1 Box Plot

*Note:* This figure displays a box plot of absolute within-subject rating changes from day 1 to day 2, separately by treatment condition. *Treated* participants show a higher mean, median, and interquartile range in their rating adjustments compared to *Control* participants. The *Control* group median is exactly zero, consistent with a benchmark of perfect recall. In each box, the central dashed line indicates the median, while the solid line marks the mean. The top and bottom edges of the box represent the 75th and 25th percentiles (interquartile range), and the whiskers extend to 1.5 times the interquartile range above and below the box.

shows the histogram of the same rating difference at the subject-product level. The bottom panel plots the corresponding empirical cumulative distribution functions (CDFs) for both groups.

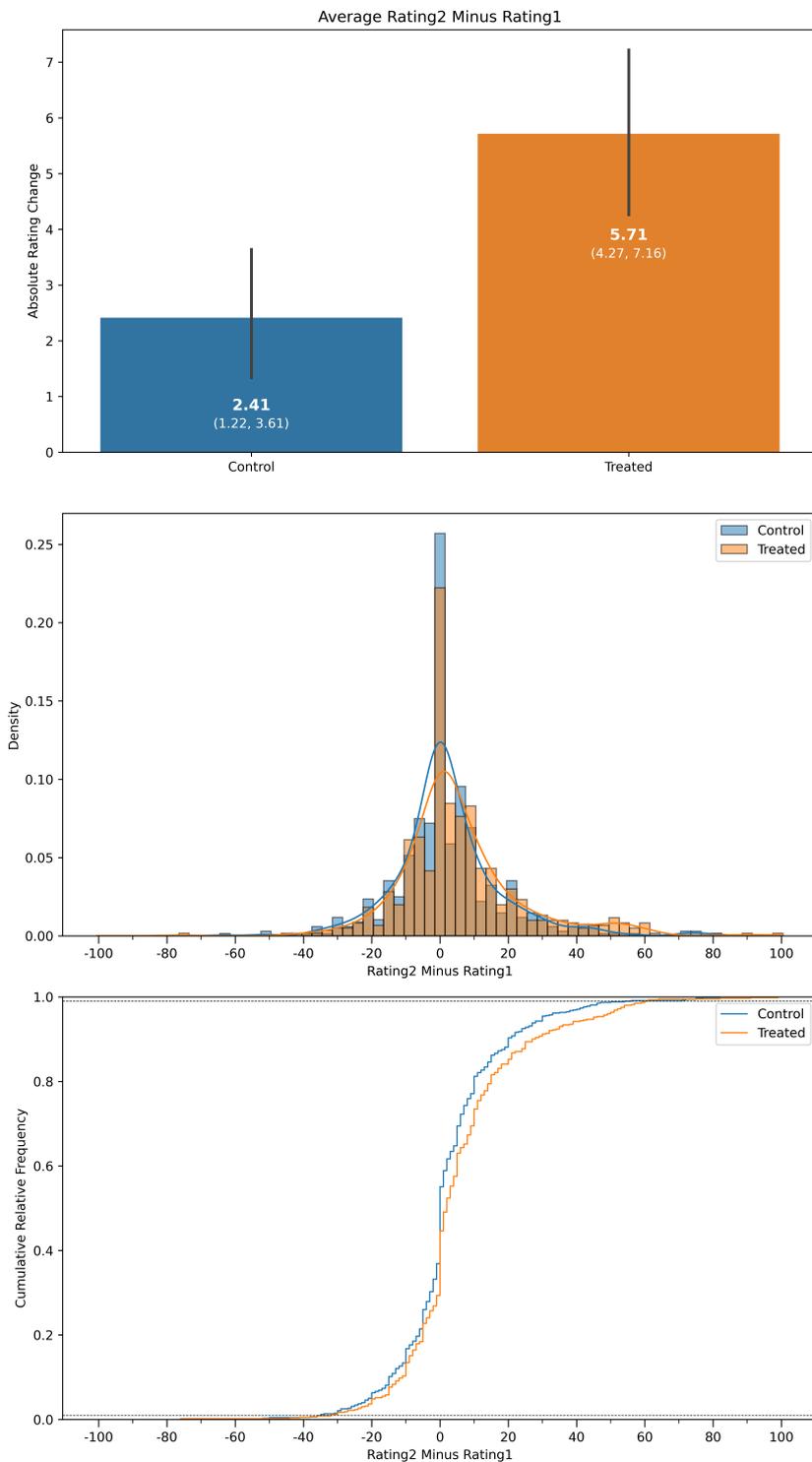
Three patterns are worth mentioning: (i) there is a significant difference between *Treated* and *Control*, (ii) in both groups, nearly one-quarter of observations reflect perfect memory (i.e., a rating difference of zero), and (iii) as seen in the CDF plot, the distribution of *Treated* almost perfectly first-order stochastically dominates the distribution of *Control*.<sup>26</sup> A Kolmogorov-Smirnov test yields borderline significant first-order stochastic dominance of the entire *Treated* CDF over the *Control* CDF ( $p = 0.058$ , KS statistic = 0.052) and highly significant dominance when excluding the bottom and top 1% quantiles ( $p < 0.01$ , KS statistic = 0.104).

This overall impression is also confirmed by regression estimates. Consider the regression:

$$RatingDay2_{i,p} = \alpha + \beta_1 \cdot Treatment_i + \beta_2 \cdot RatingDay1_{i,p} + \gamma \cdot Product_p + \varepsilon_{i,p}$$

where  $i$  denotes the subject,  $p$  the product and  $Product_p$  is a product dummy.

26. First-order stochastic dominance denotes that, in this case,  $CDF_{Treated}(x) \leq CDF_{Control}(x)$  for all  $x$ . For the given data, this condition holds within the interval defined by the 0.01 and 0.99 quantiles.



**Figure 1.7.** Main Experiment: Rating2 Minus Rating1 (Group Means, Histogram, and CDF)

*Note:* This figure shows the absolute rating changes (Rating2 minus Rating1) across treatment groups. Top panel: mean differences with 95% confidence intervals, showing a substantially higher average change in the *Treated* group. Middle panel: histogram of individual rating differences. Bottom panel: empirical CDFs, with the *Treated* distribution exhibiting near first-order stochastic dominance over the *Control* distribution. A Kolmogorov–Smirnov test confirms borderline significant dominance across the full distribution and highly significant dominance after excluding the bottom and top 1% tails (dotted horizontal lines).

See column (1) of Table 1.1 for the results with standard errors clustered at the subject level: a highly-significant 3.5 point treatment effect ( $p < 0.01$ ) that is also sizable in magnitude (13.9% of the average within-product SD in *Control*).

To give further evidence with regards to robustness, columns (2)–(4) of Table 1.1 report the following robustness specifications: (2) winsorized (5% top and bottom each), (3) only considering subjects who passed *all* attention checks (the survey provider’s policy required two checks and exclusion only if both were failed), and (4) excluding the fastest and slowest 5% (of the sum of day-1 and day-2 durations), respectively. Note that (2)–(4) were not pre-registered, but are only presented to further support the main estimate in (1). Indeed, all specifications yield a highly-significant treatment effect that is comparable in size to that of the main specification. Column (5) reports a variant of the main regression using  $RatingDay2 - RatingDay1$  as dependent variable (and dropping  $RatingDay1$  as a control – i.e., in essence imposing a coefficient of 1 on  $RatingDay1$ ), with a similar highly significant effect size.

Additionally, as a further robustness check, one could consider each product separately. Likewise, this was not pre-registered and importantly the study was not powered to detect effects on the individual-product level. Table 1.2 nevertheless reports the results: all individual-product treatment effects are positive and range from 1.81 to 6.61 points. Unsurprisingly,  $p$  values are generally larger compared to the pooled specification; still, two are relatively small (0.013 for Brain Boost and 0.063 for Dream Glow).

### 1.5.1.3 Retelling Data Results

The second main hypothesis concerned a treatment effect on subjects’ day-2 retellings. The data for this analysis was provided by two independent human coders who listened to all retellings and answered four questions for each retelling.

**Q1:** Is the audio quality of the recording sufficiently good?

**Q2:** Is there sufficient information in the audio recording to answer Q3 and Q4?

And only if the answers to Q1 and Q2 were ‘Yes’:

**Q3:** Coders categorically assess whether the recording was overly optimistic or accurate (or whether they do not know).

**Q4:** Coders provide a (subjective) numerical rating of the product’s quality solely based on this audio recording.

As pre-registered, recordings for which either Q1 or Q2 were not ‘Yes’ were dropped from the analysis of the retelling data. Pass rates were very high across groups, and differences between treatment groups were small.<sup>27</sup>

27. Overall, 93.4% of *Control* recordings and 94.8% of *Treated* recordings passed Q1 and Q2. By day, pass rates were 95.6% vs. 96.4% on day 1 and 91.2% vs. 93.2% on day 2.

**Table 1.1.** Main Experiment: Rating Regression

	(1)	(2)	(3)	(4)	(5)
Regression	Main	Winsorized	Attention	Duration	AbsRatingChange
Dependent Variable	Rating2	Rating2Wins	Rating2	Rating2	Rating2Minus1
Treatment	3.5054 (1.1454)	3.3707 (1.0970)	3.6227 (1.1748)	3.4273 (1.1835)	3.2848 (1.1813)
Rating1	0.7801 (0.0249)		0.7834 (1.1748)	0.7878 (0.0263)	
Rating1Wins		0.7896 (0.0240)			
Constant	10.6968 (1.7025)	10.5030 (1.6786)	10.4555 (1.7418)	10.1231 (1.7395)	3.2031 (1.3497)
FE(DreamGlow)	4.6348 (1.7300)	4.3397 (1.6802)	4.7560 (1.8086)	4.9472 (1.8456)	-3.2715 (1.6242)
FE(EchoScent)	4.0560 (1.7288)	3.8906 (1.7125)	3.1284 (1.7678)	3.6845 (1.8123)	-0.0421 (1.7370)
FE(VibeVest)	2.9546 (1.4042)	2.7654 (1.3893)	2.8250 (1.4347)	3.1109 (1.4918)	0.6905 (1.4660)
FE(WonderWeave)	4.2886 (1.7098)	4.3271 (1.6845)	3.5851 (1.7397)	4.8463 (1.8051)	1.9166 (1.7527)
FE(ZenPod)	0.9856 (1.4936)	0.4768 (1.4654)	0.9045 (1.5104)	0.8589 (1.6016)	-4.0190 (1.4331)
$R^2$	0.648	0.655	0.655	0.655	0.025
SD(Control)	25.17	24.32	25.06	24.80	15.83
Observations	1284	1284	1224	1161	1284
Subjects	428	428	408	387	428

Notes: This table presents OLS regression results of day-2 ratings on treatment condition, day-1 ratings and a constant, pooling the data for all six products and including product fixed effects. Standard errors are clustered at the subject level. Column (1) shows the main specification, indicating a significant treatment effect of 3.5 points, equivalent to 13.9% of the standard deviation in the control group. Columns (2)–(4) present robustness checks: winsorizing extreme values, restricting to participants who passed all attention checks, and excluding the fastest and slowest 5% by survey duration, respectively. Column (5) reports a regression using  $Rating2 - Rating1$  as the dependent variable, implicitly fixing the coefficient on day-1 rating at 1. All specifications yield highly significant and comparable treatment effects.

Regarding the categorical assessment (Q3), see Figure 1.8. The fraction of day-2 retellings in *Treated* that are classified as ‘overly optimistic’ is more than twice as large as in *Control*.<sup>28</sup> This descriptive impression is confirmed statistically: a multinomial logistic regression finds that the log-odds of a retelling being assessed as

28. It is not surprising that even some retellings in *Control* are assessed as ‘overly optimistic’ since it is highly subjective what constitutes an ‘overly optimistic’ retelling. What is striking, however, is the difference in assessment solely when varying the retelling incentives of the previous retelling one day before.

**Table 1.2.** Main Experiment: Rating Regression (Individual Products)

	(1)	(2)	(3)	(4)	(5)	(6)
Product	BrainBoost	DreamGlow	EchoScent	VibeVest	WonderWeave	ZenPod
Dependent Variable	Rating2	Rating2	Rating2	Rating2	Rating2	Rating2
Treatment	6.6100 (2.6249)	1.8050 (1.6520)	3.9563 (2.1200)	3.4193 (2.2712)	3.0479 (2.1628)	2.1912 (2.2610)
Rating1	0.8470 (0.0511)	0.6491 (0.0472)	0.8000 (0.0412)	0.7636 (0.0416)	0.7999 (0.0408)	0.7451 (0.0480)
Constant	6.9671 (2.4472)	25.3826 (3.4943)	13.4777 (2.6175)	14.4302 (2.4675)	14.3069 (2.3683)	14.2851 (3.0878)
$R^2$	0.5813	0.4714	0.6400	0.6191	0.6432	0.5420
SD(Control)	25.8267	16.7295	25.4809	27.8899	26.7810	24.4920
Observations	210	218	218	210	218	210
Subjects	210	218	218	210	218	210

Notes: This table presents OLS regression results of day-2 ratings on treatment condition, day-1 ratings and a constant for each individual product. While the study was not powered to detect product-level effects and these analyses were not pre-registered, all estimated treatment effects are positive, ranging from 1.81 to 6.61. Although standard errors are larger and most effects are not statistically significant at conventional levels, the estimates for *BrainBoost* and *DreamGlow* approach significance ( $p = 0.013$  and  $p = 0.063$ , respectively), supporting the consistency of the main treatment effect across products.

‘overly optimistic’ (relative to ‘accurate’) has a coefficient of 1.15 ( $p < 0.01$ ). This translates to an odds ratio of  $\exp(1.15) \approx 3.16$ , meaning that *Treated* subjects’ day-2 retellings are more than three times more likely to be assessed as ‘overly optimistic’ than ‘accurate,’ relative to the control group. See Table 1.A.1 for the details.

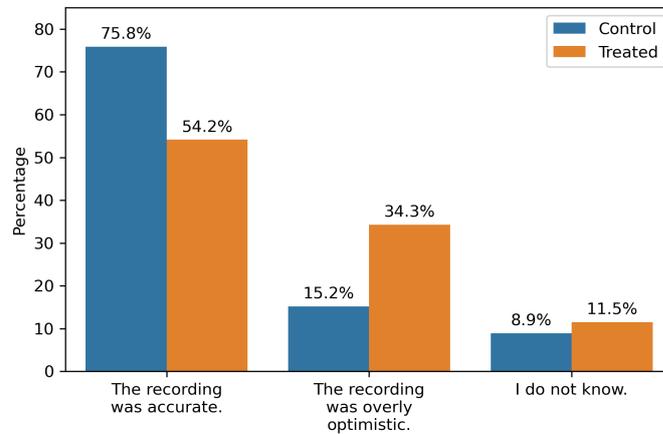
Regarding the numerical assessment (Q4), see Figure 1.9, which consists of the three panels analogous to Figure 1.7. A Kolmogorov-Smirnov test indicates strong and highly significant first-order stochastic dominance of the *Treated* CDF over the *Control* CDF ( $p < 0.01$ , KS statistic = 0.202).

As before, next consider the corresponding regression:

$$RetellingDay2_{i,p,c} = \alpha + \beta_1 \cdot Treatment_i + \gamma \cdot Product_p + \delta \cdot Coder_c + \varepsilon_{i,p,c}$$

where  $RetellingDay2_{i,p,c}$  is the numerical rating by human coder  $c \in \{RA1, RA2\}$  of subject  $i$ ’s recording of product  $p$ .

Table 1.3 reports the results, with standard errors clustered at the subject level. Column (1) presents the main specification with a highly significant treatment ef-



**Figure 1.8.** Categorical Assessment Of Day-2 Retellings

*Note:* This figure shows the categorical assessments by human coders of subjects' day-2 retellings, based on Q3 of the coding instructions. Coders classified each recording as either *accurate*, *overly optimistic*, or *do not know*, conditional on passing audio quality and content sufficiency checks. The share of recordings labeled as *overly optimistic* is more than twice as high in the *Treated* group (34.3%) compared to the *Control* group (15.2%), suggesting a strong effect of the previous day's retelling incentives on how product experiences were conveyed.

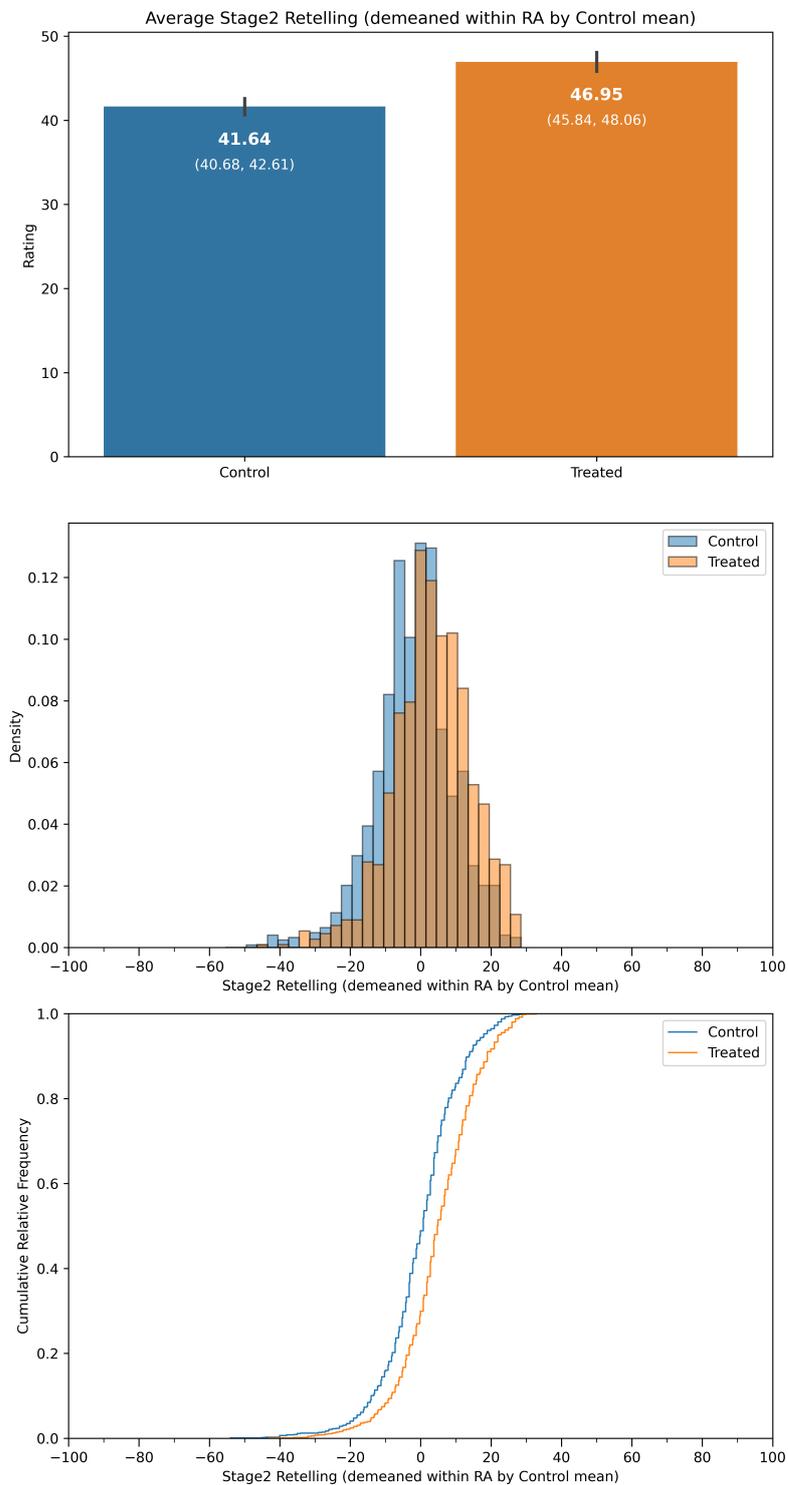
fect ( $p < 0.01$ ) of substantial magnitude (0.45 SD).<sup>29</sup> Column (2) reports a robustness specification in which ratings were standardized (mean zero, SD one) separately for each rater. Columns (3) and (4) report the main specification separately for each coder.

The findings from the rating analysis are thus overwhelmingly confirmed – and even surpassed – by the analysis of the retellings.

Note that, unlike in the analysis of subjects' self-reported ratings, it does not make sense to control for day-1 retellings when analyzing day-2 retellings, as the treatment manipulation directly targeted day-1 retellings. However, one can analyze day-1 retellings in isolation in the spirit of a manipulation check. That is, to test whether the instruction to retell overly optimistically on day 1 is reflected in the coders' evaluations. As shown in Table 1.A.2, this is indeed the case. Consistent with the idea of competing forces on day 2 – namely, incentives for accurate retelling on day 2 vs. a biasing effect from having previously retold optimistically – the treatment effect on day-1 retellings is larger than on day-2 retellings (13.5 vs. 4.9 points). For completeness, Figure 1.A.1 and Figure 1.A.2 display the corresponding plots for the categorical and numerical assessments of day-1 retellings.

Returning to the assessment of day-2 retellings, the inter-coder correlation between numerical assessments (Q4) of the same recording is 0.51. While this might

29.  $SD(\text{Control}) = 10.824$  is the average of the control group standard deviations computed separately for each rater, see columns (3) and (4).



**Figure 1.9.** Numerical Assessment Of Retellings

*Note:* This figure displays the numerical assessments of day-2 retellings by human coders, based on Q4 of the coding instructions. Top panel: average rating by treatment condition, demeaned within coder by the *Control* mean, with 95% confidence intervals. Middle panel: histogram of demeaned retelling scores. Bottom panel: empirical CDFs of the demeaned scores. Across all panels, a large treatment effect is evident.

**Table 1.3.** Main Experiment: Retelling Regression

	(1)	(2)	(3)	(4)
Regression	Main	Rating (stand.)	Only RA1	Only RA2
Dependent Variable	RetellingDay2	RetellingDay2 (stand.)	RetellingDay2	RetellingDay2
Treatment	4.8957 (0.6783)	0.4267 (0.0600)	7.3024 (1.0464)	2.7948 (0.4750)
Constant	50.7441 (0.9893)	-0.8627 (0.0779)	48.9581 (1.4087)	23.3190 (0.5462)
FE(DreamGlow)	9.6707 (0.9461)	0.9559 (0.0820)	10.1370 (1.5471)	9.2288 (0.6627)
FE(EchoScent)	8.4141 (0.9630)	0.8402 (0.0835)	8.5190 (1.5815)	8.2890 (0.6750)
FE(VibeVest)	4.8190 (0.7810)	0.4313 (0.0665)	6.6940 (1.3700)	3.1257 (0.5796)
FE(WonderWeave)	7.4893 (0.9834)	0.7783 (0.0853)	6.3384 (1.6186)	8.3690 (0.6780)
FE(ZenPod)	8.6221 (0.8147)	0.7955 (0.0722)	11.0689 (1.3138)	6.4033 (0.6655)
FE(RA2)	-28.9910 (0.4326)	-0.0014 (0.0319)		
$R^2$	0.672	0.158	0.130	0.222
SD(Control)	10.824	0.972	14.144	7.503
Observations	2363	2363	1104	1259

Notes: This table reports OLS regression results of day-2 retelling assessments by human coders (based on Q4 of the coding instructions) on treatment condition and a constant, with product and coder fixed effects and standard errors clustered at the subject (i.e., reteller) level. Column (1) presents the main specification, showing a large and highly significant treatment effect, corresponding to 0.45 SD (based on the average control-group SD across raters). Column (2) provides a robustness check using ratings standardized for each coder separately (mean zero, SD one). Columns (3) and (4) report the main specification separately for each coder.

appear modest, it is important to note that assessing a hypothetical and exotic product on an abstract 0–100 scale based solely on a brief audio recording is highly subjective – especially given that the coders themselves have individual preferences.<sup>30</sup> Importantly, as previously discussed and reported in Table 1.3 columns (3) and (4), the treatment effects remain large and significant for each coder individually.

30. To make the task manageable, coders were explicitly instructed to embrace the limited information and the subjectivity of their assessments, and to rate the product to the best of their knowledge based on the available information.

## 1.5.2 Robustness Experiment

### 1.5.2.1 Data

Next, the robustness experiment is discussed which was essentially a one-day version of the main experiment. The purpose was to empirically assess whether the main treatment effect is already present *immediately* after retelling.

Each participant was exposed to only one randomly selected product (from the same pool of six products used in the main experiment), in order to minimize the time delay between the initial rating and its immediate recall (henceforth called *Rating1.5*).

Sample restrictions closely mirrored those in the main experiment: participants were required to pass at least one of two attention checks and an automatic bot detection; subjects failing either of these were excluded and immediately replaced by a new participant. Additionally, participants who reported using notes were excluded from the final data analysis.

A sample size of 440 completed responses was pre-registered.<sup>31</sup> Of these, 387 responses indicated that no aids were used (88%) – a fraction comparable to that in the main experiment (87%) – and thus constitute the sample of the robustness experiment.

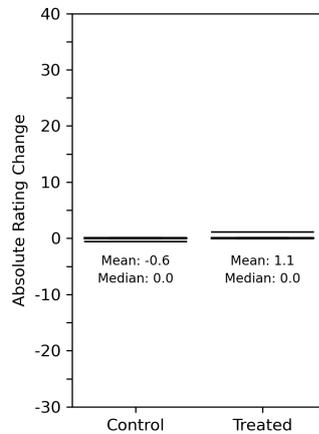
### 1.5.2.2 Results

Since participants in the robustness experiment did not retell the product twice but only provided two ratings, the analysis is limited to these ratings; retellings were neither listened to nor analyzed by human coders.

Analogously to Figure 1.6 from the main experiment, Figure 1.10 presents the absolute difference between the immediate follow-up rating (*Rating1.5*) and the initial rating (*Rating1*). This plot contrasts sharply with its main-experiment counterpart: the distribution is so tightly concentrated around zero that the box plot visually collapses into a narrow band, barely distinguishable as a box. In particular, the medians are exactly zero in both treatment groups.

As before, Figure 1.11 shows the corresponding three-panel figure. Three findings emerge. First, considering the middle panel and as already suggested by the box plots, mass is indeed much more concentrated around zero (approximately 75%, compared to slightly less than 25% in the main experiment). Second, the mean differences in the upper panel nonetheless suggest an average treatment effect. Third, the empirical CDF in the bottom panel exhibits a first-order stochastic

31. As in the main experiment, this excludes participants who were screened out during the study and immediately replaced, but it includes participants who reported using notes (even though these are later excluded from the analysis).



**Figure 1.10.** Robustness Experiment: Rating1.5 Minus Rating1 Box Plot

*Note:* This figure displays the absolute within-subject difference between *Rating1.5* and *Rating1* in the robustness experiment, separately by treatment groups. Both distributions are highly concentrated around zero, and the median is exactly zero in both groups. Unlike in the main experiment, the box plot visually collapses into a narrow band due to the lack of variation: the interquartile range is extremely small, so the box boundaries (25th and 75th percentiles) are nearly indistinguishable.

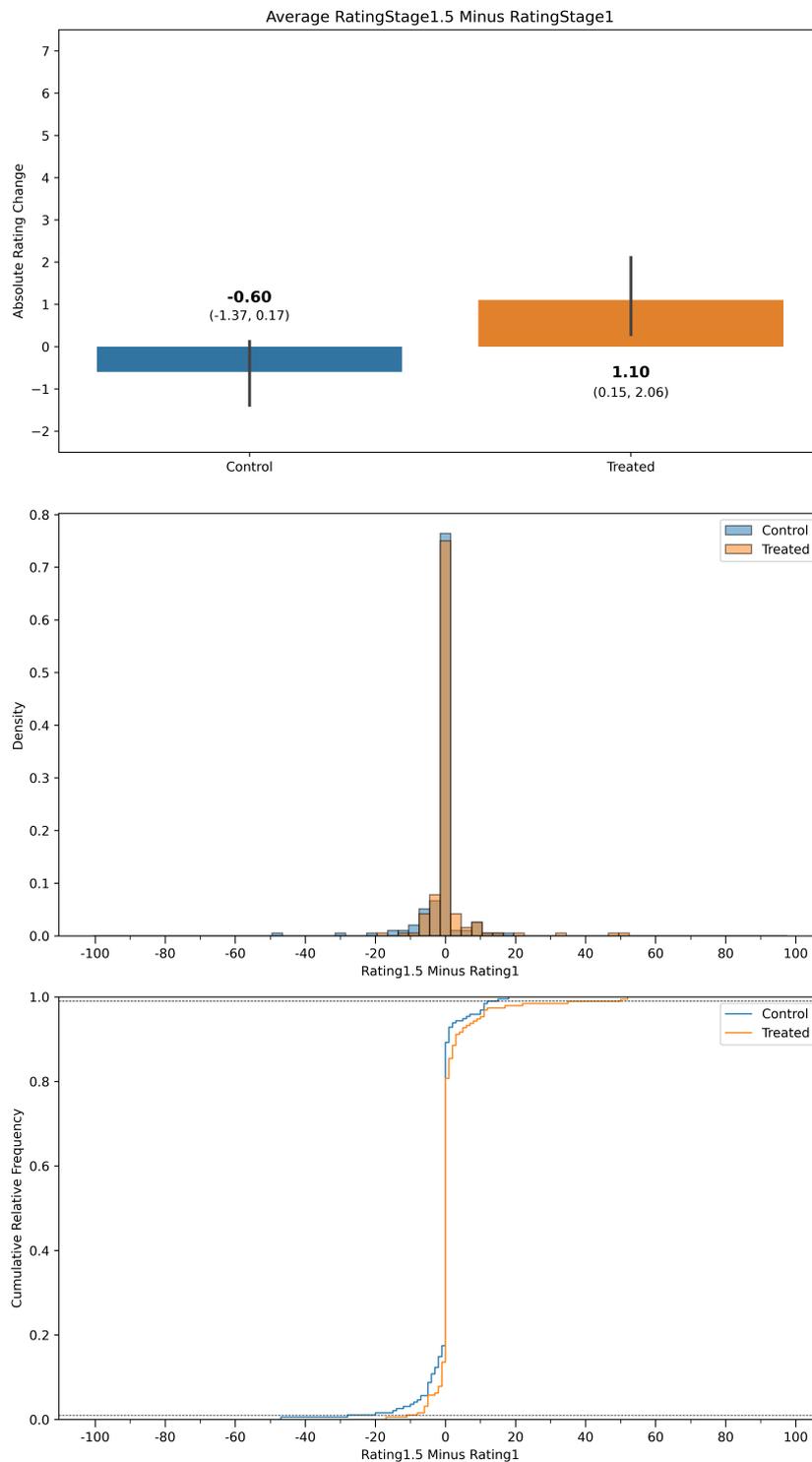
dominance pattern – even though it is mostly driven by the tails –, again hinting at a treatment effect in the immediate recall window.

Importantly, however, a Kolmogorov-Smirnov test comparing the two CDFs yields highly insignificant results ( $p > 0.99$ , KS statistic = 0.014 for the full sample;  $p = 0.75$ , KS statistic = 0.067 when excluding the bottom and top 1% quantiles). For comparison, the main experiment yielded KS test results of  $p = 0.058$ , KS statistic = 0.052 for the full sample and  $p < 0.01$ , KS statistic = 0.104 when excluding the bottom and top 1% quantiles.

Nevertheless, when estimating a regression analogous to the one in the main part, the results in Table 1.4 indicate a highly-significant treatment effect in the robustness experiment.<sup>32</sup> This finding contradicts the pre-registered auxiliary hypothesis, which posited that no such effect would be present immediately after retelling. A possible – though speculative – explanation for this immediate effect is the influence of cognitive dissonance avoidance, as discussed in Section 1.4.

However, the magnitude of the estimated treatment effect in the robustness experiment is considerably smaller than in the main experiment (52% of main-experiment point estimate). Despite the size of this difference, a two-sample  $z$ -

32. The only difference in the regression specification is that, since each subject provided only one follow-up rating, standard errors are not clustered.



**Figure 1.11.** Robustness Experiment: Rating1.5 Minus Rating1 Histogram and CDF

*Note:* This figure presents the absolute rating changes of the robustness experiment (*Rating1.5* minus *Rating1*) across treatment groups. Top panel: mean differences with 95% confidence intervals by group, showing a small but significant treatment effect. Middle panel: histogram showing the extreme concentration of values at zero. Bottom panel: empirical CDFs for both groups.

**Table 1.4.** Robustness Experiment: Rating Regression

	(1)	(2)	(3)	(4)	(5)
Regression	Main	Winsorized	Attention	Duration	AbsRatingChange
Dependent Variable	Rating1.5	Rating1.5Wins	Rating1.5	Rating1.5	Rating1.5Minus1
Treatment	1.8231 (0.6293)	1.0973 (0.4751)	1.9874 (0.6786)	1.9458 (1.1835)	1.7509 (0.6312)
Rating1	0.9721 (0.0132)		0.9709 (0.0136)	0.9755 (0.0146)	
Rating1Wins		0.9632 (0.0140)			
Constant	0.1632 (1.0075)	0.8193 (0.9099)	0.0992 (1.0406)	0.0568 (1.1091)	-1.0005 (0.8471)
FE(DreamGlow)	0.7060 (1.1820)	0.9554 (0.9540)	0.8583 (1.2308)	0.4137 (1.2926)	-0.2016 (1.1060)
FE(EchoScent)	0.3853 (1.1015)	1.1281 (0.8598)	0.2637 (1.1480)	0.2560 (1.1752)	-0.1679 (1.0747)
FE(VibeVest)	1.0246 (1.0869)	1.2669 (0.8292)	1.0907 (1.1267)	1.0146 (1.1641)	0.7030 (1.0810)
FE(WonderWeave)	1.8729 (1.0956)	3.0236 (0.8314)	1.9379 (1.1352)	2.3207 (1.1879)	1.6914 (1.0972)
FE(ZenPod)	0.6596 (1.1290)	1.4829 (0.8716)	0.7480 (1.1888)	0.4911 (1.2046)	0.2250 (1.1151)
Observations	387	387	372	347	387
Subjects	387	387	372	347	387
$R^2$	0.945	0.946	0.943	0.655	0.030
SD(Control)	23.92	15.54	23.88	22.56	5.03

Notes: This table presents OLS regression results from the robustness experiment, regressing immediate follow-up ratings (*Rating1.5*) on treatment condition, initial rating (*Rating1*) and a constant. Data for all six products is pooled and product fixed effects are included. As each subject encountered only one product, standard errors are not clustered. Column (1) reports the main specification, showing a significant but substantially smaller treatment effect compared to the main experiment (approximately 52% in magnitude). Columns (2)–(4) provide robustness checks using winsorized outcomes, restricting to participants who passed both attention checks, and excluding outliers based on response duration, respectively. Column (5) uses the within-subject difference between *Rating1.5* and *Rating1* as the dependent variable. While the treatment effect remains statistically significant across specifications, its sensitivity to outliers and reduced magnitude relative to the main experiment support the interpretation that the main treatment effect includes a genuine memory component.

test (Wald test) does not reject equality of treatment effects across experiments ( $p = 0.198$ ).<sup>33</sup>

33. That is, testing the null hypothesis of equal treatment effects across the two experiments with  $Z = \frac{\hat{\beta}_{\text{main}} - \hat{\beta}_{\text{robustness}}}{\sqrt{SE_{\text{main}}^2 + SE_{\text{robustness}}^2}}$ .

Moreover, the robustness-experiment estimate is substantially more sensitive to outliers. While winsorization in the main experiment reduces the point estimate only slightly (to 96% of the non-winsorized estimate), in the robustness experiment the winsorized estimate drops to just 60% of the non-winsorized value. As a result, the winsorized treatment effect in the robustness experiment amounts to only 33% of the corresponding winsorized estimate in the main experiment. A formal comparison of the winsorized treatment effects across the two independent experiments, analogous to before, yields a difference of 2.27, with  $Z = 1.90$  and a two-sided  $p = 0.057$ , indicating borderline significant differences.

Taken together, even though the auxiliary hypothesis was not confirmed, the treatment effect in the robustness experiment is (i) substantially smaller in magnitude, (ii) more sensitive to outliers, and (iii) not accompanied by significant distributional differences. These findings are taken to suggest that a sizable portion of the treatment effect observed in the main experiment reflects a genuine memory component, although differences in treatment effect estimates between the two experiments are only borderline significant in the winsorized specification.

The results will be summarized in the conclusion.

## 1.6 Conclusion

This study provides causal evidence on how biased retelling affects memory of originally unbiased information.

In a tightly controlled randomized experiment, subjects first assessed three hypothetical products based on short descriptions and were then randomly assigned to retell these descriptions either accurately or in an overly optimistic manner. One day later, all participants were asked to retell the original descriptions accurately and to recall their initial assessments.

Both recalled ratings and retellings – evaluated by two independent coders – showed a large and highly significant treatment effect. Subjects who had retold the descriptions in an overly optimistic manner on day 1 also gave exaggerated ratings and retellings on day 2, despite being incentivized to be accurate. These findings confirm the two main hypotheses.

A separate robustness experiment assessed whether this distortion is a genuine *memory* effect by examining whether memory was already biased *immediately* after biased retelling. A regression analogous to the main experiment still yielded a significant treatment effect. The auxiliary hypothesis that no significant treatment effect would emerge in the robustness experiment was therefore not confirmed. However, this effect was (i) smaller in magnitude, (ii) more sensitive to outliers, and (iii) not supported by significant distributional differences, pointing to a genuine memory component in the treatment effect of the main experiment, although differences in

treatment effect estimates between the two experiments are only borderline significant in the winsorized specification.

These findings make a substantial contribution to the literature. While memory has become an increasingly important topic in economics, the specific question addressed in this chapter has received little attention, with only one very recent working paper examining a related but distinct issue (see Section 1.2).

Furthermore, mechanisms like the one identified by the treatment effect could arise in many settings. The results suggest that whenever people are likely to retell past experiences or information in a biased manner – whether for strategic, motivational, or social reasons – those retellings may shape their future memories. Such dynamics are likely to be particularly relevant in financial decision-making, labor-market settings, and contexts involving discrimination.

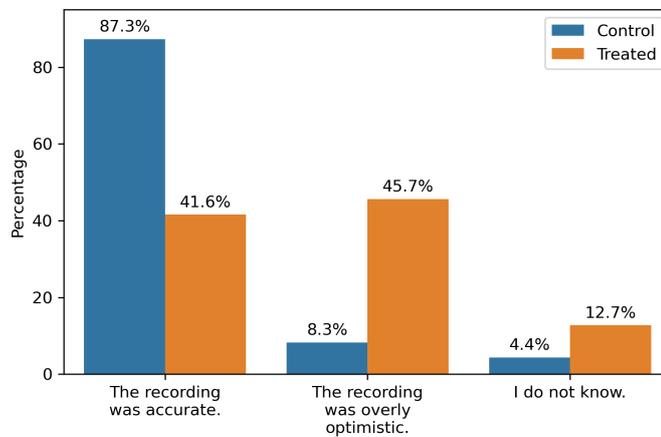
Future research could pursue several directions. First, it could explore how these effects evolve over multiple retellings or across longer time horizons. Second, it could investigate the extent to which the distortion is driven by motivated reasoning, for example by experimentally varying incentives for accuracy (see Section 1.4). Third, it could examine the structure of the retained bias, such as whether it reflects internal margin distortions (like exaggeration or diminishment) or external margin shifts (like omission or fabrication).

## Appendix 1.A Supplementary Tables and Figures

**Table 1.A.1.** Day-2 Retelling Categorical Regression (Multinomial Logit)

	Overly optimistic vs. Accurate	I don't know vs. Accurate
Treatment	1.1500 (0.1030)	0.5914 (0.1395)
Constant	-1.6063 (0.0797)	-2.1385 (0.1004)
Observations	2361	
Log-Likelihood	-1945.4	
Pseudo $R^2$	0.0338	

*Notes:* This table reports multinomial logistic regression results predicting the categorical assessment of day-2 retellings (Q3 of the coding instructions). Coders classified each recording as *accurate*, *overly optimistic*, or *do not know*. The strong treatment effect should be interpreted as a manipulation check: incentives explicitly required participants in the *Control* condition to record an accurate retelling, and in the *Treated* condition to record an overly optimistic one.



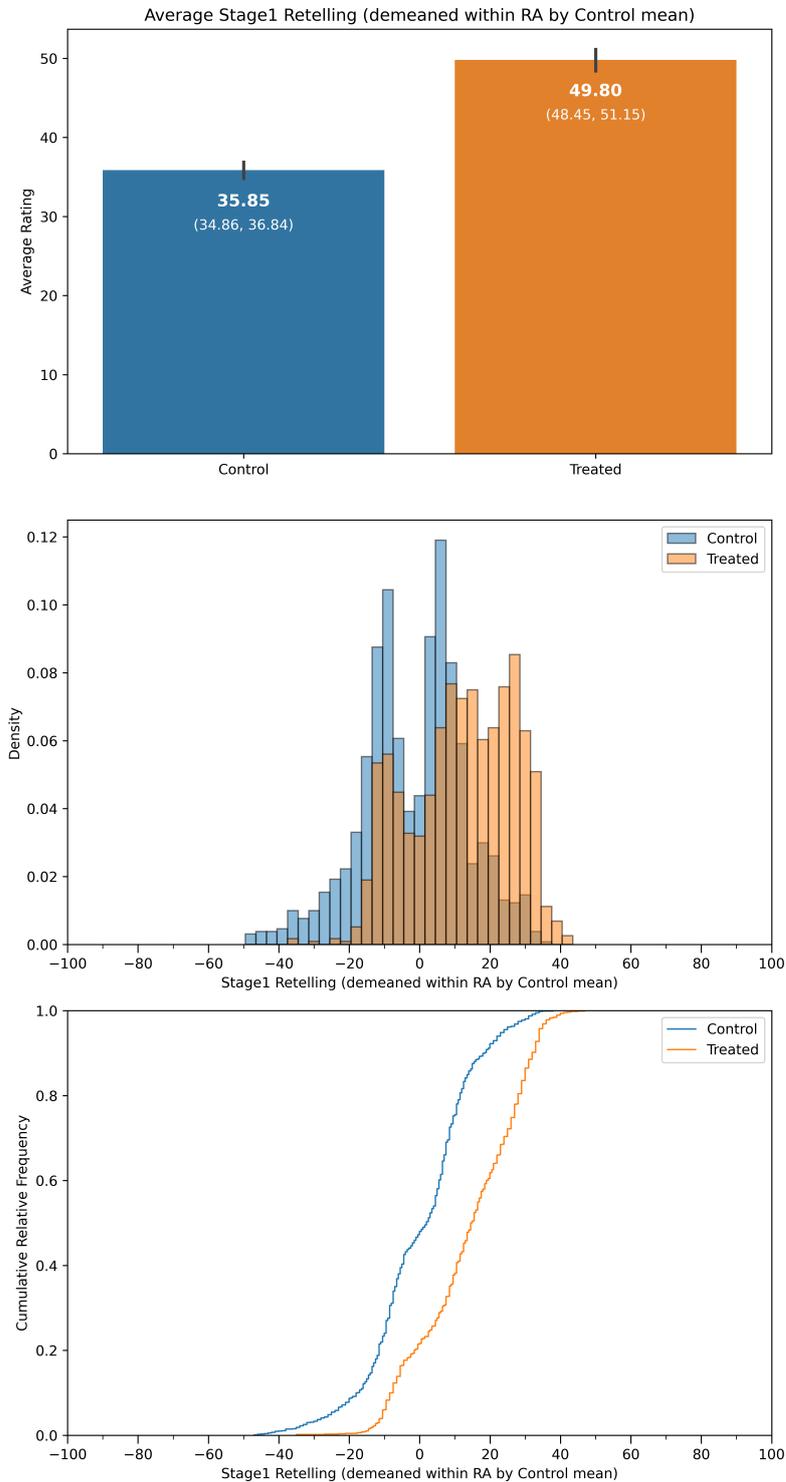
**Figure 1.A.1.** Categorical Assessment Of Day-1 Retellings

*Note:* This figure shows the distribution of categorical assessments by human coders of day-1 retellings, based on Q3 of the coding instructions. Coders classified each recording as *accurate*, *overly optimistic*, or *do not know*. The strong treatment effect should be interpreted as a manipulation check: incentives explicitly required participants in the *Control* condition to record an accurate retelling, and in the *Treated* condition to record an overly optimistic one.

**Table 1.A.2.** Retelling Regression Day 1

	(1)	(2)	(3)	(4)
Regression	Main	Rating (stand.)	Only RA1	Only RA2
Dependent Variable	RetellingDay1	RetellingDay1 (stand.)	RetellingDay1	RetellingDay1
Treatment	13.5373 (0.6475)	0.7736 (0.0382)	24.1548 (1.1439)	3.6089 (0.3035)
Constant	44.2754 (1.1526)	-1.0483 (0.0634)	40.9545 (1.6333)	16.3121 (0.3681)
FE(DreamGlow)	15.3824 (0.9200)	1.2689 (0.0540)	11.8008 (1.6734)	18.7607 (0.4775)
FE(EchoScent)	11.2799 (0.9127)	1.0237 (0.0533)	4.8997 (1.6686)	17.0965 (0.4427)
FE(VibeVest)	3.6822 (0.7805)	0.2201 (0.0468)	6.1613 (1.4229)	1.2819 (0.4663)
FE(WonderWeave)	10.6137 (0.9549)	0.9971 (0.0557)	3.2463 (1.7434)	17.2817 (0.4480)
FE(ZenPod)	5.6152 (0.7351)	0.3127 (0.0435)	10.2535 (1.3840)	1.1785 (0.4531)
FE(RA2)	-31.0511 (0.8218)	0.0334 (0.0520)		
Observations	2463	2463	1190	1273
R <sup>2</sup>	0.654	0.386	0.410	0.780
SD(Control)	14.108	0.946	18.960	9.255

Notes: This table reports OLS regression results of day-1 retelling assessments by human coders (Q4 of the coding instructions) on treatment assignment and a constant, with product and coder fixed effects and standard errors clustered at the subject level. As day-1 retellings were directly manipulated through treatment, these regressions serve as a manipulation check. Column (1) shows the main specification, revealing a large and highly significant treatment effect. Column (2) provides a standardized version of the ratings (mean zero, SD one), and columns (3) and (4) report the results separately for each coder. Across all specifications, the treatment effect on day-1 retellings is consistently larger than for day-2 (see Table 1.3).



**Figure 1A.2.** Numerical Assessment Of Day-1 Retellings

*Note:* This figure shows the numerical assessments of day-1 retellings by human coders, based on Q4 of the coding instructions. Top panel: average rating by treatment condition, demeaned within each coder by the control mean, with 95% confidence intervals. Middle panel: histogram of demeaned retelling scores. Bottom panel: empirical CDFs of the demeaned scores. As day-1 retellings were directly manipulated through treatment, these visualizations serve as a manipulation check. Throughout, a large treatment effect is visible – larger than that of day-2 retellings (Figure 1.9).

## Appendix 1.B Additional Information

### 1.B.1 Products Featured

#### 1.B.1.1 Product Set 1

##### Dream Glow

A smart ceiling projector that can also simulate sunrise.

Perfect for **relaxation**, a **romantic night** or even to **educate about astronomy**.

Additionally, it has a sunrise feature and can **gently wake you up with a simulated sunrise**, improving **wake-up routine**.

However, it **needs a constant power source** for placement.

The **ceiling projector has limited accuracy in depicting real constellations**.

It also **requires a darkened room, limiting daytime use**.



Figure 1.B.1. Product *Dream Glow*

##### Echo Scent

A personal fragrance device that can capture and replay scents from your favorite memories.

The Echo Scent device brings back memories through **personalized scents**.

It **reduces stress** and **enhances emotional connections**.

It's **portable** and **eco-friendly**.

But it has **limited scent storage** and **can struggle with complex scents**.

**Technical issues could lead to scent loss** and **overuse may reduce effectiveness**.



Figure 1.B.2. Product *Echo Scent*

##### Wonder Weave

Clothing that changes color and pattern based on temperature.

Adds a **fun** and **interactive** element to fashion.

Provides a **unique fashion statement** and is **ideal for special occasions**.

Can be considered "multiple clothes in one," making it **more sustainable** and **useful for people with little storage space**.

But it needs **special care when washing** and is **pricier than regular clothing**.

Over time, **color variability may fade**.

It **might not be suitable for all skin types**.



Figure 1.B.3. Product *Wonder Weave*

### 1.B.1.2 Product Set 2

**Vibe Vest**  
A wearable vest that translates music into vibrations.

The VibeVest allows the wearer to **feel the rhythm and beat of favorite songs**.

It **adds a new dimension to listening** and is **great for parties**.

It is even suitable for **therapeutic use**.

However, it might **not capture all the details and nuances in music**.

For some, it could be **bulky**.

The **intense vibrations might bother some**.

**Battery life is short**.



Figure 1.B.4. Product *Vibe Vest*

**Zen Pod**  
A portable meditation pod that uses guided meditation technology.

The Zen Pod meditation pod lets you **find peace and focus anywhere**.

It **provides audio assistance**. This is **especially useful for beginners**.

It also features **soundproofing to block external noise**.

It is **compact and lightweight** for **easy transportation** and can be used in **various environments**.

However, there is **limited degree of customization** and **limited selection of guided meditation sessions**.

The **soundproofing technology may not work perfectly**.

Some users may **prefer traditional meditation methods over technology-based solutions**.



Figure 1.B.5. Product *Zen Pod*

**Brain Boost**  
A wearable headband that enhances focus and cognitive abilities.

The Brain Boost headband uses electrical pulses to **stimulate brain activity**. It **improves alertness** and **concentration**.

It is **non-invasive** and **safe** to use for **users of all ages**, from students to professionals **seeking a mental edge at studying, working or problem solving**.

However, there is **limited scientific evidence about the long-term efficacy**.

It may also cause **skin irritation, discomfort or headaches with prolonged use**.

Use **should be monitored to prevent overstimulation or dependency on the device**.



Figure 1.B.6. Product *Brain Boost*

## 1.B.2 Instructions

### 1.B.2.1 Main Experiment: Stage 1, Product Set 1

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Start of Block: Intro

**Welcome!**

**Important: This study consists of two parts that are scheduled on separate dates. You have to participate in both parts to earn the bulk of your reward.**

**The first part takes place now, the second part will take place tomorrow.**

This study is designed for computer (PC or Mac) users only (desktop, laptop, etc.). If you are accessing this study on a smartphone, a tablet or any other non-PC devices, please switch to PC and enter the study again, or return the submission on Prolific.

All your answers will be anonymized. This means that we have no way of linking your responses to your identity.

Your payment will consist of a fixed participation fee as well as a potential bonus depending on your answers.

One out of one hundred participants will be randomly selected to receive their bonus based on specific criteria described later in the study.

**Note that you will need to use your microphone and speakers / headphones.**

To participate in this study, you must reside in the USA. If you do not reside in the USA, please return the study on Prolific.

Page Break

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Please confirm that you will participate in the second part of the study that will take place tomorrow:

Yes, I will participate in the second part tomorrow.

You will receive a separate invitation for the second part tomorrow.

Page Break

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**Data Protection and Informed Consent**

*Please consent to the processing of your data and our privacy policy.*

To display the full privacy policy click here

*[if clicked:]*

**Who is responsible for the study?**

Responsible for the execution of the study and therefore for the processing of your data: Alexander Laubel  
Bonn University  
Regina-Pacis-Weg 3  
53113 Bonn

Germany

**What are the purposes of the study?**

The purpose of this study is to improve our understanding of human behavior in economic contexts. We are interested in natural, unbiased behavior. Hence, no details on the background of this research project is given beforehand, which is in accordance with the standard in experimental economics. All necessary information will be provided in due time.

**What happens to my data?**

All participating employees and scientists work in accordance with the provisions of the Data Protection Regulation, the Federal Data Protection Act and the relevant State Data Protection Acts. Immediately after the collection, your data will be stored anonymized and then statistically analysed. No conclusions about your person can be drawn from these results. Your data will be stored by a technical service provider outside the EU. We have taken all necessary precautions and concluded all contracts which are necessary in order to comply with data protection. Even if the service provider is not located in the EU, your data is secure as we have concluded contracts with the service provider designated for this purpose in the EU. For this study, we collect "special categories of personal data", in particular data revealing migration background (racial or ethnic origin - term according to Art. 9 GDPR), political opinions, religious or philosophical beliefs or trade union membership, genetic data, health data or data concerning a person's sex life or sexual orientation, which we use only for research purposes and solely with your consent.

**Declaration of Consent**

I hereby consent to the processing of my personal data for this research project for the questions on human behavior. I can withdraw my consent at any time. I have taken note of all information concerning the usage of my data and on my rights in the privacy policy. Your data will be stored and analyzed in full compliance with the highest standards of the data protection laws of the European Union. In particular, no conclusions about your person will be drawn. You can withdraw your consent at any time.

I consent

I do not consent

*[If "I do not consent" was selected, survey ends.]*

End of Block: Introduction

---

Start of Block: AttentionCheck1

This question concerns the following problem: Often, participants click through surveys like this one without reading the instructions carefully. As a consequence, many of the participants can't be used in research. To show us that you are reading the questions

carefully, please give the following answers to the question below: "Very interested" and "I've never heard of it".

Based on the text you read above, what have you been asked to answer to the following question: How interested are you in American Football?

- Very interested
- A bit interested
- Not interested
- I've never heard of it

End of Block: AttentionCheck1

---

Start of Block: MicCheck

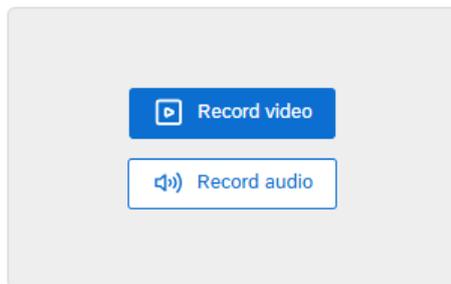
**Please talk for at least 10 seconds about your opinion on daylight savings time.**

Whether you are in favor or against daylight savings does not affect your eligibility to participate in this study.

However, we ask that you talk for at least 10 seconds about this topic.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your opinion.
- 5) After 10 seconds of recording, you can click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record your opinion again.
- 7) If you are clearly understood, proceed to the next page.



End of Block: MicCheck

---

Start of Block: ProlificID

**What is your Prolific ID?**

Note that this response should auto-fill with the correct ID.

End of Block: ProlificID

---

Start of Block: AttentionCheck2

The beverage test you are about to take is very simple, when asked for your favorite beverage you must select 'Lipton Ice Tea.' This is an attention check.

Based on the text you read above, what beverage have you been asked to answer?

- Bud Light
- Coca-Cola
- Mountain Dew
- Gatorade
- Lipton Ice Tea
- Ocean Spray Juice

*[If both attention checks failed, survey ends.]*

End of Block: AttentionCheck2

---

Start of Block: IntroProducts

The actual survey starts now and you will see **hypothetical products**.

For each product, you will see a short summary of a **product description mentioning positive and negative aspects**.

Please **treat the product description as if it were real**.

Please read each summary carefully.

Afterwards, you will need to answer questions about the products.

End of Block: IntroProducts

---

Start of Block: DreamGlowRating1

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*

Perfect for **relaxation**, a **romantic night** or even to **educate about astronomy**.

Additionally, it has a sunrise feature and can **gently wake you up with a simulated sunrise, improving wake-up routine**.

However, it **needs a constant power source** for placement.

The **ceiling projector has limited accuracy in depicting real constellations**.

It also **requires a darkened room, limiting daytime use**.



(You will be able to proceed after 30 seconds.)

Page Break

---

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

### Dream Glow

*A smart ceiling projector that can also simulate sunrise.*



Worst rating  Best rating  
Please click on the slider.

*[after clicking on the slider:]*

Worst rating  Best rating  
Your answer: 60

This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances

the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: DreamGlowRating1

---

Start of Block: EchoScentRating1

### Echo Scent

*A personal fragrance device that can capture and replay scents from your favorite memories.*

The Echo Scent device brings back memories through **personalized scents**.

It **reduces stress** and **enhances emotional connections**.

It's **portable** and **eco-friendly**.

But it has **limited scent storage** and **can struggle with complex scents**.

**Technical issues could lead to scent loss** and **overuse may reduce effectiveness**.



*(You will be able to proceed after 30 seconds.)*

Page Break

---

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

### Echo Scent

*A personal fragrance device that can capture and replay scents from your favorite memories.*



Worst rating  Best rating

Please click on the slider.

[after clicking on the slider:]

Worst rating  Best rating

Your answer: 60

This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: EchoScentRating1

---

Start of Block: WonderWeaveRating1

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*

Adds a **fun** and **interactive** element to fashion.

Provides a **unique fashion statement** and is **ideal for special occasions**.

Can be considered "multiple clothes in one," making it **more sustainable** and **useful for people with little storage space**.

But it needs **special care when washing** and is **pricier than regular clothing**.

Over time, **color variability may fade**.

It **might not be suitable for all skin types**.



*(You will be able to proceed after 30 seconds.)*

Page Break 

---

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*



Worst rating  Best rating

Please click on the slider.

*[after clicking on the slider:]*

Worst rating  Best rating

Your answer: 60

This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: WonderWeaveRating1

---

Start of Block: RetellingInfo

Now you will be informing another person about the products.

For this purpose, you will record audio messages for each product.

Each of your audio recordings will then be sent to another participant. Note: no single participant will receive more than one audio recording by you.

**This other participant has not seen the original product description that you just saw.**

[If Treatment == 0:]

**In your voice recording, you want to convey an accurate impression of the products to the other participant.**

This means:

- The other participant will be asked to give an honest assessment of the product quality **solely based on your recording**.
- This assessment by the other participant may be selected to determine your bonus.
- **It is best for you if the rating the other participant gives is as close as possible to the average rating of other participants who saw the product description like you did.**

[If Treatment == 1:]

**In your voice recording, you want to convey an overly positive impression of the products to the other participant.**

This means:

- The other participant will be asked to give an honest assessment of the product quality **solely based on your recording**.
- This assessment by the other participant may be selected to determine your bonus.
- **It is best for you if the rating the other participant gives is as close as possible to 100.**
- The other participant does not know that your recording is overly positive (but it is also not relevant for their payment).

**Important:** You must not communicate a numerical rating. Instead, you should retell the product description.

If you communicate a specific rating number, you may be excluded from the bonus payment.

End of Block: RetellingInfo

---

Start of Block: DreamGlowRetelling1

Consider this product:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



*[If Treatment == 0:]*

You want to **convey an accurate impression of the product** to the other participant.

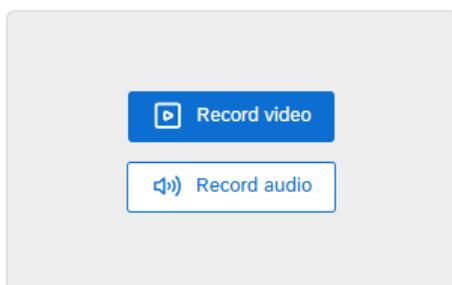
*[If Treatment == 1:]*

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: DreamGlowRetelling1

---

Start of Block: EchoScentRetelling1

Consider this product:

**Echo Scent**

*A personal fragrance device that can capture and replay scents from your favorite memories.*



*[If Treatment == 0:]*

You want to **convey an accurate impression of the product** to the other participant.

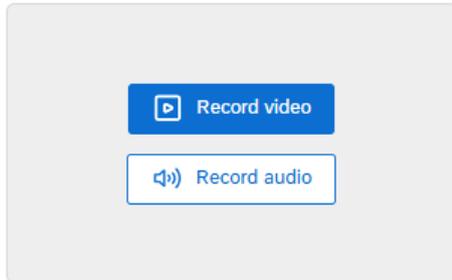
*[If Treatment == 1:]*

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: EchoScentRetelling1

---

Start of Block: WonderWeaveRetelling1

Consider this product:

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*



*[If Treatment == 0:]*

You want to **convey an accurate impression of the product** to the other participant.

*[If Treatment == 1:]*

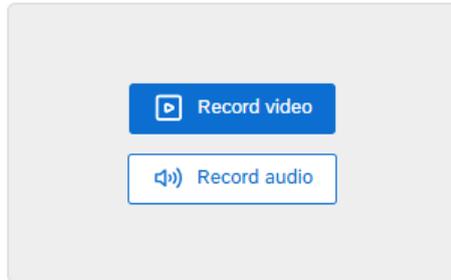
You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.

- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.  
7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: WonderWeaveRetelling1

---

Start of Block: Survey

This is the final question of the survey.

Did you use any aids during the study?

For example, did you take notes on product descriptions using your phone or a sheet of paper? Or did you take screenshots of the product descriptions?

There are no negative consequences whatsoever if you now answer "Yes," but it would greatly help us in evaluating the responses. Please be honest as it would truly help us a lot.

- Yes, I used aids.
- No, I did not use aids.

End of Block: Survey

---

Start of Block: End

Thank you for taking part in the first part of this study.

**Important: make sure to participate in the second part of this study as you will earn the bulk of your reward only if you do.**

**You will receive an invitation to participate in the second part of this study tomorrow.**

Please click the button below to be redirected to Prolific and register your submission of the first part of this study.

End of Block: End

---

**1.B.2.2 Main Experiment: Stage 1, Product Set 2**

*Note: Sections identical to Product Set 1 are omitted.*

[...]

---

Start of Block: VibeVestRating1

**Vibe Vest**

*A wearable vest that translates music into vibrations.*

The Vibe Vest allows the wearer to **feel the rhythm and beat of favorite songs.**

It **adds a new dimension to listening** and is **great for parties.**

It is even suitable for **therapeutic use.**

However, it might **not capture all the details and nuances in music.**

For some, it could be **bulky.**

The **intense vibrations might bother some.**

**Battery life is short.**



*(You will be able to proceed after 30 seconds.)*

---

Page Break

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

**Vibe Vest**

*A wearable vest that translates music into vibrations.*

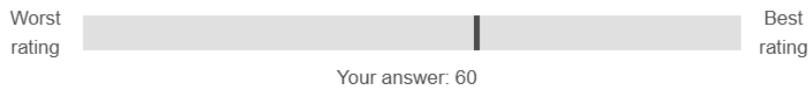


Worst rating

Best rating

Please click on the slider.

*[after clicking on the slider:]*



This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: VibeVestRating1

---

Start of Block: ZenPodRating1

### Zen Pod

*A portable meditation pod that uses guided meditation technology.*

The Zen Pod meditation pod lets you **find peace and focus anywhere**.

It **provides audio assistance**. This is **especially useful for beginners**.

It also features **soundproofing to block external noise**.

It is **compact** and **lightweight** for **easy transportation** and can be used in **various environments**.

However, there is **limited degree of customization** and **limited selection of guided meditation sessions**.

The **soundproofing technology may not work perfectly**.

Some users may **prefer traditional meditation methods over technology-based solutions**.



*(You will be able to proceed after 30 seconds.)*

Page Break 

---

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

### Zen Pod

*A portable meditation pod that uses guided meditation technology.*



Worst rating  Best rating  
Please click on the slider.

[after clicking on the slider:]

Worst rating  Best rating  
Your answer: 60

This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: ZenPodRating1

---

Start of Block: BrainBoostRating1

### Brain Boost

*A wearable headband that enhances focus and cognitive abilities.*

The Brain Boost headband uses electrical pulses to **stimulate brain activity**. It **improves alertness and concentration**.

It is **non-invasive** and **safe** to use for **users of all ages**, from students to professionals **seeking a mental edge at studying, working or problem solving**.

However, there is **limited scientific evidence about the long-term efficacy**.

It may also cause **skin irritation, discomfort or headaches with prolonged use**.

Use **should be monitored to prevent overstimulation or dependency on the device**.



(You will be able to proceed after 30 seconds.)

Page Break

---

On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

**Brain Boost**

*A wearable headband that enhances focus and cognitive abilities.*



*[after clicking on the slider:]*



This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: BrainBoostRating1

---

[...]

Start of Block: DreamGlowRetelling1

Consider this product:

**Vibe Vest**

*A wearable vest that translates music into vibrations.*



*[If Treatment == 0:]*

You want to **convey an accurate impression of the product** to the other participant.

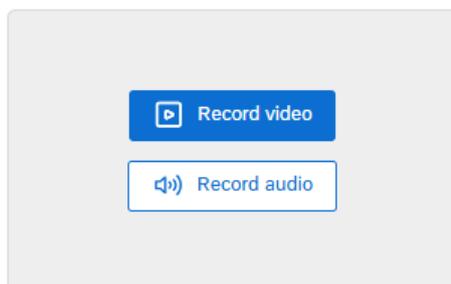
*[If Treatment == 1:]*

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: DreamGlowRetelling1

---

Start of Block: ZenPodRetelling1

Consider this product:

**Zen Pod**

*A portable meditation pod that uses guided meditation technology.*



[If Treatment == 0:]

You want to **convey an accurate impression of the product** to the other participant.

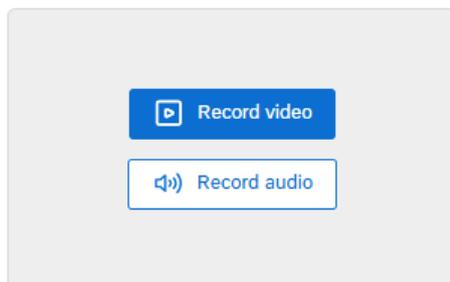
[If Treatment == 1:]

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: EchoScentRetelling1

---

Start of Block: WonderWeaveRetelling1

Consider this product:

**Brain Boost**

*A wearable headband that enhances focus and cognitive abilities.*



*[If Treatment == 0:]*

You want to **convey an accurate impression of the product** to the other participant.

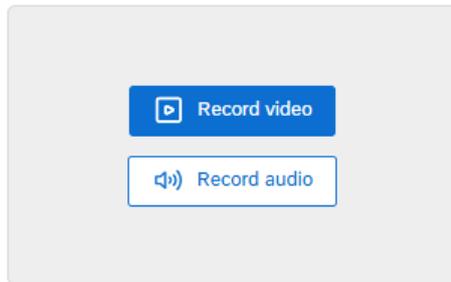
*[If Treatment == 1:]*

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: WonderWeaveRetelling1

---

[...]

**1.B.2.3 Main Experiment: Stage 2, Product Set 1**

*Note: Product Set 2 is omitted, as it is analogous to Product Set 1 with the same modifications applied in Stage 1 from Product Set 1 to Product Set 2.*

---

Start of Block: Intro

**Welcome!**

This is the second part of a study whose first part you completed previously.

This study is designed for computer (PC or Mac) users only (desktop, laptop, etc.). If you are accessing this study on a smartphone, a tablet or any other non-PC devices, please switch to PC and enter the study again, or return the submission on Prolific.

All your answers will be anonymized. This means that we have no way of linking your responses to your identity.

Your payment will consist of a fixed participation fee as well as a potential bonus depending on your answers.

One out of one hundred participants will be randomly selected to receive their bonus based on specific criteria described later in the study.

**Note that you will need to use your microphone and speakers / headphones.**

To participate in this study, you must reside in the USA. If you do not reside in the USA, please return the study on Prolific.

---

End of Block: Introduction

Start of Block: ProlificID

**What is your Prolific ID?**

Note that this response should auto-fill with the correct ID.

---

End of Block: ProlificID

Start of Block: AttentionCheck

Oftentimes, participants click through surveys like this one without reading the instructions carefully. As a consequence, many responses cannot be used in research.

To show us that you are reading the questions carefully, please give the following answers to the question below: "Never" and "Every day".

Based on the text you read above, how often do you eat meat?

- Never
- Occasionally
- Several times a week
- Every day

End of Block: AttentionCheck

---

Start of Block: CueDreamGlow

In the first part of this study, you saw **descriptions of hypothetical products** mentioning both positive and negative aspects.

Afterwards, you provided a recorded retelling for each product.

Please take a moment to think about these recordings and write down a few keywords that come to mind for each product.

Consider this product:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



Please write down a few keywords that come to mind about your recording:

*(You will be able to proceed after 10 seconds.)*

End of Block: CueDreamGlow

**Start of Block: CueEchoScent**

In the first part of this study, you saw **descriptions of hypothetical products** mentioning both positive and negative aspects.

Afterwards, you provided a recorded retelling for each product.

Please take a moment to think about these recordings and write down a few keywords that come to mind for each product.

Consider this product:

**Echo Scent**

*A personal fragrance device that can capture and replay scents from your favorite memories.*



**Please write down a few keywords that come to mind about your recording:**

*(You will be able to proceed after 10 seconds.)*

**End of Block: CueEchoScent**

---

**Start of Block: CueWonderWeave**

In the first part of this study, you saw **descriptions of hypothetical products** mentioning both positive and negative aspects.

Afterwards, you provided a recorded retelling for each product.

Please take a moment to think about these recordings and write down a few keywords that come to mind for each product.

Consider this product:

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*



**Please write down a few keywords that come to mind about your recording:**

*(You will be able to proceed after 10 seconds.)*

**End of Block: CueWonderWeave**

---

**Start of Block: Retelling2Intro**

Now we ask you to **retell the product descriptions once more to other participants.**

These other participants **have not seen the original product descriptions** that you previously saw.

The participants who will receive your recordings now are **completely different participants** from the participants who received your retellings in the first part of the study.

Additionally, no single participant will ever receive more than one retelling from you.

Page Break

---

**In your voice recording, you want to convey an accurate impression of the products to the other participant.**

This means:

- The other participant will be asked to give an honest assessment of the product quality **solely based on your recording**. This other participant neither saw the product description nor received any other recording about this product.
- This answer of the other participant may be selected to determine your bonus.
- **It is best for you if the rating the other participant gives is as close as possible to the average rating of other participants who saw the product description like you did.**

Page Break

---

**Important: You must not communicate a numerical rating. Instead, you should retell the product description.**

If you communicate a specific rating number, you may be excluded from the bonus payment.

End of Block: RetellingInfo

---

Start of Block: DreamGlowRetelling2

Consider this product:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*

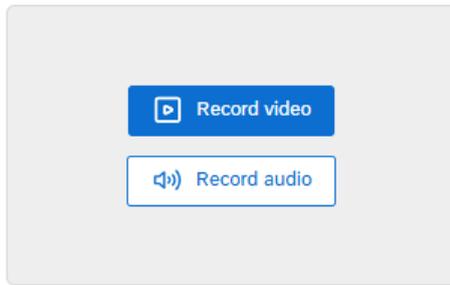


You want to **convey an accurate impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: DreamGlowRetelling2

---

Start of Block: EchoScentRetelling2

Consider this product:

**Echo Scent**

*A personal fragrance device that can capture and replay scents from your favorite memories.*

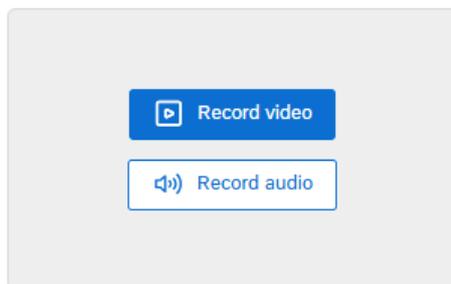


You want to **convey an accurate impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: EchoScentRetelling2

---

Start of Block: WonderWeaveRetelling2

Consider this product:

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*

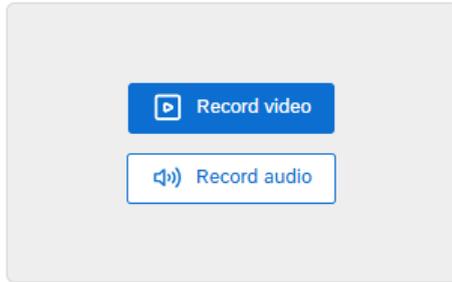


You want to **convey an accurate impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: WonderWeaveRetelling2

---

Start of Block: Rating2Intro

In the first part of this study, you also provided a rating for each product.

Now we ask you to recall your rating.

End of Block: Rating2Intro

---

Start of Block: DreamGlowRating2

Consider this product:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*

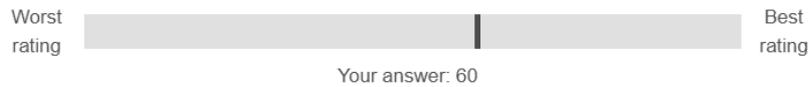


On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.



*[after clicking on the slider:]*



This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants from the first session who saw the exact same product description as you did in the first session.

End of Block: DreamGlowRating2

---

Start of Block: EchoScentRating2

Consider this product again:

**Echo Scent**

*A personal fragrance device that can capture and replay scents from your favorite memories.*

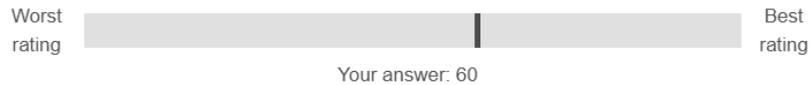


On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.



*[after clicking on the slider:]*



This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants from the first session who saw the exact same product description as you did in the first session.

End of Block: EchoScentRating2

---

Start of Block: WonderWeaveRating2

Consider this product again:

**Wonder Weave**

*Clothing that changes color and pattern based on temperature.*



On a scale from 0 to 100, how would you personally rate this product?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

Worst rating  Best rating

Please click on the slider.

*[after clicking on the slider:]*

Worst rating  Best rating

Your answer: 60

This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to the average answer of other participants from the first session who saw the exact same product description as you did in the first session.

End of Block: WonderWeaveRating2

---

Start of Block: Survey

Did you use any aids during the study?

For example, did you take notes on product descriptions using your phone or a sheet of paper? Or did you take screenshots of the product descriptions?

There are no negative consequences whatsoever if you now answer "Yes," but it would greatly help us in evaluating the responses. Please be honest as it would truly help us a lot.

- Yes, I used aids.
- No, I did not use aids.

End of Block: Survey

---

Start of Block: End

Thank you for taking part in this study.

**Please click the button below to be redirected to Prolific and register your submission.**

End of Block: End

---

#### **1.B.2.4 Robustness Experiment**

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**Start of Block: Intro****Welcome!**

This study is designed for computer (PC or Mac) users only (desktop, laptop, etc.). If you are accessing this study on a smartphone, a tablet or any other non-PC devices, please switch to PC and enter the study again, or return the submission on Prolific.

All your answers will be anonymized. This means that we have no way of linking your responses to your identity.

Your payment will consist of a fixed participation fee as well as a potential bonus depending on your answers.

One out of one hundred participants will be randomly selected to receive their bonus based on specific criteria described later in the study.

**Note that you will need to use your microphone and speakers / headphones.**

To participate in this study, you must reside in the USA. If you do not reside in the USA, please return the study on Prolific.

---

Page Break

**Data Protection and Informed Consent**

*Please consent to the processing of your data and our privacy policy.*

To display the full privacy policy click here

*[if clicked:]*

**Who is responsible for the study?**

Responsible for the execution of the study and therefore for the processing of your data: Alexander Laubel  
Bonn University  
Regina-Pacis-Weg 3  
53113 Bonn  
Germany

**What are the purposes of the study?**

The purpose of this study is to improve our understanding of human behavior in economic contexts. We are interested in natural, unbiased behavior. Hence, no details on the background of this research project is given beforehand, which is in accordance with the standard in experimental economics. All necessary information will be provided in due time.

**What happens to my data?**

All participating employees and scientists work in accordance with the provisions of the Data Protection Regulation, the Federal Data Protection Act and the relevant State Data Protection Acts. Immediately after the collection, your data will be stored anonymized and then statistically analysed. No conclusions about your person can be drawn from these results. Your data will be stored by a technical service provider outside the EU. We have taken all necessary precautions and concluded all contracts which are necessary in order to comply with data protection. Even if the service provider is not located in the EU, your data is secure as we have concluded contracts with the service

provider designated for this purpose in the EU. For this study, we collect "special categories of personal data", in particular data revealing migration background (racial or ethnic origin - term according to Art. 9 GDPR), political opinions, religious or philosophical beliefs or trade union membership, genetic data, health data or data concerning a person's sex life or sexual orientation, which we use only for research purposes and solely with your consent.

**Declaration of Consent**

I hereby consent to the processing of my personal data for this research project for the questions on human behavior. I can withdraw my consent at any time. I have taken note of all information concerning the usage of my data and on my rights in the privacy policy. Your data will be stored and analyzed in full compliance with the highest standards of the data protection laws of the European Union. In particular, no conclusions about your person will be drawn. You can withdraw your consent at any time.

I consent

I do not consent

*[If "I do not consent" was selected, survey ends.]*

End of Block: Introduction

---

Start of Block: AttentionCheck1

This question concerns the following problem: Often, participants click through surveys like this one without reading the instructions carefully. As a consequence, many of the participants can't be used in research. To show us that you are reading the questions carefully, please give the following answers to the question below: "Very interested" and "I've never heard of it".

Based on the text you read above, what have you been asked to answer to the following question: How interested are you in American Football?

Very interested

A bit interested

Not interested

I've never heard of it

End of Block: AttentionCheck1

---

Start of Block: MicCheck

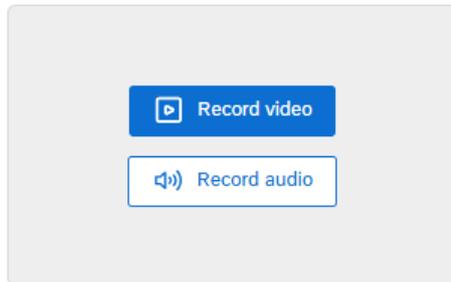
**Please talk for at least 10 seconds about your opinion on daylight savings time.**

Whether you are in favor or against daylight savings does not affect your eligibility to participate in this study.

However, we ask that you talk for at least 10 seconds about this topic.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your opinion.
- 5) After 10 seconds of recording, you can click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record your opinion again.
- 7) If you are clearly understood, proceed to the next page.



End of Block: MicCheck

---

Start of Block: ProlificID

**What is your Prolific ID?**

Note that this response should auto-fill with the correct ID.

End of Block: ProlificID

---

Start of Block: AttentionCheck2

The beverage test you are about to take is very simple, when asked for your favorite beverage you must select 'Lipton Ice Tea.' This is an attention check.

Based on the text you read above, what beverage have you been asked to answer?

- Bud Light
- Coca-Cola
- Mountain Dew
- Gatorade
- Lipton Ice Tea
- Ocean Spray Juice

*[If both attention checks failed, survey ends.]*

End of Block: AttentionCheck2

---

Start of Block: IntroProduct

The actual survey starts now.

You will see a **short description of a hypothetical product**.

The **description contains positive and negative aspects of the product**.

Please **treat the product description as if it was real**.

Please read the product description carefully.

Afterward, you will need to answer questions about the product.

End of Block: IntroProduct

---

Start of Block: DreamGlowRating1

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*

Perfect for **relaxation**, a **romantic night** or even to **educate about astronomy**.

Additionally, it has a sunrise feature and can **gently wake you up with a simulated sunrise, improving wake-up routine**.

However, it **needs a constant power source** for placement.

The **ceiling projector has limited accuracy in depicting real constellations**.

It also **requires a darkened room, limiting daytime use**.



*(You will be able to proceed after 30 seconds.)*

Page Break

On a scale from 0 to 100, how would you personally rate this product?

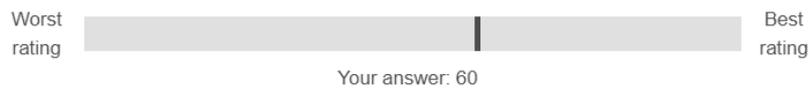
Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

### **Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



*[after clicking on the slider:]*



This answer can be relevant to determine your bonus payment.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances

the closer your answer is to the average answer of other participants. These other participants saw the exact same product description as you did.

End of Block: DreamGlowRating1

---

Start of Block: RetellingInfo

Now you will be informing another person about the product.

For this purpose, you will record audio message for each product that will be sent to another participant.

**This other participant has not seen the original product description that you just saw.**

*[If Treatment == 0:]*

**In your voice recording, you want to convey an accurate impression of the product to the other participant.**

This means:

- The other participant will be asked to give an honest assessment of the product quality **solely based on your recording**.
- This assessment by the other participant may be selected to determine your bonus.
- **It is best for you if the rating the other participant gives is as close as possible to the average rating of other participants who saw the product description like you did.**

*[If Treatment == 1:]*

**In your voice recording, you want to convey an overly positive impression of the product to the other participant.**

This means:

- The other participant will be asked to give an honest assessment of the product quality **solely based on your recording**.
- This assessment by the other participant may be selected to determine your bonus.
- **It is best for you if the rating the other participant gives is as close as possible to 100.**
- The other participant does not know that your recording is overly positive (but it is also not relevant for their payment).

**Important: You must not communicate a numerical rating. Instead, you should retell the product description.**

If you communicate a specific rating number, you may be excluded from the bonus payment.

End of Block: RetellingInfo

---

Start of Block: DreamGlowRetelling1

Consider this product:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



*[If Treatment == 0:]*

**You want to convey an accurate impression of the product to the other participant.**

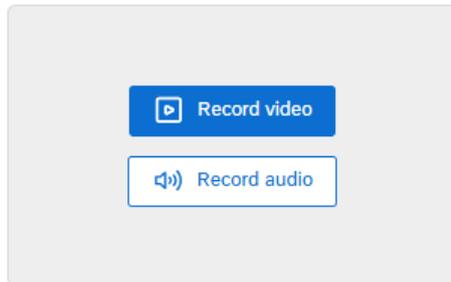
[If Treatment == 1:]

You want to **convey an overly positive impression of the product** to the other participant.

Please upload a voice recording of how you retell the product description.

Here is how it works:

- 1) Click on "Record audio" below. (Do not click on "Record video".)
- 2) You may need to agree to use your microphone.
- 3) Then click on the red button to start recording.
- 4) Record your retelling.
- 5) Click the red button again to stop recording.
- 6) Briefly listen to your recording to ensure you are clearly understood. If not, please delete the recording and record again.
- 7) If yes, proceed to the next page.



Your recording cannot exceed 45 seconds.

**Each recording will be sent to a different randomly selected participant. This other participant neither saw the initial text description nor received any other recording for this product.**

End of Block: DreamGlowRetelling1

---

Start of Block: RatingAgain

You will now be asked to recall the rating you gave the product.

End of Block: RatingAgain

---

Start of Block: DreamGlowRating1.5

Consider this product again:

**Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



What rating did you give this product earlier in the survey? Please state your initial rating again.

Worst rating  Best rating

Please click on the slider.

*[after clicking on the slider:]*

Worst rating 

 Best rating

Your answer: 60

This answer can be relevant to determine whether you get the bonus.

Details:

If this answer is randomly selected to determine your bonus, you increase your chances the closer your answer is to your initial rating of this product.

End of Block: DreamGlowRating1.5

---

Start of Block: Survey

This is the final question of the survey.

Did you use any aids during the study?

For example, did you take notes on product descriptions using your phone or a sheet of paper? Or did you take screenshots of the product descriptions?

There are no negative consequences whatsoever if you now answer "Yes," but it would greatly help us in evaluating the responses. Please be honest as it would truly help us a lot.

- Yes, I used aids.
- No, I did not use aids.

End of Block: Survey

---

### **1.B.2.5 Coding Instructions for the Classification of Retellings**

*[block below was looped for each audio recording, displaying the appropriate product]*

---

Start of Block: Loop

### **Instructions**

Your task is to listen to audio recordings and answer questions about each recording.

The recordings are part of a larger study, in which some subjects provided these recordings ("senders") and other subjects will later listen to them ("receivers").

What you will do is very valuable for us because it helps us to get a better idea of the nature of these recordings.

You will be listening to **all** the recordings.

However, in the real experiment each receiver **will only listen to one recording**.

**Therefore, when listening to a recording behave as if you were only listening to that recording.**

**That is, when listening to a recording disregard all the other recordings you have already heard and consider the specific audio recording in isolation.**

Below, your tasks starts.

### **Product**

#### **Dream Glow**

*A smart ceiling projector that can also simulate sunrise.*



### **Audio Recording**



#### Question 1

Is the recording (mostly) understandable in terms of the audio quality? (If the accent is really impossible to understand, please also select "No.")

- Yes
- No

#### Question 2

If you think the audio recording really provides no basis to form a rating on, please click "too little information." In that case, skip questions 3 and 4, and continue to the next recording.

- Sufficient information
- Too little information

#### Question 3

Some senders were asked to provide an "overly optimistic" recording while others were asked to provide an "accurate" recording.

Would you say this recording was "accurate" or "overly optimistic"?

The idea behind this question is to see whether the manipulation in the experiment worked and whether subjects indeed provide overly optimistic ratings when asked to do so.

Please answer to the best of your knowledge and only answer "I do not know" if you really have no idea.

Important: disregard all the other recordings you have heard and only view this recording in isolation.

- The recording was accurate.
- The recording was overly optimistic.
- I do not know.

#### Question 4

Now we want you to assume that the recording was accurate. In the main experiment, receivers will not know that some recordings are overly optimistic; they will just assume they are accurate. Pretend that this is the case for you, too.

How would you rate this product solely based on the recording on a scale from 0 to 100?

Think about the product's appeal and desirability in terms of its features, design, and anything else that might be relevant to you.

(It is clear that you do not have perfect information to do this; some audio recordings are very short and fragmentary. Just form an honest, subjective opinion based on the recording.)

Important: disregard all the other recordings you have heard and only view this recording in isolation.

Worst rating  Best rating

Please click on the slider.

*[after clicking on the slider:]*

Worst rating  Best rating

Your answer: 60

End of Block: Loop

---

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## Chapter 2

# Illusion of Control in a Complex Environment\*

### 2.1 Introduction

Do people overestimate their control over outcomes? This laboratory experiment investigates such a tendency, which has sometimes been called *illusion of control*, in a tightly controlled setting. Subjects' beliefs about their likelihood of winning a board game are compared in a randomized controlled design that varies whether players generate die rolls with a physical die or with a die roll computer. The main research question is: Do subjects hold a different (specifically, higher) belief about winning the board game when rolling the physical die compared to when using a die roll computer?

To motivate the importance of this question, consider *illusion of control* to be a central phenomenon – more specifically, a bias – within the domain of *beliefs about control*, analogously to, for instance, *overconfidence* in the domain of *beliefs about ability*. In psychology, *beliefs about control* were conceptualized by Rotter (1966) as “locus of control,” referring to whether a person believes they have control over events in their life (internal locus of control) or whether external factors such as

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luck, fate or other people's actions are more important (external locus of control). For a discussion of how *illusion of control* relates to *locus of control*, see Section 2.2. Beliefs about control were shown to be interrelated with various highly central concepts in human life. First, beliefs about control were found to be related to the happiness derived from certain outcomes. In particular, a wide range of psychological studies interpret people's judgments of control as reflecting a need to maintain and enhance self-esteem, and to foster well-being. Perceptions of uncontrollability have also been linked to negative emotional, cognitive and motivational aspects, and even depression.<sup>1</sup> Furthermore, beliefs about control were shown to be related to whether someone is seen as deserving an outcome.<sup>2</sup> Third, beliefs about control were found to be connected with the desirability of an outcome.<sup>3</sup>

To clarify the economic relevance of understanding beliefs about control, consider the following examples.

In financial contexts, investors may believe that they are more in control of their returns than they actually are, and this could help to understand overconfidence as well as trading patterns such as excessive trading, which has frequently been observed in the literature (Odean, 1999; Barber and Odean, 2000; Barberis and Thaler, 2003). Moreover, illusion of control on the part of retail investors might for instance explain the widespread existence of actively-managed funds despite evidence that they do not outperform passive alternatives (French, 2008; Fama and French, 2010).

Furthermore, for example in both labor market and health contexts, illusion of control can have important implications. It is relevant for our understanding of labor markets whether individuals tend to overestimate the degree of control they have over their job prospects. Likewise, whether people believe they are more responsible for their health outcomes than warranted has important implications. These questions are not only central for understanding individual behavior but also for explaining policy preferences, in particular with regard to social security and redistribution.

While all these questions in financial, labor market or health contexts undoubtedly are of tremendous relevance, in light of all kinds of unobserved moving parts and the lack of exogenous variation, cleanly identifying illusion-of-control effects is a major challenge. This study, therefore, focuses on a highly controlled laboratory setting to cleanly identify the causal effect of perceived agency on beliefs.

1. See Heider (1958), Overmier and Seligman (1967), Kelley (1973), Lefcourt (1973), Langer (1975), Miller and Ross (1975), Abramson, Seligman, and Teasdale (1978), Weiner (1979), Bandura (1989), Thompson and Spacapan (1991), Koenig, Clements, and Alloy (1992), Thompson, Armstrong, and Thomas (1998), and White (2003).

2. See, for example, Trhal and Radermacher (2009), Krawczyk (2010), and Mollerstrom, Reme, and Sørensen (2015).

3. See Miller and Ross (1975), who coined the term *self-serving bias* for this phenomenon, or Bradley (1978).

As discussed in more detail in Section 2.5, the results of the pre-registered main data collection show no treatment effects in any of the pre-registered analyses, nor in additional robustness checks. As such, the results stand in contrast to the majority of the psychology literature that consistently finds an illusion-of-control effect (see Presson and Benassi, 1996; Stefan and David, 2013, for meta-analyses) as well as to two earlier experiments with closely related designs that suggested the existence of a treatment effect.<sup>4</sup> However, the null results of the main experiment align with Labajova et al. (2022), Filippin and Crosetto (2016) and Charness and Gneezy (2010), who also find no effect, or Schütze et al. (2024) who find that illusion-of-control effects are moderated by psychological traits.

The remainder of this chapter is organized as follows. Section 2.2 discusses the relevant literature in economics and psychology. Section 2.3 describes the research design and the hypotheses. Section 2.4 presents a formalization of the research questions and how they are studied empirically. Section 2.5 describes the results, and Section 2.6 concludes.

The experiment was pre-registered under AEARCTR-0013367.

## 2.2 Literature

This section first highlights the psychological studies that laid the groundwork for research on illusion of control, followed by a discussion of the limited economic literature. It concludes by outlining the ways in which the present study advances the existing body of research.

### 2.2.1 Psychology Literature

As already noted in the introduction, illusion of control is related to *locus of control*; see Rotter (1966) for the foundational contribution on the latter. Before turning to the literature on illusion of control, it is useful to briefly outline the key differences between the two concepts. Locus of control is a personality trait that describes how individuals attribute the causes of events in their lives. That is, whether subjects believe they have control over events in their life (internal locus of control) or whether external factors such as luck, fate or other people's actions are considered to be more important. Accordingly, locus of control is typically measured through survey instruments and relates to general life events and outcomes. Illusion of control, by contrast, refers to a specific, systematic, and cleanly identified bias whereby individuals believe they have more control than they actually do. In other words, locus of control does not carry the normative implication of being a *bias*, which is inherent to the notion of illusion of control.

4. These previous two experiments are also discussed in Section 2.3.1 and Section 2.5.

The concept of illusion of control in psychology derives from Langer (1975) who defines illusion of control as “an expectancy of a personal success probability inappropriately higher than the objective probability would warrant” (p. 311). It was “hypothesized that factors that are associated with skill situations (e.g., practice, competition, choice), when introduced into chance situations, lead people to perceive a degree of control over outcomes” (Presson and Benassi, 1996, p. 496). While early studies (e.g., Langer, 1975; Langer and Roth, 1975) focused on the issue of illusory control in entirely uncontrollable situations, Thompson et al. (2007) have extended this focus and shown that perceptions of illusory control can also occur in contexts where some level of controllability is present. The present study follows the former idea by focusing on de facto irrelevant physical engagement as a factor that is usually associated with skill, but considers both situations that are purely chance and those in which skill also plays a role.<sup>5</sup>

For a synthesizing review of the illusion-of-control literature, see Thompson, Armstrong, and Thomas (1998). The authors summarize the literature and identify conditions that, according to them, influence illusion of control as well as provide explanations for it. They also provide a unifying theory called the “control heuristic” (see also Thompson et al., 2004; Thompson et al., 2007). Thompson (2022) provides a summarizing handbook chapter.

Furthermore, there are two psychological meta-analyses on illusion of control: Presson and Benassi (1996) and Stefan and David (2013).

Presson and Benassi (1996), reviewing 53 experiments from 29 articles, find a “positive, consistent, and moderately strong illusion of control effect [...] across different tasks, in many situations, and by numerous independent researchers” (p. 506). They also point out that with regard to illusion of control, the psychology literature (at least until 1996) generally distinguished between *illusory control* and *illusory prediction*. *Illusory prediction* denotes the idea that subjects have the illusion that they can predict outcomes they have no control over. And, “[o]ddly enough, few experiments have actually measured illusory control in the sense that participants judge the extent to which they directly affect outcomes [i.e., in the *illusory-control* sense; A. L.]” (p. 496).<sup>6</sup>

Stefan and David (2013), explicitly building on Presson and Benassi (1996) and only covering publications after 1996, also find that “[g]enerally, experiments tend

5. That is, while the die roll itself is purely random, the events about which beliefs are elicited include both situations determined solely by chance and situations that involve an element of skill.

6. As a result, most of the traditional psychology literature has a different focus than the present study, which precisely aims at measuring illusion of control in this *illusory-control* sense that has, according to Presson and Benassi (1996), been studied only to a limited extent in the psychology literature.

to obtain moderate to large effect sizes, and the illusion of control is constantly present as an effect of several situational and psychological factors” (p. 377).<sup>7</sup>

To mention some exemplary findings suggestive of illusion of control in the psychology literature, Wortman (1975) and Benassi et al. (1981) find that participants judged that they had more control over which side of a die landed face-up when they could have a guess prior to the die roll. Additionally, Wortman (1975) found that when participants can pick whether heads or tails makes them win a reward, they think it is more likely that they will win the reward compared to when the experimenter determines which side corresponds to the rewards. Several studies found that participants believed they were (at least partly) in control over outcomes even though they were not.<sup>8</sup> Research has also focused on the factors that mediate illusion of control. For example, Fenton-O’Creevy et al. (2003) claim that illusion of control is more pronounced in situations that are perceived as stressful or competitive such as financial trading. Dunn and Wilson (1990) find that illusion of control diminishes when stakes are high and its presence in low-stakes situations may, according to them, stem from psychological benefits such as enhanced mood. Further, it was found that people think that chances of success are higher if they are familiar with the randomization procedure/device (e.g., Benassi, Sweeney, and Drevno, 1979; Thompson, 1999) or are themselves more involved in the randomization procedure (e.g., Langer, 1975; Dunn and Wilson, 1990). Moreover, the timing of the outcome can also affect perceived control, with future events being more prone to illusion of control (e.g., Strickland, Lewicki, and Katz, 1966; Williams and LeBoeuf, 2017; Klusowski, Small, and Simmons, 2021).

Last, illusion of control also features in the gambling literature (Ginakis and Ohtsuka, 2005; Ejova, Delfabbro, and Navarro, 2010; King, Ejova, and Delfabbro, 2012).

For more recent studies after Stefan and David (2013)’s meta-analysis, see for example Yarritu, Matute, and Vadillo (2014), Williams and LeBoeuf (2017) or Klusowski, Small, and Simmons (2021).

Yarritu, Matute, and Vadillo (2014) employ a design of active and observing subjects, with active subjects administering a drug to cure an illness (in a hypothetical, experimental setting). They find that illusion of control seems to be driven less by personal engagement (as identified by comparing active and observing subjects), and more by the frequency of a presumed cause (in this case: administering

7. The overall weighted mean effect size was 0.62 SD (CI = [0.49, 0.75]). The effect was robust with respect to the choice of independent variables, the type of measurement and the task presentation (see Stefan and David, 2013, Table 1).

8. See Alloy and Abramson (1979), Ono (1987), Matute (1995, 1996), Rudski, Lischner, and Albert (1999), Thompson (1999), and Vyse (2013).

the drug). The authors interpret these results as supporting a cognitive explanation rather than a motivational/self-serving one for illusion of control.<sup>9</sup>

Williams and LeBoeuf (2017) find that consumers believe they will have more control over the future than they did (for similar events) in the past.

Klusowski, Small, and Simmons (2021) study the effect of choice on illusion of control in 17 experiments with a large sample size ( $N = 10,825$ ). They find that “[c]hoice rarely made people feel more likely to achieve preferable outcomes when all options were functionally identical.”<sup>10</sup>

Taking stock, the psychology literature has overwhelming found evidence for illusion of control (Presson and Benassi, 1996; Stefan and David, 2013), with Klusowski, Small, and Simmons (2021) being the most striking evidence against the existence of illusion of control in a highly powered study consisting of 17 experiments ( $N = 10,825$ ).

### 2.2.2 Economics Literature

Moving to the economics literature, there are far fewer studies on illusion of control than in psychology. As discussed in more detail below when outlining the contributions of this study, some of these papers can be interpreted as evidence for the existence of illusion of control as it will be formalized later, without, however, allowing for quantitative estimates at the level of subjects’ beliefs. Given the sparsity of economic evidence, the relevant papers are discussed in turn.

In Fellner (2009) participants form a portfolio of two risky lotteries (i.e., coin tosses) and one risk-free alternative, with strong incentives to pass a threshold that requires investing in the risky lotteries. Some individuals randomly get to toss one of the two coins that resemble the risky investments. Fellner (2009) finds that subjects invest excessively in the coin toss when they are in charge of tossing that particular coin. However, this excessive investment is broadly consistent with optimality due to multiple ‘approximately optimal’ responses. Thus, there is perhaps suggestive evidence that subjects think they have higher chances of winning the lottery for which they toss the coin, but since subjects’ behavior remains broadly consistent with money maximization, it is not clear whether subjects do not simply like to bet on themselves because, e.g., they like to be decisive. Since only investment behavior

9. The idea behind this cognitive explanation is that subjects exhibit cognitive biases when trying to infer causal relationships from (potential cause, outcome)  $\in \{0, 1\} \times \{0, 1\}$  pairs where both potential cause and outcome are binarized as ‘absent’ (= 0) or ‘present’ (= 1). The explanation favored by the authors is that higher (presumed) cause frequency *ceteris paribus* induces a higher belief in a causal relationship.

10. This is robust to different outcome measures (experiments 1–3), a visual process (4), variation in the level of uncertainty (5–7), or increasing the subjectivity of the outcome evaluations (8–9). Choice had an effect only when it conferred actual control (10–11). In the rare cases in which choosers felt more likely to achieve preferable outcomes (12–15), choice seemed to reflect people’s preexisting beliefs rather than cause an illusion (16–17).

was observed, the study cannot provide quantitative estimates of illusion of control at the belief level without further assumptions.

Powdthavee and Riyanto (2015) study to what extent people can be induced to believe in alleged ‘wisdom’ of an expert even if the event considered is saliently truly random. More specifically, subjects bet on coin tosses multiple times and each time they can either buy ‘predictions’ (in fact just random guesses) before the bet or, if they do not buy it before, otherwise see the prediction after the bet. By a large sample size and the law of large numbers, some of these predictions will actually be correct several times in a row – thereby suggesting a notion of ‘expert wisdom’ to some subjects. Subjects were found to be willing to pay for these predictions of truly random outcomes after witnessing only a short streak of accurate predictions. Thus, since seeing the predictions before the bet was costly, it is natural to assume that subjects genuinely believed that they could increase their success probability by buying it. Within the conceptual framework adopted in this chapter (see Section 2.4), this finding can be interpreted as evidence for the existence of illusion of control, but – again – one can only approximate its magnitude through subjects’ willingness-to-pay. Thus, in particular, one cannot directly infer subjects’ beliefs from the observed behavior without additional assumptions.

In Owens, Grossman, and Fackler (2014) subjects choose to bet \$20 either on themselves or on a randomly matched partner answering a quiz question correctly. Crucially, before the bet, the same subjects state their beliefs that they themselves or the (same) matched partner, respectively, is correct in exactly this question. Owens, Grossman, and Fackler (2014) then find that subjects bet significantly more often on themselves than an expected-money maximizer with these elicited beliefs would do. Importantly, though, subjects’ behavior could also be rationalized by a so-called *preference for authority* (and therefore a willingness-to-pay) for undertaking certain actions, e.g. tossing coins or answering quiz questions. While a *preference for authority* does not rule out illusion of control, it likewise does not entail it. In other words, the two concepts are logically independent. Perhaps, thus, subjects are simply willing to give up on the (expected) monetary pay-off because they derive pleasure from playing the quiz, without suffering from illusion of control.<sup>11</sup>

In Meissner and Wulf (2017), the authors investigate how cognitive diversity within decision-making teams affects the illusion of control bias in strategic decisions. They hypothesize that high cognitive diversity reduces this bias through increased task conflict, fostering broader information sharing and reducing overconfidence. Conversely, they predict that low cognitive diversity has no effect on the bias. They show that high cognitive diversity significantly reduces illusion of con-

11. Note that one cannot raise the same objection to Fellner (2009), since subjects’ decisions there did not have an impact on their *authority*. In other words, who did what was independent of subjects’ decisions in Fellner (2009). The same logic holds in the present study, as further discussed in Section 2.3.1.

trol, while low cognitive diversity does not affect it. However, the study cannot estimate the magnitude of the bias reduction at the level of participants' beliefs, as only decisions were observed.

Importantly, there are also several studies in economics calling into question the existence of illusion of control.

Charness and Gneezy (2010) find no support for illusion of control. In their study, subjects bet a fraction of their endowment on a die roll and there are four treatments varying who rolls the die (subject; experimenter; subject picks; subject picks but it costs a small amount to pick oneself). Importantly, even though 68% in 'subject picks' decide to roll the die, only 22% are willing to roll the die when it costs a small amount. Combined with the fact that the fraction invested in the risky asset is virtually constant across the conditions, this strongly indicates that while some people might derive pleasure from rolling the die per se (i.e., have a *preference for authority*), this preference is very weak and in particular subjects do not seem to believe in higher success probabilities from rolling the die themselves. An important limitation of Charness and Gneezy (2010) may be, though, that their experiment employs advanced economics students who can, one might argue, be thought of being more familiar with maxims of expected-money maximization (or more prone to experimenter demand).

Filippin and Crosetto (2016) also find no support for illusion of control. In their study, participants either actively roll dice to determine outcomes or click to collect lottery boxes, manipulating involvement in the decision-making process. Neither treatment significantly alters subjects' choices or their perceived probability of winning.

Labajova et al. (2022) is unique in that they study illusion of control in a field setting. Participants (78 farmers and 10 farm advisors) made hypothetical loan allocation decisions for purchasing tractors, with monetary incentives tied to performance. The experiment varied whether the participant rolled the die to determine the outcome or whether the experimenter rolled the die. The authors find no evidence supporting the presence of illusion of control. Neither perceived control nor risk preferences differed significantly between the treatments and the null result was robust across various model specifications.

Very recently, Schütze et al. (2024) study whether psychological traits moderate risk-taking behavior influenced by illusion of control in financial decisions. In their experiment, participants could choose lottery tickets under different treatments varying (i) whether they select their own winning numbers vs. having them pre-selected, and (ii) whether outcomes were determined before or after betting. Psychological traits (e.g., sensation seeking or attachment styles) were measured through questionnaires. While participants displayed greater risk-taking when they chose their own winning numbers and when the outcome was yet to occur, sensation seeking and attachment styles moderated these behaviors. Importantly, though, risk attitudes are not direct evidence for illusion of control.

Most research in the economics literature, however, tends to conflate a notion of illusion of control and other related concepts into a more general notion of over-confidence and refrains from focusing on illusion of control.

### 2.2.3 Relevance of This Study for the Literature

This study contributes to the literature in two main ways beyond simply adding empirical evidence to a sub-field in which limited economic research has been conducted.

First, the design will pay attention to *complex environments* defined as events for which the objective probability is non-trivial. These include both pure chance events with computational complexity and strategic settings where outcomes depend on players' actions. Both of these cases will be featured in the design of this chapter (see Section 2.3.1). A complex-environment design is able to detect a more subtle, 'less conscious' notion of illusion of control in the sense of a 'tendency to eyeball more favorably' when oneself rolls the die. That is, while behaving in a way that is suggestive of a belief that, e.g., the probability of tossing heads is not equal to  $\frac{1}{2}$  might be subject to a (normative) 'irrationality' criticism, this is not the case in the complex environment. Here, behaving in a way that suggests a wrong subjective belief is not so much reason to blame the subject of irrationality. An additional advantage of focusing on complex environments is that they mirror the kinds of uncertainties people encounter in real-world situations more closely than simple, well-defined chance events. Therefore, such a 'tendency to eyeball more favorably' in complex environments when objective probabilities are not straightforward can be understood as conceptually distinct from misperceiving the probability of, e.g., tossing heads.

Additionally, as described in more detail in the next section, the study particularly aims to arrive at quantitative measures of comparative-statics estimates of illusion-of-control effects on the belief level understood as a *difference in beliefs induced by some pseudo-relevant aspect of a stochastic process* (in particular, in the context of this study: how die rolls are generated). That is, while essentially all studies in the literature at best find evidence that illusion of control is at work, none allows for measuring estimates of illusion of control on the level of beliefs in such a comparative-statics fashion with a proper control condition. In order to arrive at these estimates, unlike the existing studies just discussed, the current approach employs a comparative-statics notion of illusion of control within a potential-outcomes framework (see Section 2.4). This approach enables illusion of control to be measured in its 'original currency' of beliefs rather than inferred through behavior or willingness-to-pay.

## 2.3 Research Design and Hypotheses

### 2.3.1 Research Design

As discussed above, this experiment was implemented in a board-game setting. This choice balanced control and naturalism: as it allows for an experimental implementation, it enables an unparalleled degree of control to cleanly identify effects. At the same time, the board-game setting has a naturalistic, real-world feel. In particular, the specific board game *Ludo* (*Mensch ärgere dich nicht* in German) is highly popular among the German sample, with the majority of subjects being (very) familiar with it (see Figure 2.A.1). The rules in German (exactly as presented to participants in the experiment) as well as an English translation are provided in Section 2.B.1.1.

Going into the details of the design, each subject had exactly one of three roles: *Player 1*, *Player 2*, or *Observer*. *Player 1* and *Player 2* played the board game against each other, while *Observer* watched a live stream of the game in another room. The board game did not start from the beginning, but from a specific game situation chosen for experimental purposes (see below). Moreover, subjects encountered several game situations individually, in separate cubicles of the Bonn EconLab. These situations systematically varied the relative importance of skill and chance. At the end of each session, three subjects (one per role) and one game situation were randomly selected and implemented, and the responses for that situation were incentive-relevant.

Between sessions, there were two treatment conditions: *Treated* and *Control*. The only difference between the conditions concerns how players generate die rolls. In *Treated*, *Player 1* used a physical die and a dice shuffler, while *Player 2* used a die roll computer (i.e., a tablet computer that displayed a random integer from 1 to 6 upon clicking a button). In *Control*, both *Player 1* and *Player 2* used a die roll computer each. Thus, this treatment variation induced an exogenous change in *Player 1*'s perceived agency, which according to the literature may play a key role in the emergence of illusion-of-control effects (see Section 2.2), while holding everything else constant. Treatment assignment was between sessions, rather than within subjects across game situations, both for organizational reasons and to avoid unintended priming, thereby prioritizing internal validity.

Before going through the design in detail, certain design choices will briefly be justified. The decision to present multiple game situations is one of the key differences from a previous design (conducted as what will later be referred to as *Previous Experiment 1*) and aimed at increasing the statistical power and robustness of the findings.<sup>12</sup> Another key difference from this previous experiment is that subjects did not actually play all situations for which they provided beliefs, but at the end

12. Notably, however, the study was *not* intended to be powered to detect differences between specific situations in the spirit of: 'The effect is larger in situation  $x$  than in situation  $y$ .'

of each session exactly one subject of each role as well as one game situation were randomly selected and implemented. The reason for this choice was cost-efficiency. The design of *Previous Experiment 1* is henceforth sometimes referred to as the ‘less hypothetical, less scalable’ one (the extent to which the new design is more scalable will be elaborated on later).

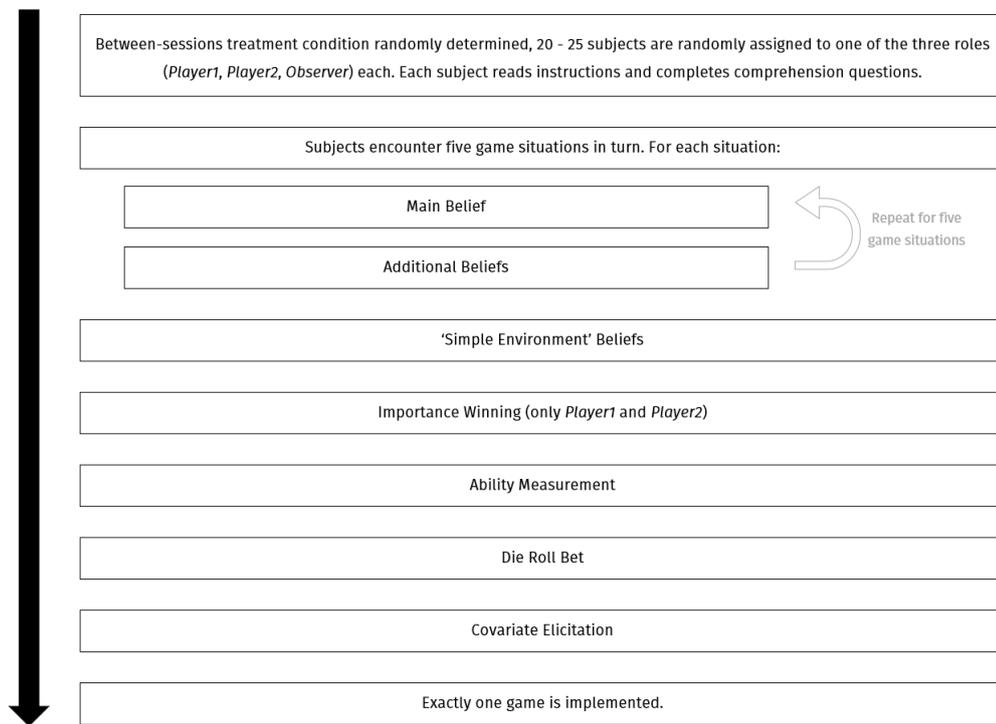
In both designs, beyond the two actual players, the *Observer* role was introduced to isolate the effect of one’s own illusion of control from alternative explanations for belief differences between *Treated* and *Control*, one important example being subjects thinking of the two random devices (physical die and die roll computer) as ‘differently truly random.’ If such an explanation were true, subjects in the *Observer* role should also exhibit such belief differences.

Last, even though treatment variation only concerned *Player 1*, subjects in the role of *Player 2* were needed to make the game implementable.<sup>13</sup> To further increase cost-efficiency, during data collection the majority of all subjects were assigned the *Player 1* role, an intermediary number were assigned the *Observer* role and only a necessary number of people were assigned the *Player 2* role.

The design is now described in detail. For an overview of the timeline of a session, see Figure 2.1 as well as the following paragraphs. For the entire verbatim instructions, see Section 2.B.1.3.

Upon entering the room, subjects immediately saw the board game prominently displayed in the center, which was intended to make the belief elicitation task feel less hypothetical (see Figure 2.2). Subjects then took individual seats in cubicles, where all data collection occurred. After introductory remarks, subjects were instructed to read the rules of the game (see Section 2.B.1.1) and could only proceed after 90 seconds. Afterwards, they answered comprehension questions on both the incentives of the experiment and the rules of the game. Thereafter, the actual experiment started and subjects encountered five game situations in turn. Each situation appeared as a photo on their screen. Subjects were told to imagine starting from this point (for *Player 1* and *Player 2*) or watching others play (*Observer*). Then the incentivized main belief was elicited: “What do you think is the likelihood of you (for the *Observer*: of *Player 1*) winning this game?” Afterwards, two additional beliefs were elicited: subjects were asked how confident they are in their main belief answer (*Confidence* belief) and how much they think skill vs. luck matters in order to win starting the game from this point (*Control* belief). Both of these additional beliefs were elicited via (unincentivized) Likert scales. This procedure was repeated across all five situations, which systematically varied the relevance of skill and luck (see Section 2.B.1.2 for an overview). The order of the five situations was random, but all subjects encountered the same five situations. Additionally, and completely

13. An alternative design could have both players use a physical die and dice shuffler each in *Treated*. In order to identify the treatment effect more cleanly, the treatment variation was designed to concern only how *Player 1* generates the die rolls, and also focus on *Player 1* in the data analysis.



**Figure 2.1.** Timeline of a Session

*Note:* The five game situations systematically varied the relevance of skill vs. luck. The main belief was incentivized and referred to the chance of winning (for *Observer* subjects: of *Player 1* winning), while two additional beliefs – confidence in the main belief and the perceived role of skill versus luck – were elicited on unincentivized Likert scales. The ‘simple environment’ belief concerned the probability of taking out *Player 2*’s piece with the next roll, which has a straightforward objective benchmark. The ability measure consisted of three *Ludo* puzzles with objectively better moves, serving as a proxy for playing skill. The die-roll bet elicited beliefs about the randomness of the physical die versus the computer die roll. At the end, one game situation and one subject of each role were randomly selected for actual play, which determined incentivization. For further details, see the main text.

orthogonally to the other randomizations, the colors of the pieces featured in the situations were randomized while holding everything else constant.<sup>14</sup>

After these five situations, an additional sixth (so-called ‘simple environment’) game situation was presented. In this sixth game situation, analogous beliefs were elicited, but crucially not for the event of winning, but for taking out *Player 2*’s piece

14. That is, each situation existed in a ‘baseline version’ that used red for *Player 1* and yellow for *Player 2*, and in a ‘swapped version’ in which red and yellow were swapped while otherwise leaving everything else unchanged. The idea was to ‘randomize away’ any effects that might be due to the colors used. The other two remaining available colors, black and green, were not used because they were hard to distinguish on the photos. Mixing either black or green with red or yellow was not feasible as the order of the colors on the board is fixed. That is, red and yellow as well as green and black are opposite of each other, and it was therefore only possible to swap red and yellow. See Section 2.B.1.2 for an overview of all game situations.



**Figure 2.2.** Laboratory Setup with Board Game Equipment

*Note:* This figure shows the laboratory set-up. Upon entering the room, subjects immediately saw the board game prominently displayed in the center, which was intended to make the belief elicitation task feel less hypothetical. Subjects then took individual seats in cubicles, where all data collection occurred. After data collection was completed, three subjects were selected to implement one of the game scenarios. The two players sat on the chairs in the center of the room, while the observer was seated in the adjacent room at the back.

in the next move. In short, this event – in contrast to the events of the previous five situations – occurs if and only if a certain number is rolled in the next move. Hence, the event has a straightforward objective benchmark of  $\frac{1}{6}$ , which motivates the term ‘simple environment.’ This situation was included to bridge the research design with the literature that has largely focused on such ‘simple environments’ with straightforward objective benchmarks.

After these beliefs were elicited, subjects were asked how important it is for them to win if selected to play (single item, not per situation; not asked for *Observer* subjects).

Thereafter, a so-called *ability measurement* took place. The idea behind this stage was that it was considered useful to have proxies for a subject’s (true) ability to play the board game. Implementation-wise, separately from the other elicitations, subjects completed three ‘*Ludo* puzzles.’ Each puzzle consisted of a game situation (different from the ones before) and a given die outcome. Subjects were then asked

which of two options would be their next move. The puzzles were designed such that there was an objective ranking of which move was better and correct responses therefore served as a proxy for game-playing ability. For example, if one can take out an opponent's piece, it is better than moving another piece that is not in any danger. For details on how all these variables were elicited, including the specific form of the Ludo puzzles, see Section 2.B.1.

Then, a *DieRollBet* belief was elicited: subjects were asked how often the experimenter would roll a 6 using that player's assigned device. That is, in *Treated*, *Player 1* and *Observer* were asked how often the experimenter would roll a 6 using a physical die and dice shuffler, and otherwise using the die roll computer. The idea behind this question was to further inquire whether subjects might think the two die-roll devices are (differently) truly random. If, for example, subjects think the die to be generally more rigged than the die roll computer, this tendency should show up in *DieRollBet*. These six main beliefs (five complex, one simple situation) and *DieRollBet* constitute the full set of incentivized responses.

Afterward, the following covariates were elicited: familiarity with *Ludo*, gender, religiosity, and preferences regarding automation.<sup>15</sup> At the very end, subjects were asked whether they thought they could cheat in the game (assuring them that stating 'Yes' would have no negative consequences for them, but that it would greatly help evaluating the answers) and what they thought the purpose of the study was (free text).

After this, data collection was completed, and three subjects and one game situation were randomly selected for implementation. That is, the selected subjects in the roles *Player 1* and *Player 2* actually played the randomly chosen game situation while the selected *Observer* watched them play in an adjacent room. Incentivization of the main belief was based on the belief stated for the implemented game situation via a binarized scoring rule (Hossain and Okui, 2013). Since every subject and every game situation could in principle be selected, each of these main beliefs was incentivized. The two additional beliefs (*Confidence* and *Control*) could not be incentivized. All remaining subjects (i.e., all but the three selected) were sent to a different room to receive the participation fee, while the three selected subjects prepared to play. The latter then received both the participation fee and a potential bonus, depending on their stated beliefs and the game outcome.

Prior to this experiment, two closely related experiments had been conducted. The first one (*Previous Experiment 1*), devised and conducted as my master's thesis, differs from the design just described in that a session only consisted of three subjects in one treatment arm. That is, two sessions had to be conducted to obtain a single (*Treated,Control*) pair. These subjects then provided only the main belief for

15. This item asked participants to rate, on a 7-point scale from 'Very negative' to 'Very positive,' how they personally evaluate the increasing automation of tasks that were previously performed by humans.

one game situation. This design yielded first positive evidence of the existence of illusion of control (see also Section 2.5), but the design was too cost-intensive in terms of time, lab usage, and research staffing. Thus, the current design was developed as a scalable alternative.<sup>16</sup> Since it was unclear whether the effect from the first design would translate to the scalable one, before going ‘all in’ financially a pilot was considered prudent. This pilot of the current design was the second experiment (henceforth: *Previous Experiment 2*). When discussing the results of the main experiment, I will briefly refer to these other two experiments when it is appropriate.

For the instructions of all three experiments, see Section 2.B.1.3.

### 2.3.2 Hypotheses

As pre-registered, the main hypothesis concerns *Player 1*. The *Observer* role serves as a ‘control role,’ and *Player 2* is not the focus of analysis. The analysis is conducted using OLS regression.

The following hypotheses were pre-registered:

**Hypothesis *Player 1*:** Considering *Player 1* subjects, when pooling the five main beliefs (*B1 - B5*) and regressing them on a binary treatment variable, controlling for game-situation fixed effects, the coefficient on the binary treatment variable is positive (i.e., the belief is higher in *Treated*).

**Hypothesis *Observer*:** Considering *Observer* subjects, when pooling the five main beliefs (*B1 - B5*) and regressing them on a binary treatment variable, controlling for game-situation fixed effects, the coefficient on the binary treatment variable is smaller than that from **Hypothesis *Player 1***.

It was specified that if elicited covariates are not well-balanced across treatment conditions, they would be included as additional controls (but not as interactions) in the regressions. In practice, all analyses will be reported both with and without control variables.

Additionally, it was pre-registered to analyze as dependent variables the *Control* (‘luck vs. skill’) belief, the sixth (simple-environment) belief, the belief about the overall importance of winning, and the *DieRollBet*. Last, it was specified to examine the *Confidence* belief as an independent variable, both per se and interacted with treatment condition.

16. To get an idea of the scalability: in the first design, it took approximately 45 minutes to collect one (*Player 1*, *Player 2*, *Observer*) observation for one treatment arm. In the scalable design, it was possible to collect as many as 120 observations (5 observations of 24 subjects), and it was even possible to assign roles in proportion to their informational value. In particular, as few people as possible were assigned to the *Player 2* role, and more subjects to the *Player 1* role than to the *Observer* role.

## 2.4 Formalization

Before turning to the results, a formalization of the research questions and their empirical investigation is presented.

### 2.4.1 The Case for a Comparative-Statics Notion of Illusion of Control

To address questions of the existence and magnitude of a causal illusion of control effect in a methodologically sound way, it is reasonable to adopt a comparative-statics framework in which only one aspect  $c$  is varied across two conditions, and the appropriate research question then spells out to: “Is there (and if so, how large is) illusion of control *when ceteris paribus changing  $c = c_1$  to  $a = c_2$* ?” For example,  $c \in \{T, C\}$  could be binary and denote whether the subject rolls a die ( $c = T$ ) or whether the die roll is generated by a computer ( $c = C$ ). The illusion-of-control effect in this context can be defined as  $IOC := p(T) - p(C)$ , where  $p(\cdot)$  denotes the belief in the respective conditions that, e.g., the die roll yields a preferable outcome.

In this formalization *illusion of control* captures any difference in beliefs arising from whether it is the subject or the computer rolling the die while holding everything else constant. (What ‘everything else’ amounts to depends on the specific environment of course.) In particular, this definition of *illusion of control* does not aspire to measure only an ‘overestimation of one’s impact’ because a difference in beliefs between  $c = T$  and  $c = C$  could, for example, also be due to an interaction between an overestimation of one’s impact and of one’s ability. It is important to highlight that this is true for the entirety of this study: estimates of illusion of control do *not* aim to isolate only an overestimation of one’s impact.

As highlighted in Section 2.2, the psychological literature mostly deviates from such a comparative-statics framework of illusion of control. Instead, it typically examines how, in purely random processes, subjects’ beliefs of some probability deviate from the *true* value (see, e.g., Langer, 1975). For instance, if a subject believes that the chance of rolling a certain number with a six-sided die is greater than  $\frac{1}{6}$ , this is sometimes interpreted as evidence of illusion of control. However, since no counterfactual is specified, this is *either* a different notion of illusion of control than the one proposed here and more in the spirit of a general, non-comparative-statics feature that can strictly speaking not be pinned down to the fact that the subject rolls the die. *Or* it is in fact the comparative-statics notion just described, but *implicitly* assumes that the difference between the subjective belief and the objective probability is *only* due to the subject rolling the die. In this latter reading, the implicit assumption is that if the die roll were instead generated by a computer, the subject would have answered or behaved in line with a belief of  $\frac{1}{6}$ .

Why is this a problem? The general, non-comparative-statics framework does not allow for causal inferences of the form ‘rolling the die caused illusion of control.’ When the objective probability is not straightforwardly computable – as is the

case in this study –, a subject might simply provide an estimate that systematically deviates from the true value, without believing that the probability is higher *because* the subject rolls the die. Precisely here, a counterfactual condition is needed to rule out biases and deviations that are unrelated to how die rolls are generated.

#### 2.4.1.1 Addressing Theoretical Challenges and Objections

A first conceptual identification objection to measuring illusion of control is that, even if a systematic difference between  $c = T$  and  $c = C$  were found, it could just as well stem from differing beliefs about the fairness of the respective random device (the physical die or the die roll computer). Thus, the argument would be that one cannot be sure to detect illusion of control since any measured difference might simply reflect differing beliefs about the fairness of the devices. Addressing this objection is in fact the core motivation for including subjects in the *Observer* role in the first place. If an allegedly found effect is due to such ‘differing beliefs about the fairness of the respective random device’ it should appear equally for the roles *Player 1* and *Observer*.

Second, this experiment does not suffer from identification problems related to a *preference for authority* (as critiqued in other experiments in Section 2.2). The reason is that subjects’ decisions do not affect their authority in the game. It is, for example, not the case that their answers determine who rolls the die, as in other experiments where a willingness-to-pay for rolling the die is elicited. In contrast, here subjects simply report beliefs without influencing the structure of the experiment.

Additionally, two further challenges arise in the attempt to measure *quantitative beliefs* within this experimental framework.

First, eliciting beliefs might itself distort them. For example, subjects may genuinely believe that they are more likely to roll a 6 than their opponent when rolling the die once. However, when asked to explicitly state their belief, they may report the correct probability of  $\frac{1}{6}$ , because the question primes them to consider the underlying mathematical structure. This potential problem is one of the reasons for studying *complex* environments: here the objective probability cannot be made salient by the elicitation process, because it is not straightforward in the first place. Still, one might object that elicitation leads subjects not to report their ‘true’ or ‘original’ belief, but instead to engage in heuristic calculations they would not otherwise have made. According to this objection, elicitation itself distorts beliefs. The reply is that, if the goal is to measure illusion of control ‘in the currency of beliefs’ (rather than through proxies such as willingness-to-pay), there is no alternative to asking directly about beliefs. One must assume that any such distortion is negligible, or at least cancels out when comparing  $c = T$  and  $c = C$ .

A second challenge arises from the fact that binarized scoring elicitation itself relies on random lotteries, while the study’s focus is precisely on subjects’ perception of randomness. A closely related criticism in the spirit of Danz, Vesterlund, and Wil-

son (2022) would be that the binarized scoring rule inherently has some methodological problems: ironically, their results suggest that the binarized scoring rule works best when it is explained least. Addressing both criticisms together: even if subjects do not fully understand the random payoff structure of the binarized scoring rule, it is sufficient that they understand it is in their best interest to report their true belief. Following Danz, Vesterlund, and Wilson (2022), subjects in this experiment were informed only that sincerity was optimal, with the mathematical details available upon request after the experiment.<sup>17</sup>

### 2.4.2 Formalization and Econometric Assumptions

For a given situation  $s$ , each subject is assumed to have a latent belief in each of the three roles and in each of the two treatment conditions. Within a role and treatment condition, this belief is modeled as the realization of an i.i.d. random variable from some well-behaved distribution  $f_{s,r,c}$  with  $r \in \{Player\ 1, Player\ 2, Observer\}$  and  $c \in \{T, C\}$ . For a given  $s$ , there are thus six random latent beliefs per subject, of which exactly one is observed. These beliefs are denoted by  $p_s(r, c)$ .

For a given  $s$ , the illusion-of-control estimates are defined as the (absolute) difference between the average belief when changing the treatment condition from  $c = C$  to  $c = T$ .<sup>18</sup> Since these estimates exist for the three possible roles  $r$ , three estimates are obtained:

$$IOC_r := \bar{p}_r(T) - \bar{p}_r(C)$$

Because treatment assignment is randomized, any individual characteristic is orthogonal to treatment assignment. The estimates therefore capture the average systematic difference in subjects' beliefs about their own probability of winning (for *Player 1* and *Player 2*) or about another subject's probability of winning (for *Observer*), varying only in whether *Player 1* uses a physical die or a die roll computer. Under the assumption that all beliefs are i.i.d. across individuals, all six estimates  $\bar{p}_r(c)$  converge to the corresponding  $\mathbb{E}[p_r(c)]$ , with  $r \in \{Player\ 1, Player\ 2, Observer\}$  and  $c \in \{T, C\}$ . Likewise, the illusion-of-control estimates converge to the corresponding true average treatment effects.

When estimating these effects in Section 2.5, a slightly different specification is used than simply comparing two subgroup averages for  $c = T$  and  $c = C$ . In particular, treatment effects are estimated using regression models, where the coefficients can be interpreted as the effect of switching from  $c = C$  to  $c = T$ . This approach has

17. Only one subject asked to see the details.

18. The comparison could likewise be defined not as an absolute difference but as a ratio. The definition here is primarily illustrative; the data analysis in Section 2.5 employs a slightly different formalization in a regression framework.

two advantages: standard errors for treatment effects are straightforwardly computed, and covariates can easily be added. The data analysis is explained in more detail in Section 2.5.

## 2.5 Data and Results

### 2.5.1 Data Collection and Exclusion Criteria

Data collection took place in the Bonn EconLab in April and May 2024.

To ensure high-quality data, it is crucial that subjects understood (i) the incentives, (ii) the rules and fundamental strategic principles of the game, and (iii) believed cheating was not possible. These concerns were addressed and, as pre-registered, subjects who satisfied *at least one* of the following criteria were excluded from data analysis: (i) subjects who did not answer all comprehension questions correctly on the first attempt, (ii) subjects who did not answer the Ludo puzzles correctly, or (iii) subjects who, at the end of the experiment, indicated that they believed cheating was possible. Subjects were explicitly told that there would be no negative consequences whatsoever from stating that they believed cheating was possible, but that it would greatly help the researchers analyzing the data if they were honest.

The five comprehension questions covered both game rules (questions 1–3) and incentives (questions 4–5) and were intentionally simple. The three Ludo puzzles tested subjects' understanding of very basic strategic aspects of the game.

Approximately 63% of all subjects passed all exclusion criteria and thus form the main subject pool of all analyses. For robustness reasons and to provide a comprehensive view, results will also be reported for partial relaxations of each exclusion criterion. For more details on how many subjects passed each combination of the criteria, see Figure 2.A.2.

### 2.5.2 Results

Turning to the results, in the pre-analysis plan, it was specified that if elicited covariates are found to be imbalanced across treatment conditions, they will be included as additional controls (but not as interactions) in the regression. In practice, all analyses are reported both with and without control variables.

#### 2.5.2.1 Main Analyses

Regarding the main hypothesis, the pre-registered analyses are OLS regressions that pool beliefs for the five game situations as dependent variable, and regress them on a treatment indicator (and a constant) with fixed effects for the game situations and clustering standard errors on the subject level.

See Table 2.1 for the results. The treatment-effect estimate is not significantly different from zero with point estimates very close to zero for *Player 1* subjects, contrary to the pre-registered hypothesis of a positive treatment effect. The analogous point estimate for *Observer* subjects is negative, but not significantly so either. The point estimate of the difference is hence positive as hypothesized, but not significantly so. For robustness specifications, see Table 2.A.1 (*Player 1*) and Table 2.A.2 (*Observer*). Importantly, though, the results for *Player 1* remain unchanged.

These findings contrast with the results from both previous experiments that suggested a treatment effect for *Player 1*. In the first experiment, which employed the less hypothetical, less scalable design, the main treatment effect was 4.6 pp ( $p = 0.041$ ), see Table 2.3. In the second experiment, which served as a pilot for this study, the treatment effect with the same exclusion criteria was 2.2 pp ( $p = 0.108$ ), see Table 2.2. Before collecting data for the main experiment, it seemed plausible that a treatment effect might be smaller in magnitude in the new design due to the more hypothetical implementation (and incentive-relevance), but overall a treatment effect was expected. The differences between these experiments and the main study are discussed in more detail later.

While the results deviate from the pre-registered hypothesis, they situate well in that part of the existing literature that calls into question the existence of illusion of control. Considering that the previous two experiments indicated an illusion-of-control effect, the overall pattern of results (i.e., all three experiments) reflects the inconclusive nature of the broader literature.

### 2.5.2.2 Secondary Analyses

Regarding the *Control* belief (that skill vs. luck matters in a given situation) there is a sizable, borderline-significant treatment effect (coefficient: 0.15-0.16 SD with and without controls, respectively,  $p = 0.065$ ), see Table 2.4. That is, *Player 1* subjects in *Treated* indeed on average believe that winning from a given situation depends more on skill (vs. luck) than in *Control*. The corresponding point estimate for *Observer* subjects, however, is also sizable (0.15 SD without controls and 0.10 SD with controls), albeit far from significantly so ( $p = 0.334$  and  $p = 0.468$ , respectively).

Regarding the simple-environment, sixth belief and the *DieRollBet*, the treatment coefficients are not significantly different from zero with point estimates close to zero.<sup>19</sup>

As for subjects' answer to how important it would be to them to win the game (only elicited for *Player 1* and *Player 2*), the treatment effect estimate is positive, but likewise not significantly different from zero (see Table 2.A.5).<sup>20</sup>

19. See Table 2.A.3 for the simple-environment, sixth belief and Table 2.A.4 for the *DieRollBet*.

20. The interaction effect between this variable and treatment is also very close to zero (coefficient 0.15 pp,  $p = 0.88$ ).

**Table 2.1.** Main Belief Regression

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	0.07 (0.10)	-0.08 (0.98)	-0.77 (1.22)	-2.06 (1.27)
Constant	65.47 (1.02)	65.94 (1.13)	66.61 (1.41)	69.47 (1.63)
FE(B2)	-32.13 (1.31)	-32.13 (1.32)	-35.82 (2.04)	-35.82 (2.05)
FE(B3)	-24.13 (1.34)	-24.13 (1.35)	-26.40 (2.11)	-26.40 (2.12)
FE(B4)	-0.72 (1.07)	-0.72 (1.07)	-3.42 (1.53)	-3.42 (1.54)
FE(B5)	-39.45 (1.33)	-39.45 (1.34)	-40.11 (2.27)	-40.11 (2.28)
Observations	1035	1035	415	415
Subjects	207	207	83	83
Controls		✓		✓

Notes: This table reports results from an OLS regression. The dependent variable is the main belief (likelihood of winning), expressed in percent (0–100). Each regression pools beliefs from the five complex-environment game situations and includes game-situation fixed effects (B2–B5; B1 is the reference). Standard errors are clustered at the subject level. Columns (2) and (4) include additional controls (gender, Ludo familiarity, religiosity, automation preference).

With regard to the role of the *Confidence* belief, while confidence is significantly positively associated with higher beliefs of winning the game, the interaction with treatment condition is negative but not statistically significantly so (see Table 2.A.6). The coefficient estimates for treatment (not interacted) are as before very close to zero and highly insignificant (coefficient 0.16 pp,  $p = 0.91$ ).

Finally, during the implementation of the boardgames, a research assistant took notes of the die rolls actually realized. See Figure 2.A.3 for the results. There is no evidence for different realizations – i.e., the two devices being differently fair – when comparing the two device types.<sup>21</sup>

### 2.5.3 Discussion of Main Experiment With Previous Experiments

When attempting to reconcile the main effects observed in the previous experiments with the null result of the current one, it remains speculative whether the

21. First, the means are 3.72 and 3.75 for die roll computer and physical die, respectively (Welch  $t$ -test  $p = 0.9$ ). Second, looking at individual die realizations of either device does not suggest systematic differences either.

**Table 2.2.** Previous Experiment 2 (Pilot): Main Belief Regression

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	1.87 (1.37)	2.20 (1.37)	1.09 (1.99)	1.58 (1.78)
Constant	63.86 (1.31)	64.61 (1.61)	67.63 (1.98)	68.63 (2.04)
FE(B2)	-31.74 (1.66)	-31.74 (1.67)	-36.54 (2.27)	-36.54 (2.32)
FE(B3)	-23.61 (1.92)	-23.61 (1.94)	-27.68 (2.41)	-27.68 (2.47)
FE(B4)	-1.56 (1.34)	-1.56 (1.35)	-4.89 (2.30)	-4.89 (2.35)
FE(B5)	-38.69 (1.86)	-38.69 (1.88)	-39.65 (2.74)	-39.65 (2.80)
Observations	470	470	185	185
Subjects	94	94	37	37
Controls		✓		✓

*Notes:* This table reports results from an OLS regression analogous to the one in Table 2.1, but using the pilot data. The dependent variable is the main belief (likelihood of winning) of the five complex-environment game situations. Values are in percent (i.e., between 0 and 100). Each regression pools the beliefs of the five game situations and includes game-situation fixed effects. Standard errors are clustered at the subject level. Columns (2) and (4) include additional controls.

**Table 2.3.** Previous Experiment 1 (Thesis): Main Belief Regression

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	4.61 (2.20)	4.19 (2.50)	-3.04 (2.50)	-3.45 (3.00)
Constant	60.77 (1.60)	63.40 (4.20)	66.19 (1.70)	65.05 (4.80)
Observations	96	96	98	98
Subjects	96	96	98	98
Controls		✓		✓

*Notes:* This table reports results from an OLS regression analogous to the one in Table 2.1, but using the data of the first experiment (less hypothetical, less scalable design). The dependent variable is the main belief (likelihood of winning). Values are in percent (i.e., between 0 and 100). Each belief was measured only once per subject (for one game situation), hence no fixed effects and standard errors are not clustered. Columns (2) and (4) include additional controls.

**Table 2.4.** Control Belief Regression

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	0.1500 (0.0827)	0.1545 (0.0827)	0.1480 (0.1531)	0.1000 (0.1380)
Constant	-0.2090 (0.0665)	-0.1008 (0.0741)	-0.1096 (0.1217)	-0.0832 (0.1570)
FE(B2)	0.0227 (0.0646)	0.0227 (0.0648)	-0.0439 (0.1091)	-0.0439 (0.1100)
FE(B3)	0.2100 (0.0742)	0.2100 (0.0743)	-0.0671 (0.1190)	-0.0671 (0.1200)
FE(B4)	0.1726 (0.0742)	0.1726 (0.0744)	-0.0270 (0.1230)	-0.0270 (0.1240)
FE(B5)	-0.0326 (0.0504)	-0.0326 (0.0505)	-0.1259 (0.0971)	-0.1259 (0.0980)
Observations	1035	1035	415	415
Subjects	207	207	83	83
Controls		✓		✓

Notes: This table reports results from an OLS regression. The dependent variable is the belief how much skill vs. luck matters to win in a given situation. Values were measured on a ten-point Likert scale and are normalized (mean zero, SD one) within each game situation. Each regression pools the beliefs of the five game situations and includes game-situation fixed effects. Standard errors are clustered at the subject level. Columns (2) and (4) include additional controls (gender, Ludo familiarity, religiosity, automation preference).

earlier findings were false positives, whether the current result is a false negative, or whether there are sufficiently systematic differences between the experiments such that they genuinely produced different outcomes.

Nevertheless, I consider it worthwhile to compare the three experiments, not least in the interest of transparency, given that they yielded different outcomes.

Before turning to design differences, it is worth noting that the final experiment encountered difficulties in participant recruitment. Although the originally planned sample size was eventually reached, more sessions were required because many did not fill up as expected. While it is unclear whether or how this would bias treatment effects toward zero, it does represent a difference between the main experiment and the two earlier ones.

Turning to the research design, a key difference in the research design concerns the description of incentives which was changed from the pilot to the main experiment in response to Danz, Vesterlund, and Wilson (2022). In both the pilot and the main experiment, a binarized scoring rule was employed (Hossain and Okui, 2013), and participants were told that it is in their best interest to answer truthfully. In the pilot, however, subjects could display the precise formula by clicking a

button, while in the main experiment subjects were told that they can request to see the formula after the experiment.<sup>22</sup> It is purely speculative at this point, but it would be consistent with Danz, Vesterlund, and Wilson (2022) if the difference in the results regarding the main treatment effect is due to this change.

Further design differences between the pilot and the main experiment are outlined below, but are generally considered minor. Aside from the incentive description, the introduction text and all five comprehension questions were identical in both versions. Regarding the elicitation of the Ludo beliefs, the five game situations used were also exactly the same and each belief that was elicited in the main experiment (main belief of winning; *Confidence* in the main belief answer; *Control* belief about the relative importance of skill vs. luck) was also included in the pilot. However, in the pilot two more unincentivized beliefs were elicited for each game situation: a *Ranking* belief and *Best/Worst* beliefs. These beliefs aimed to test whether subjects perceived their own relative skill position to shift under treatment, or whether treatment was believed to benefit all players equally. Both asked subjects to imagine that they and nine other randomly selected participants were ranked according to their skill in winning the game for the given situation. The *Ranking* belief then asked subjects: “Which position do you think you would have in this ranking?” The *Best/Worst* belief asked: “Please also provide an estimate of the winning probability for the two individuals in the first (= best) and tenth (= worst) positions of the ranking.” The purpose of these measures was to better understand the mechanisms behind a potential treatment effect.<sup>23</sup> As no sufficiently informative patterns emerged, these additional beliefs were excluded from the main experiment to reduce session length and to increase statistical power and sample size for the main belief measures.

In the main experiment, there were some further changes relative to the pilot, but these only occurred after the collection of the main outcomes.<sup>24</sup>

22. Only one participant actually did so.

23. The idea was as follows: Assume there is a treatment effect such that subjects in *Treated* report higher winning beliefs than subjects in *Control*. An interesting distinction would be whether, in a counterfactual/potential outcomes sense, subjects believe that all players, regardless of skill level, have a higher probability of winning in *Treated* than in *Control* (while their own rank remains constant), or whether they believe they themselves move up in the ranking. Thus, if there were treatment effects for both the main belief and the *Ranking* belief, but not for the *Best/Worst* belief, this would suggest a ‘moving up the ranks’ interpretation. Conversely, if there were no effect on the *Ranking* belief but an effect on the *Best/Worst* belief, this would indicate a shift in the entire skill distribution. Of course, mixtures of both are possible.

24. Taking into account comments, the main experiment asked subjects how important it would be to them to win the game if they were selected to play (elicited only once per subject, and not for *Observer* subjects). The Ludo-puzzles block measuring basic strategic reasoning was shortened from five to three puzzles, dropping the two hardest ones that subjects solved correctly only about half the time. The pilot’s socio-demographic survey also included a question on party identification, which proved uninformative, and additional blocks on locus of control, beliefs about the sources of

Compared to the first experiment (master's thesis), there are too many differences to briefly summarize them in a similar manner. The main difference is that sessions of the first experiment only consisted of three subjects (one of each role), and beliefs were elicited only for one game situation but with guaranteed implementation and incentive relevance. For the verbatim instructions, see Section 2.B.3.

## 2.6 Conclusion

This chapter presents causal evidence on illusion of control in a tightly controlled laboratory experiment. In particular, the study examines whether a pseudo-relevant aspect of the environment – namely, whether die rolls are generated by a physical die or a computer – causally affects subjects' beliefs about their probability of winning the board game.

Two preliminary experiments – one with a limited sample size in a non-scalable design, and another as a pilot of a scalable design – suggested that conducting a main experiment with a sufficiently large sample was worthwhile.

Most importantly, regarding the main belief, the patterns observed in the two previous experiments did not replicate in the main study. That is, treatment effect estimates are not significantly different from zero and coefficient-wise very close to zero. This finding holds for several robustness specifications. Ultimately, it is not fruitful to speculate whether the differing results are due to chance or a systematic factor.

As hypothesized, there is indeed a treatment effect in subjects' beliefs about the role of skill vs. luck in determining outcomes (*Control* belief). That said, this effect is not significant at conventional levels ( $p = 0.065$ ) and similar effect sizes are observed for *Observer* subjects, albeit with much higher  $p$ -values.

Overall, however, this experiment aligns with the strand of economic literature, as well as with Klusowski, Small, and Simmons (2021) in psychology, that has been more cautious in postulating a general illusion-of-control effect. This stands in contrast to much of the psychological literature, where a robust effect has frequently been reported (Presson and Benassi, 1996; Stefan and David, 2013).

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socio-economic inequality, and self-esteem, which were excluded in the main experiment to reduce duration.

## Appendix 2.A Supplementary Tables and Figures

### 2.A.1 Main Experiment: Robustness Specifications

**Table 2.A.1.** Main Experiment: Main Regression Robustness Specifications (Player 1)

	(1)	(2)	(3)	(4)	(5)
Specification	Winsorized	Duration	ComprRelax	PuzzleRelax	CheatRelax
Role	Player 1				
Treatment	-0.13 (0.90)	0.31 (1.00)	0.24 (0.88)	0.24 (0.90)	-0.08 (0.95)
Constant	65.94 (1.00)	65.73 (1.19)	66.50 (1.02)	65.88 (1.00)	65.98 (1.13)
FE(B2)	-32.11 (1.25)	-32.15 (1.38)	-32.03 (1.27)	-31.42 (1.23)	-31.90 (1.30)
FE(B3)	-24.09 (1.25)	-24.98 (1.37)	-24.88 (1.24)	-24.02 (1.22)	-23.62 (1.35)
FE(B4)	-0.76 (0.99)	-1.11 (1.15)	-1.44 (0.97)	-1.61 (1.00)	-0.76 (1.05)
FE(B5)	-39.23 (1.28)	-40.16 (1.37)	-40.19 (1.23)	-38.54 (1.25)	-39.31 (1.31)
Observations	1035	940	1255	1245	1070
Subjects	207	188	251	249	214
Controls	✓	✓	✓	✓	✓

*Notes:* This table reports robustness specifications of the main-belief regression (Table 2.1) for *Player 1* subjects. The dependent variable is the main belief (likelihood of winning), expressed in percent (0–100). Each regression pools beliefs from the five complex-environment game situations and includes game-situation fixed effects (B2–B5; B1 is the reference). Standard errors are clustered at the subject level. All columns include the full set of controls (gender, Ludo familiarity, religiosity, and automation preference). Specifications differ by sample restrictions or relaxations: *Winsorized* trims extreme belief values (the top and bottom 5% each); *Duration* excludes very fast or slow completions (the top and bottom 5% each); *ComprRelax* allows subjects to answer all comprehension questions correctly on the second try (rather than requiring correctness on the first try), *PuzzleRelax* requires at least two out of three Ludo puzzles to be answered correctly (rather than all three), and *CheatRelax* includes subjects who indicated that they believed cheating was possible. In each case, the relaxation is partial, i.e., only the specified criterion is relaxed while all other exclusion criteria remain enforced.

**Table 2.A.2.** Main Experiment: Main Regression Robustness Specifications (Observer)

	(1)	(2)	(3)	(4)	(5)
Specification	Winsorized	Duration	ComprRelax	PuzzleRelax	CheatRelax
Role	Observer	Observer	Observer	Observer	Observer
Treatment	-1.94 (1.24)	-2.94 (1.35)	-2.01 (1.11)	-2.12 (1.28)	-1.92 (1.32)
Constant	68.88 (1.52)	69.84 (1.71)	69.05 (1.51)	69.26 (1.59)	68.87 (1.62)
FE(B2)	-35.58 (1.91)	-36.07 (2.14)	-34.31 (2.04)	-33.98 (1.99)	-36.17 (1.99)
FE(B3)	-26.07 (2.03)	-26.74 (2.31)	-26.44 (1.98)	-26.12 (2.13)	-26.47 (2.02)
FE(B4)	-2.92 (1.45)	-3.93 (1.61)	-2.81 (1.44)	-4.05 (1.40)	-3.70 (1.47)
FE(B5)	-39.61 (2.14)	-40.21 (2.50)	-39.07 (2.14)	-39.07 (2.22)	-39.34 (2.16)
Observations	415	365	475	475	445
Subjects	83	73	95	95	89
Controls	✓	✓	✓	✓	✓

*Notes:* This table reports robustness specifications of the main-belief regression (Table 2.1) for *Observer* subjects. The dependent variable is the main belief (likelihood of winning), expressed in percent (0–100). Each regression pools beliefs from the five complex-environment game situations and includes game-situation fixed effects (B2–B5; B1 is the reference). Standard errors are clustered at the subject level. All columns include the full set of controls (gender, Ludo familiarity, religiosity, and automation preference). Specifications differ by sample restrictions or relaxations: *Winsorized* trims extreme belief values (the top and bottom 5% each); *Duration* excludes very fast or slow completions (the top and bottom 5% each); *ComprRelax* allows subjects to answer all comprehension questions correctly on the second try (rather than requiring correctness on the first try), *PuzzleRelax* requires at least two out of three Ludo puzzles to be answered correctly (rather than all three), and *CheatRelax* includes subjects who indicated that they believed cheating was possible. In each case, the relaxation is partial, i.e., only the specified criterion is relaxed while all other exclusion criteria remain enforced.

## 2.A.2 Main Experiment: Additional Analyses

**Table 2.A.3.** B6 Analysis

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	0.60 (1.15)	0.55 (1.14)	1.41 (0.85)	0.80 (0.89)
Constant	18.75 (0.83)	18.48 (0.95)	16.80 (0.58)	16.80 (0.81)
Observations	207	207	83	83
Subjects	207	207	83	83
Controls		✓		✓

*Notes:* This table reports OLS regression results for the sixth (simple-environment) belief. The dependent variable is the simple-environment belief (probability of taking out *Player 2*'s piece in the next move), expressed in percent (0–100). Columns (1)–(2) report estimates for *Player 1* subjects; columns (3)–(4) for *Observer* subjects. All models are estimated without game-situation fixed effects and standard errors are not clustered, as only one belief is included per subject. Columns (2) and (4) include the full set of controls (gender, Ludo familiarity, religiosity, automation preference).

**Table 2.A.4.** DieRollBet Analysis

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	-0.01 (0.07)	0.00 (0.07)	0.08 (0.11)	0.00 (0.12)
Constant	1.23 (0.05)	1.28 (0.06)	1.13 (0.07)	1.25 (0.11)
Observations	207	207	83	83
Subjects	207	207	83	83
Controls		✓		✓

*Notes:* This table reports OLS regression results for the *DieRollBet* belief. The dependent variable is the number of times that participants expected the experimenter would roll a six when using the assigned device six times. In *Treated*, the assigned device was a physical die and dice shuffler; in *Control*, it was a die roll computer. Columns (1)–(2) report estimates for *Player 1* subjects; columns (3)–(4) for *Observer* subjects. Standard errors are not clustered as only one belief was elicited per subject. Columns (2) and (4) include the full set of controls (gender, Ludo familiarity, religiosity, automation preference).

**Table 2.A.5.** Importance Winning Analysis

	(1)	(2)
Role	Player 1	Player 1
Treatment	0.11 (0.14)	0.08 (0.13)
Constant	-0.05 (0.10)	-0.21 (0.11)
Observations	207	207
Subjects	207	207
Controls		✓

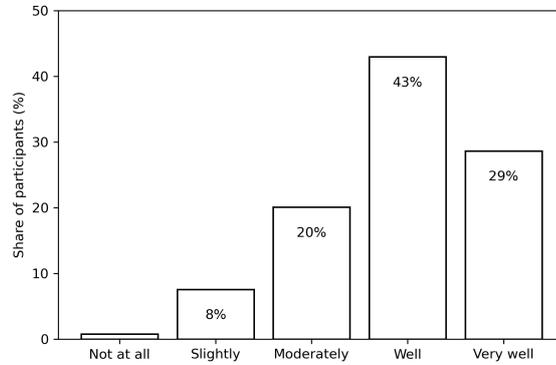
Notes: This table reports OLS estimates of the treatment effect on *Player 1*'s reported importance of winning the game (0–10 scale, standardized such that the mean equals zero and the SD equals one). The outcome was not elicited for *Observer*. Column (2) includes all control variables (gender, Ludo familiarity, religiosity, and automation preferences).

**Table 2.A.6.** Confidence Analysis

	(1)	(2)	(3)	(4)
Role	Player 1	Player 1	Observer	Observer
Treatment	0.13 (0.96)	-0.01 (0.93)	-1.06 (1.32)	-1.95 (1.36)
Confidence	1.90 (0.74)	1.84 (0.68)	0.32 (0.95)	0.26 (0.91)
Treatment × Confidence	-1.00 (1.00)	-1.15 (0.95)	-1.03 (1.46)	-0.38 (1.45)
Constant	65.34 (0.97)	66.01 (1.09)	66.65 (1.42)	68.86 (1.65)
Observations	1070	1070	445	445
Subjects	207	207	83	83
Controls		✓		✓

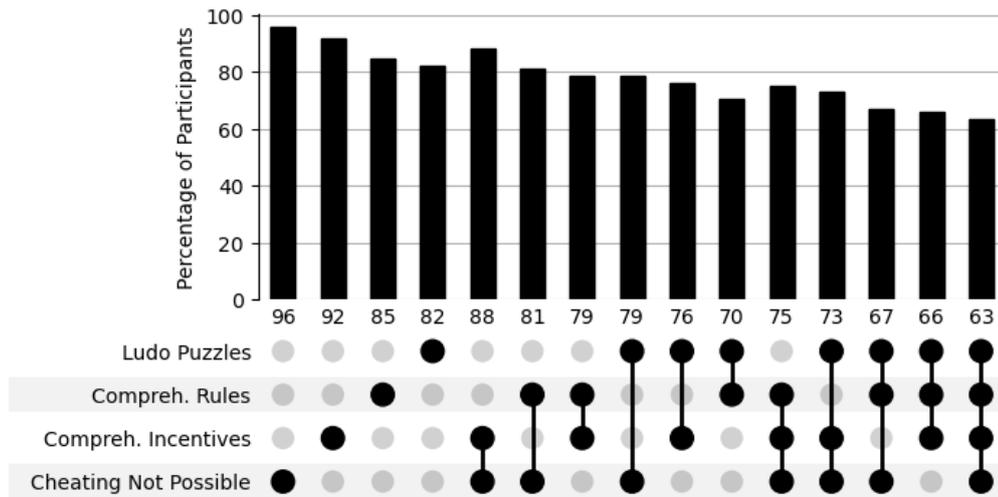
Notes: This table reports OLS estimates of the main belief (likelihood of winning) on treatment condition, confidence, and their interaction. The data pools the main belief across the five complex-environment beliefs. Confidence is standardized such that the mean equals zero and the SD equals one. Columns (2) and (4) include controls (gender, Ludo familiarity, religiosity, automation preferences). All regressions include game-situation fixed effects. Standard errors are clustered at the subject level.

### 2.A.3 Miscellaneous



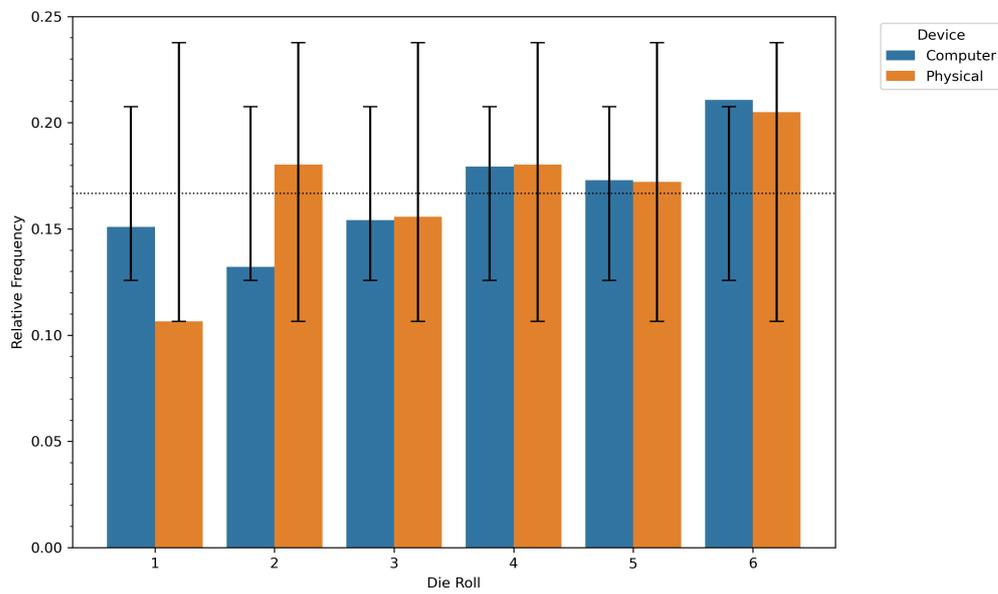
**Figure 2.A.1.** Main Experiment: Ludo Familiarity

Note: This figure depicts the distribution of self-reported familiarity with Ludo.



**Figure 2.A.2.** Main Experiment: Exclusion Criteria Pass Rates

Note: This figure displays the number of subjects who passed each exclusion criterion and every combination thereof. The comprehension check is split into two components: correct answers to the game-rule questions and correct answers to the incentive questions. The pre-registered analysis sample corresponds to the group on the far right, who passed all criteria.



**Figure 2.A.3.** Actual Die Realizations During The Board-Game Implementations

*Note:* This figure shows the relative frequencies of actual die outcomes by device type (computer-generated vs. physical) of the main experiment, based on manual recordings by a research assistant. The dotted line indicates the expected uniform frequency ( $\frac{1}{6}$ ), and the error bars represent 95% confidence intervals under the assumption of a fair device. There is no significant difference in outcomes between device types, indicating comparable fairness.

## **Appendix 2.B Instructions and Additional Information**

### **2.B.1 Main Experiment: Instructions and Additional Information**

#### **2.B.1.1 Ludo Rules**

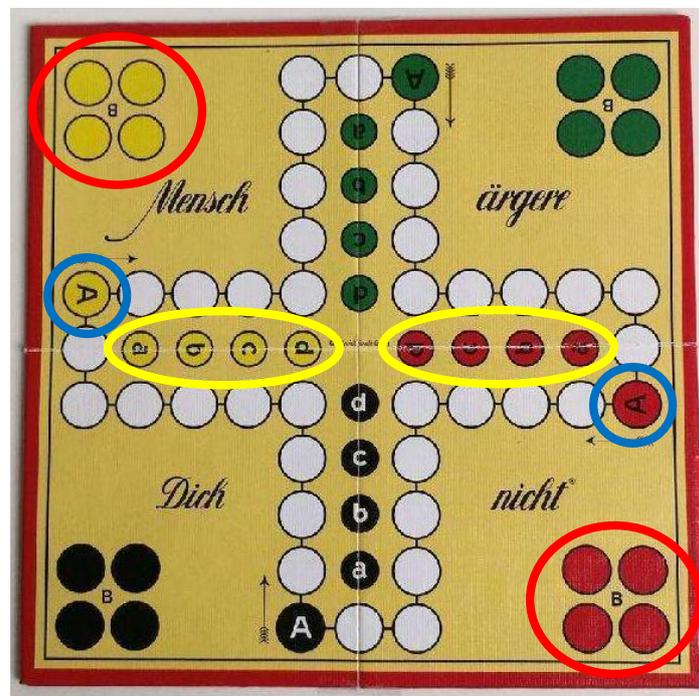
These printed-out rules were available to the subjects for the entirety of the experiment.

**German (Original)**

## Mensch ärgere dich nicht: Regeln

- Jede\*r Spieler\*in hat vier Figuren, die am Anfang im ‚Startblock‘ sind.
- Ziel ist es, alle vier Figuren in die ‚Zielfelder‘ zu bekommen. Gewinner ist, wem das als erstes gelingt.
- Die Spieler sind abwechselnd dran.
- Hat man keine Figuren auf dem Feld (also ist jede Figur entweder im Startblock oder auf einem Zielfeld), muss man eine 6 würfeln, um ‚rauszukommen‘.
  - Hat man *keine* Figur in den Zielfeldern oder füllen die Figuren die Zielfelder ‚von hinten auf‘ darf man pro Zug **höchstens drei Mal würfeln**, bis man eine 6 würfelt. Beispiel für ‚von hinten auffüllen‘: zwei Figuren in den Zielfeldern nehmen das letzte und das vorletzte Zielfeld ein.
  - Ist das nicht der Fall, darf man pro Zug **bloß ein Mal würfeln**.
  - Hat man in diesen drei Würfen bzw. in diesem einen Wurf keine 6, ist der nächste Spieler dran.
- Würfelt man eine 6 und hat noch mindestens eine Figur im Startblock, dann **muss** man diese Figur auf das Startfeld setzen und würfelt anschließend nochmal. Sofern man noch Figuren im Startblock hat, **muss** das Startfeld nach einer 6 mit diesem zweiten Wurf geräumt werden – das heißt: die Figur bewegen, die man gerade auf das Feld gesetzt hat –, außer eine eigene Figur besetzt dieses Feld. Sofern man noch Figuren im Startblock hat, muss man das Startfeld zum nächstmöglichen Zeitpunkt räumen
- Würfelt man eine 6 und hat *keine* Figur mehr im Startblock, bewegt man eine beliebige Figur auf dem Feld sechs Felder, außer man muss nach dem ‚Rauskommen‘ das Startfeld räumen. Danach darf man nochmal würfeln. Sollte man auch beim zweiten Mal eine 6 würfeln, darf man ein drittes Mal würfeln usw.
- Würfelt man eine Zahl von 1 bis 5, bewegt man eine der Figuren auf dem Feld so viele Felder, wie man gewürfelt hat. Man kann sich frei aussuchen, welche Figur man bewegt (außer man muss nach dem ‚Rauskommen‘ das Startfeld räumen). Nach einer dem Wurf einer 1 bis 5 darf man *nicht* nochmal würfeln.
- Beim Bewegen der eigenen Figur ist Folgendes zu beachten:
  - Man darf seinen Würfelwurf *nicht* aufteilen, sondern muss, wenn man beispielsweise eine 5 geworfen hat, genau eine Figur fünf Felder weit bewegen.
  - Wenn man beim Bewegen der Figur eine andere Figur (eigene oder gegnerische) überspringt, zählt das Feld mit der anderen Figur trotzdem mit.

- Man kann seinen Zug nicht auf einem Feld beenden, auf dem eine **eigene** Figur steht. Falls das der Fall wäre, dann muss man eine andere Figur stattdessen bewegen. Hat man keine Figur, die man bewegen kann, dann verfällt der Wurf.
- Beendet man seinen Zug auf einem Feld, auf dem eine **gegnerische** Figur steht, so schlägt man diese. Das heißt: man stellt seine Figur auf dieses Feld und stellt die gegnerische Figur zurück in den entsprechenden Startblock.
- Man darf auch Figuren in den Zielfeldern mit Würfeln bewegen (inklusive überspringen), um die Zielfelder ‚von hinten aufzufüllen‘.



Die Startblöcke sind rot umkreist und gekennzeichnet mit einem ‚B‘.

Die Zielfelder sind gelb umkreist und gekennzeichnet mit den Buchstaben a, b, c und d.

Wenn man rauskommt, setzt man eine Figur aus dem Startblock auf das Startfeld (umkreist in Blau, gekennzeichnet mit einem ‚A‘).

Ziel ist es dann, mit jeder Figur eine komplette Runde im Uhrzeigersinn auf den weißen Feldern zu absolvieren, bis man die eigenen Zielfelder erreicht.

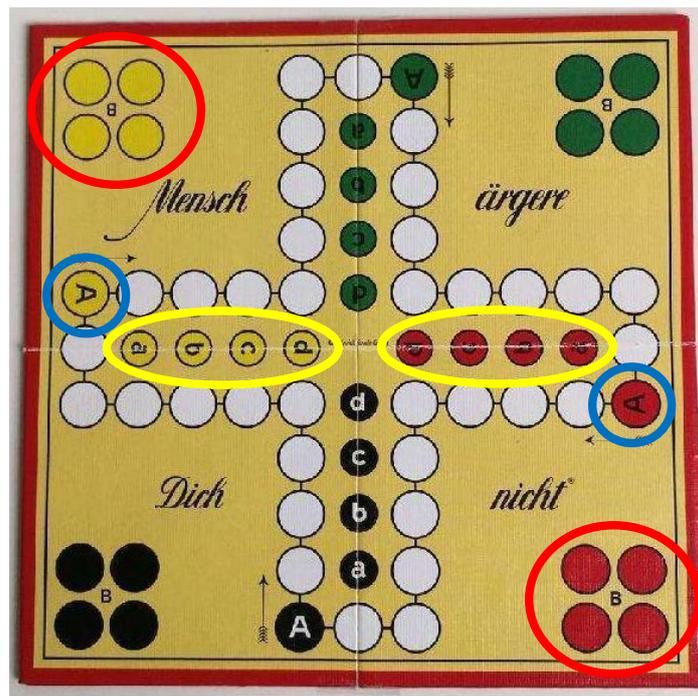
Steht man mit der letzten Figur vor den Zielfeldern und würfelt eine zu hohe Zahl, so verfällt der Wurf. Passiert dies mit einer 6, so darf man noch einmal würfeln; bei einer anderen Zahl ist der Gegner dran.

**English (Translation)**

## Ludo: Rules

- Each player has four pieces, which all start in the 'starting area.'
- The goal is to move all four pieces into the 'home fields.' The winner is the first player to do this.
- Players take turns.
- If a player has no pieces on the board (i.e., all pieces are either in the starting area or already in a home field), they must roll a 6 to 'get out.'
  - If the player has *no* pieces in the home fields or is 'filling the home fields from the back,' they may **roll up to three times** per turn to roll a 6. Example of 'filling from the back:' two pieces occupy the last and second-to-last home fields.
  - If that's not the case, the player may roll **only once per turn**.
  - If no 6 is rolled in these three (or one) attempts, the next player takes their turn.
- If a player rolls a 6 and still has at least one piece in the starting area, they **must** place this piece on the starting square and then roll again. As long as the player still has pieces in the starting area, the starting square **must** be cleared with this second roll – that is, the piece just placed on the starting square must be moved – unless the square is occupied by another of their own pieces. If there are still pieces in the starting area, the starting square must be cleared at the next possible opportunity.
- If a player rolls a 6 and has *no* pieces left in the starting area, they move any piece already on the board six spaces forward, unless they need to clear the starting square after coming out. After that, they may roll again. If they roll another 6, they may roll a third time, and so on.
- If a player rolls a number from 1 to 5, they move one of their pieces on the board forward by exactly that number of spaces. The player may freely choose which piece to move (unless the starting square must be cleared after coming out). After rolling a 1 to 5, the player does *not* get to roll again.
- When moving a piece, the following must be observed:
  - The number rolled may *not* be split; for example, a roll of 5 must be used to move exactly one piece five spaces.
  - If a piece jumps over another piece (own or opponent's), the occupied square still counts as one of the steps.
  - A player may not end their move on a square occupied by one of their **own** pieces. If this would happen, they must move a different piece instead. If no piece can be moved, the turn is forfeited.

- If a player ends their move on a square occupied by an **opponent's** piece, the opponent's piece is knocked off the board and returned to their respective starting area, while the player's piece takes its place.
- Pieces already in the home fields may also be moved (including being jumped over), in order to 'fill the home fields from the back.'



The starting areas are circled in red and marked with a 'B.'

The home fields are circled in yellow and marked with the letters a, b, c, and d.

When getting out, a piece is placed from the starting area onto the starting square (circled in blue, marked with an 'A').

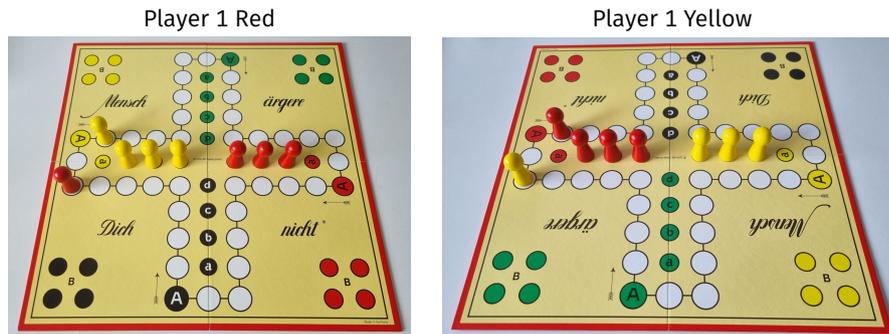
The objective is to complete a full clockwise lap around the white squares with each piece until reaching the home fields.

If a player is in front of the home fields with their final piece and rolls a number that is too high, the roll is forfeited. If this happens with a 6, they may roll again; if it's any other number, the next player takes their turn.

### 2.B.1.2 Game Situations

This is an overview of all game situations subjects encountered throughout the main part of the study and for which they provided the main belief:

#### Situation 1: Baseline



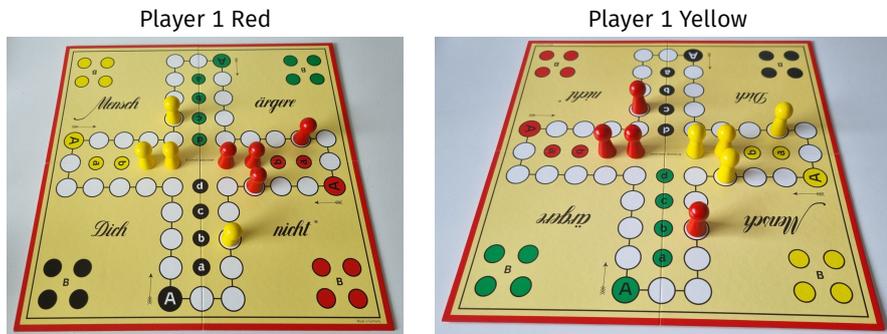
#### Situation 2: Choice One



#### Situation 3: Skill One



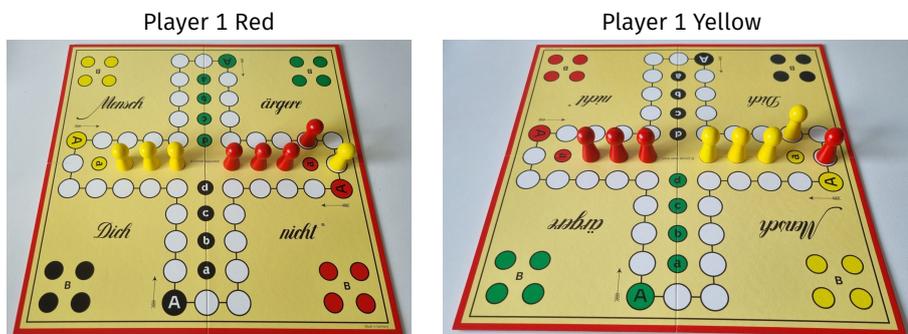
**Situation 4: Skill Both**



**Situation 5: Baseline Inverse**

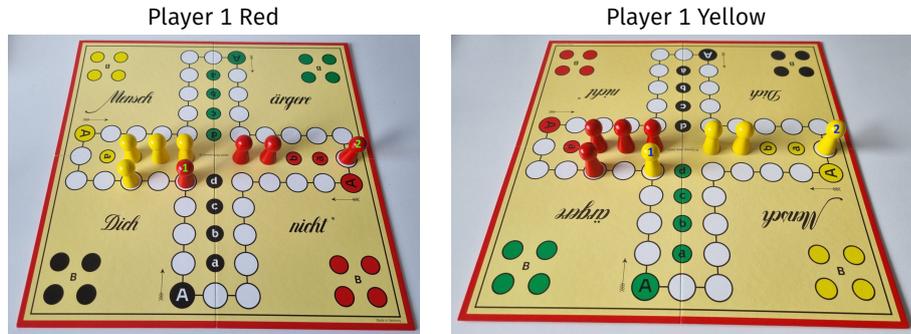


**Situation 6: Simple Environment**



Additionally, these are the situations that were used in the Ludo puzzles:

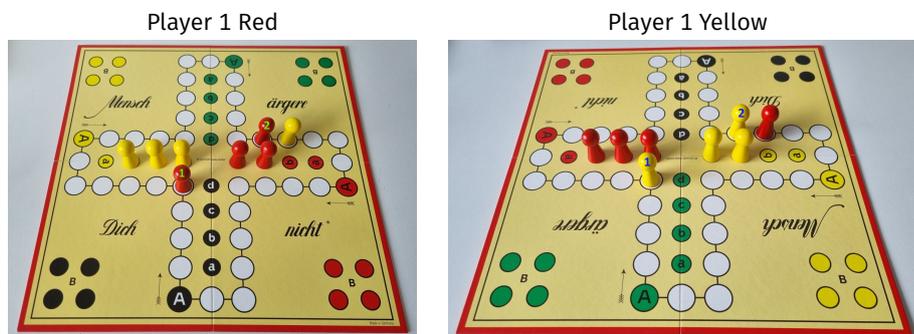
### Ludo Puzzle 1



### Ludo Puzzle 2



### Ludo Puzzle 3



The figures were marked with 1 and 2, respectively, to refer to them in the quiz questions (see below).

Last, this was the game situation used in the comprehension questions:



### 2.B.1.3 Instructions (Main Experiment)

Subjects received information on their screen and via printed-out rules of *Ludo*. All were in German. Both the original and an English translation can be found below.

#### German (Original)

*Player 1 and Player 2*

Start of Block: Intro

**Guten Tag.**

Bitte warten Sie, bis Sie die Anweisung bekommen, dass es losgeht.

Page Break

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Das heutige Experiment dreht sich um das Brettspiel *Mensch ärgere dich nicht*.

Stellen Sie sich vor, Sie spielen *Mensch ärgere dich nicht* gegen eine andere Versuchsperson.

Am Ende des Experiments werden zufällig Versuchspersonen ausgewählt, die tatsächlich *Mensch ärgere dich nicht* gegeneinander spielen.

Es ist also sehr gut möglich, dass Sie gleich tatsächlich *Mensch ärgere dich nicht* gegen eine andere Versuchsperson spielen.

Während dieses Spiels zwischen zufällig ausgewählten Versuchspersonen werden das Spielbrett und die Würfelwürfe von oben gefilmt. Auf den Videoaufnahmen sieht man nur den Tisch, d.h. man sieht von den zufällig ausgewählten Versuchspersonen, die das Spiel tatsächlich spielen, nur die Arme.

Hinweis: Ein Betrugsversuch im Brettspiel führt zum Ausschluss aus dem Experiment. Um Betrugsmöglichkeiten auszuschließen, muss mit dem Würfelbecher deutlich sichtbar gewürfelt werden.

Page Break

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Ihre Auszahlung heute besteht aus zwei Teilen: Sie erhalten sicher 12 €. Falls Sie zufällig ausgewählt werden, *Mensch ärgere dich nicht* gegen eine andere Versuchsperson zu spielen haben Sie die Möglichkeit, einen Bonus von 5 € zu gewinnen. Aus allen für die Auszahlung potentiell relevanten Antworten wird in diesem Fall genau eine zufällig ausgewählt, die tatsächlich für den Bonus relevant ist. Es ist also am besten für Sie, jede für die Auszahlung potentiell relevante Frage so zu beantworten, als wäre sie wirklich relevant.

Bei allen für Ihre Auszahlung potentiell relevanten Fragen ist es am besten für Sie, wenn Sie ehrlich antworten.

Falls Sie sich dafür interessieren, warum das der Fall ist, gehen Sie bitte am Ende der Studie auf die Studienleiter zu; sie erklären Ihnen gern die exakte Formel, die sicherstellt, dass es für Sie am besten ist, ehrlich zu antworten.

Page Break

---

Bevor es weitergeht, machen Sie sich bitte mit den Regeln von *Mensch ärgere dich nicht* vertraut, die vor Ihnen ausgedruckt liegen.

**Lesen Sie die Regeln aufmerksam durch, auch wenn Sie glauben, sie bereits sehr gut zu kennen, da es viele leicht unterschiedliche Varianten gibt.**

**Sie müssen im Laufe der Studie Verständnisfragen zu den Regeln beantworten.**

Die Regeln werden die ganze Zeit über ausgedruckt bei Ihnen bleiben.

Sie können fortfahren in: *[counter ticking down from 90 seconds]*

End of Block: Intro

---

Start of Block: Comprehension

Ihnen werden nun Verständnisfragen gestellt. Betrachten Sie diese Spielsituation:

*[ImageComprehensionSituation]*

Nehmen Sie an, **Schwarz** ist dran. Was passiert, wenn **Schwarz** eine 6 würfelt?

- Schwarz zieht sechs Felder und würfelt nochmal.
- Schwarz zieht sechs Felder und Grün ist dran.
- Schwarz macht nichts und Grün ist dran.

Nehmen Sie stattdessen an, **Grün** ist dran. Was passiert, wenn **Grün** eine 2 würfelt?

- Grün kann nicht ziehen und Schwarz ist dran.
- Grün schlägt Schwarz, d.h. Grün zieht auf das Feld, auf dem Schwarz ist, und Schwarz muss zurück in den Startblock.
- Grün zieht zwei Felder, wobei das Feld mit Schwarz nicht mitzählt.

Nehmen Sie an, **Grün** würfelt in der Tat eine 2 und anschließend ist **Schwarz** dran. Was passiert dann, wenn **Schwarz** dran ist?

- Schwarz würfelt solange bis eine 6 gewürfelt wird. Dann ist Grün dran.
- Schwarz würfelt ein Mal. Falls Schwarz eine 6 würfelt, stellt Schwarz seine Figur aufs Startfeld und zieht sechs Felder.
- Schwarz würfelt höchstens drei Mal, um eine 6 zu würfeln. Falls das gelingt, stellt Schwarz seine Figur aufs Startfeld, würfelt nochmal und bewegt die Figur entsprechend dem zweiten Würfelwurf.

Page Break

---

*[only display if at least one answer wrong]*

**Sie haben die folgende(n) Frage(n) falsch beantwortet, bitte probieren Sie es noch einmal:**

Betrachten Sie diese Spielsituation:

*[ImageComprehensionSituation]*

*[display incorrectly answered questions again]*

Page Break

---

*[only display if at least one answer wrong after retry]*

**Sie haben die folgende(n) Frage(n) falsch beantwortet, bitte nehmen Sie die richtige(n) Antwort(en) zur Kenntnis.**

Das war die Spielsituation:

*[ImageComprehensionSituation]*

*[only display question answered incorrectly after retry]*

Nehmen Sie an, **Schwarz** ist dran. Was passiert, wenn **Schwarz** eine 6 würfelt?

- Schwarz zieht sechs Felder und würfelt nochmal.**
- Schwarz zieht sechs Felder und Grün ist dran.
- Schwarz macht nichts und Grün ist dran.

Nehmen Sie stattdessen an, **Grün** ist dran. Was passiert, wenn **Grün** eine 2 würfelt?

- Grün kann nicht ziehen und Schwarz ist dran.
- Grün schlägt Schwarz, d.h. Grün zieht auf das Feld, auf dem Schwarz ist, und Schwarz muss zurück in den Startblock.
- Grün zieht zwei Felder, wobei das Feld mit Schwarz nicht mitzählt.

Nehmen Sie an, **Grün** würfelt in der Tat eine 2 und anschließend ist **Schwarz** dran. Was passiert dann, wenn **Schwarz** dran ist?

- Schwarz würfelt solange bis eine 6 gewürfelt wird. Dann ist Grün dran.
- Schwarz würfelt ein Mal. Falls Schwarz eine 6 würfelt, stellt Schwarz seine Figur aufs Startfeld und zieht sechs Felder.
- Schwarz würfelt höchstens drei Mal, um eine 6 zu würfeln. Falls das gelingt, stellt Schwarz seine Figur aufs Startfeld, würfelt nochmal und bewegt die Figur entsprechend dem zweiten Würfelwurf.

Page Break

---

*[display if all answers correct]*

Sie haben alle Fragen richtig beantwortet.

Page Break

---

Ihnen werden nun weitere Verständnisfragen gestellt:

Aus welchen Teilen besteht Ihre Auszahlung?

- Ich erhalte in jedem Fall 12 €. Ansonsten erhalte ich nichts.
- Ich erhalte in jedem Fall 12 € und außerdem erhalte ich potentiell einen Bonus über 5 € abhängig von meinen Antworten.

Im Laufe des Experiments müssen Sie mehrmals Antworten am Computer eingeben, von denen manche relevant sein können für den Bonus-Teil Ihrer Auszahlung. Falls Sie

zufällig ausgewählt werden, am Ende wirklich *Mensch ärgere dich nicht* zu spielen, wie bestimmt sich aus diesen potentiell relevanten Antworten Ihr Bonus?

- Es ist immer die durchschnittliche Antwort relevant.
- Aus allen für die Auszahlung potentiell relevanten Antworten wird genau eine zufällig ausgewählt, die tatsächlich für den Bonus relevant ist.
- Von allen Antworten ist immer die größte relevant.

Page Break

---

*[only display if at least one answer wrong]*

**Sie haben die folgende(n) Frage(n) falsch beantwortet, bitte probieren Sie es noch einmal:**

*[display incorrectly answered questions again]*

Page Break

---

*[only display if at least one answer wrong after retry]*

**Sie haben die folgende(n) Frage(n) falsch beantwortet, bitte nehmen Sie die richtige(n) Antwort(en) zur Kenntnis.**

*[only display question answered incorrectly after retry]*

Hier sehen Sie die richtigen Antworten in **grün**:

Aus welchen Teilen besteht Ihre Auszahlung?

- Ich erhalte in jedem Fall 12 €. Ansonsten erhalte ich nichts.
- Ich erhalte in jedem Fall 12 € und außerdem erhalte ich potentiell einen Bonus über 5 € abhängig von meinen Antworten.

Im Laufe des Experiments müssen Sie mehrmals Antworten am Computer eingeben, von denen manche relevant sein können für den Bonus-Teil Ihrer Auszahlung. Falls Sie

zufällig ausgewählt werden, am Ende wirklich *Mensch ärgere dich nicht* zu spielen, wie bestimmt sich aus diesen potentiell relevanten Antworten Ihr Bonus?

- Es ist immer die durchschnittliche Antwort relevant.
- Aus allen für die Auszahlung potentiell relevanten Antworten wird genau eine zufällig ausgewählt, die tatsächlich für den Bonus relevant ist.
- Von allen Antworten ist immer die größte relevant.

Page Break

---

*[display if all answers correct]*

Sie haben alle Fragen richtig beantwortet.

Page Break

---

Das eigentliche Experiment beginnt nun und es werden Ihnen Fragen gestellt.

**Bitte lesen Sie jede Zeile sorgfältig, da sich manchmal nur kleine Details zwischen den Fragen ändern, zum Beispiel ob Sie **Rot** oder **Gelb** sind, und das für Ihre Auszahlung relevant sein kann.**

End of Block: Comprehension

---

Start of Block: BeliefElicitation

*[looped for five situations in random order]*

Stellen Sie sich vor, Sie spielen diese Situation von *Mensch ärgere dich nicht* gegen eine andere Versuchsperson bis zum Ende:

*[ImageGameSituation]*

**Sie sind **Rot** [**Gelb**]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]**

*[Treated, Player1:]*

**Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.**

*[Treated, Player2:]*

**Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.**

*[Control, Player1 and Player2:]*

**Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.**

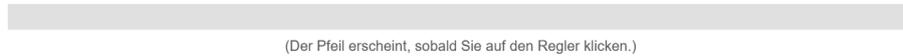
Am Ende des Experiments werden zufällig Versuchspersonen ausgewählt, wirklich *Mensch ärgere dich nicht* zu spielen. Es ist also gut möglich, dass Sie genau diese Situation gleich in genau dieser Rolle spielen werden.

**Was glauben Sie: Wie wahrscheinlich ist es, dass Sie das Spiel gewinnen?**

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den Bonus über 5 € erhalten. Hinsichtlich Ihrer Auszahlung ist es für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie gewinnen.

Bitte geben Sie Ihre Antwort ein, indem Sie auf dem Regler zwischen 0% (ganz links) und 100% (ganz rechts) durch Klicken Ihre Schätzung auswählen. Sie können Ihre Antwort beliebig verändern, bis Sie mit Ihrer Antwort zufrieden sind. Scrollen Sie gern nochmal nach oben, wenn Sie sich ein genaueres Bild der Spielsituation machen wollen.

*[before first click on slider:]*



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

*[after first click on slider:]*



Sie glauben, Sie gewinnen mit 60% Wahrscheinlichkeit.

*[slider could be adjusted as many times as desired with updating subtext]*

Page Break

---

Sie haben angegeben, dass Sie glauben, Sie gewinnen das Spiel beginnend ab dieser Situation mit *[previous answer]* % Wahrscheinlichkeit.

Wie sicher sind Sie sich darüber?



Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituation]*

**Sie sind Rot [Gelb]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]**

*[Treated, Player1:]*

**Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.**

*[Treated, Player2:]*

**Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.**

*[Control, Player1 and Player2:]*

**Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.**

Page Break

---

Zu welchem Grad glauben Sie hängt das Gewinnen des Spiels beginnend ab dieser konkreten Spielsituation vom Glück vs. vom Können ab?



Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituation]*

**Sie sind Rot [Gelb]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]**

*[Treated, Player1:]*

**Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.**

*[Treated, Player2:]*

**Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.**

*[Control, Player1 and Player2:]*

**Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.**

End of Block: BeliefElicitation

---

Start of Block: BeliefElicitationSimple

**Achtung: andere Frage als zuvor.**

Stellen Sie sich vor, Sie spielen diese Situation von *Mensch ärgere dich nicht* gegen eine andere Versuchsperson **für einen Zug**:

*[ImageGameSituationSimpleEnvironment]*

**Sie sind Rot [Gelb]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]**

*[Treated, Player1:]*

**Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.**

[Treated, Player2:]

**Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.**

[Control, Player1 and Player2:]

**Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.**

Am Ende des Experiments werden zufällig Versuchspersonen ausgewählt, wirklich *Mensch ärgere dich nicht* zu spielen. Es ist also gut möglich, dass Sie genau diese Situation gleich in genau dieser Rolle spielen werden.

[Player1:]

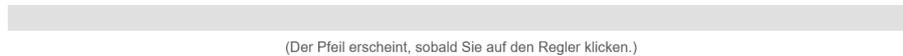
**Was glauben Sie: Wie wahrscheinlich ist es, dass Sie im nächsten Wurf die gelbe Figur schlagen [im nächsten Wurf die rote Figur schlagen]?**

[Player2:]

**Was glauben Sie: Wie wahrscheinlich ist es, dass Ihr Gegenüber im nächsten Wurf Ihre gelbe Figur schlägt [im nächsten Wurf Ihre rote Figur schlägt]?**

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den Bonus über 5 € erhalten. Hinsichtlich Ihrer Auszahlung ist es für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie die gelbe Figur schlagen.

*[before first click on slider:]*



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

*[after first click on slider:]*



Sie glauben, Sie schlagen die gelbe Figur im nächsten Zug mit 60% Wahrscheinlichkeit.

*[slider could be adjusted as many times as desired with updating subtext]*

Page Break

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*[Player1:]*

Sie haben angegeben, dass Sie glauben, Sie schlagen die gelbe [rote] Figur im nächsten Wurf mit *[previous answer]* % Wahrscheinlichkeit.

*[Player2:]*

Sie haben angegeben, dass Sie glauben, Ihr Gegenüber schlägt Ihre rote [gelbe] Figur im nächsten Wurf mit *[previous answer]* % Wahrscheinlichkeit.

Wie sicher sind Sie sich darüber?



Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituationSimpleEnvironment]*

**Sie sind Rot [Gelb]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]**

*[Treated, Player1:]*

**Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.**

*[Treated, Player2:]*

**Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.**

*[Control, Player1 and Player2:]*

**Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.**

Page Break

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*[Player1:]*

Zu welchem Grad glauben Sie hängt es vom Glück vs. vom Können ab, ob Sie im nächsten Zug die gelbe [rote] Figur schlagen?

[Player2:]

Zu welchem Grad glauben Sie hängt es vom Glück vs. vom Können ab, ob Ihr Gegenüber im nächsten Zug Ihre rote [gelbe] Figur schlägt?

1 Nur Glück	2	3	4	5	6	7	8	9	10 Nur Können
----------------	---	---	---	---	---	---	---	---	------------------

Zur Erinnerung, das war die Spielsituation:

[ImageGameSituationSimpleEnvironment]

Sie sind **Rot** [**Gelb**]. Sie sind am Zug. [Ihr Gegenüber ist am Zug.]

[Treated, Player1:]

Sie würfeln mit dem Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.

[Treated, Player2:]

Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfelt mit einem Würfel und Würfelbecher.

[Control, Player1 and Player2:]

Sowohl Ihre als auch die Würfelwürfe Ihres Gegenübers werden jeweils von einem Würfelcomputer zufällig generiert.

End of Block: BeliefElicitationSimple

---

Start of Block: ImportanceWinning

Nehmen Sie an, Sie werden ausgewählt, am Ende gegen eine andere Person *Mensch ärgere dich nicht* zu spielen.

Wie wichtig ist es Ihnen, zu gewinnen?

1 Maximal unwichtig	2	3	4	5	6	7	8	9	10 Maximal wichtig
------------------------	---	---	---	---	---	---	---	---	-----------------------

End of Block: ImportanceWinning

---

Start of Block: AbilityMeasurement

Ihnen werden nun weitere Fragen zu *Mensch ärgere dich nicht* gestellt.

Page Break

---

[looped for three puzzles in random order]

Nehmen Sie an, **Sie sind Rot [Gelb]** und haben eine [AbilityPuzzle1:] 2 [AbilityPuzzle2&3:] 3 gewürfelt.

Welche der beiden Spielfiguren würden Sie bewegen?

1

2

Ihre beiden Spielfiguren sind mit 1 bzw. 2 im Foto beschriftet.

Page Break

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Nehmen Sie an, **Sie sind Rot [Gelb]** und haben eine [AbilityPuzzle1:] 2 [AbilityPuzzle2&3:] 3 gewürfelt.

Sie haben angegeben, dass Sie die mit [previous answer] beschriftete Figur bewegen würden.

Wie sicher sind Sie sich darüber?

1 Sehr unsicher	2	3	4	5	6	7	8	9	10 Sehr sicher
--------------------	---	---	---	---	---	---	---	---	-------------------

End of Block: AbilityMeasurement

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Start of Block: DieRollBet

### Schätz-Spiel

Mit den Versuchspersonen, die am Ende *Mensch ärgere dich nicht* spielen, kann nach dem Spiel ein Schätz-Spiel stattfinden, das entscheiden kann, ob der Bonus gewonnen wird.

Falls Sie ausgewählt werden, *Mensch ärgere dich nicht* am Ende zu spielen und das Schätz-Spiel in der Tat über Ihren Bonus entscheiden wird, funktioniert es so: Separat vom *Mensch ärgere dich nicht*-Spiel würfelt ein Studienleiter sechs Mal mit Ihrem Würfel und Würfelbecher [Würfelcomputer]. Wenn Sie richtig schätzen, wie oft eine 6 gewürfelt wird, erhalten Sie den Bonus.

**Was glauben Sie: Wie oft wird eine 6 gewürfelt, wenn der Studienleiter sechs Mal mit Ihrem Würfel und Würfelbecher [Würfelcomputer] würfelt?**

0	1	2	3	4	5	6
---	---	---	---	---	---	---

Falls Sie zufällig ausgewählt werden, *Mensch ärgere dich nicht* zu spielen, und falls diese Frage dann zufällig ausgewählt wird, für Ihren Bonus relevant zu sein, erhalten Sie den Bonus genau dann, wenn diese Schätzung richtig ist.

End of Block: DieRollBet

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Start of Block: SurveyBlock

Wie gut sind Sie allgemein mit *Mensch ärgere dich nicht* vertraut?

Gar nicht	Ein bisschen	Mittel	Gut	Sehr gut
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Page Break

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Was ist Ihr Geschlecht?

Männlich	Weiblich	Divers / keine Angabe
----------	----------	-----------------------

Page Break

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Wie würden Sie Ihre Religiosität beschreiben?

Nicht religiös
Gewisse Religiosität, aber kein Besuch von Gottesdiensten / keine Interaktion mit Geistlichen
Moderate Religiosität (dann und wann Besuch von Gottesdiensten / Interaktion mit Geistlichen)
Hohe Religiosität (regelmäßiger Gottesdienstbesuch / regelmäßige Interaktion mit Geistlichen)

Page Break

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Es gibt einige Bereiche, in denen Prozesse zunehmend von Maschinen übernommen werden, die zuvor von Menschen gemacht wurden. Laut vieler Stimmen wird sich diese Entwicklung in vielen Lebensbereichen weiter fortsetzen. Ein oft genanntes Beispiel ist, dass künstliche Intelligenz eine immer größere Rolle spielen wird.

Bitte abstrahieren Sie von allgemeinen gesellschaftlichen Auswirkungen dieser Entwicklung, wie zum Beispiel, ob diese Automatisierung bestehende Arbeitsplätze gefährden oder neue kreieren wird, oder ob es einen positiven oder negativen Effekt auf die allgemeine wirtschaftliche Lage haben wird.

Wir interessieren uns hier stattdessen dafür, ob Sie eine solche Entwicklung hinsichtlich Ihrer eigenen Erfahrung positiv oder negativ finden. Mit anderen Worten, finden Sie es positiv oder negativ, wenn Sie als Endnutzer\*in sich in automatisierteren Prozessen wiederfinden?

Es gibt hier kein richtig oder falsch. Wir sind nur an Ihrer Meinung interessiert und es würde uns helfen, wenn Sie ehrlich sind.

Sehr negativ	Negativ	Etwas negativ	Neutral	Etwas positiv	Positiv	Sehr positiv
--------------	---------	---------------	---------	---------------	---------	--------------

Page Break

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Haben Sie an irgendeinem Punkt des Experiments das Gefühl gehabt, Sie könnten bei

den Würfelwürfen betrügen, ohne dass es der Studienleitung auffällt?

Es gibt keinerlei negativen Konsequenzen für Sie, wenn Sie "Ja" antworten.

Seien Sie deswegen bitte ehrlich, es würde uns bei der Auswertung der Studie sehr helfen.

Ja

Nein

Page Break

---

Worum denken Sie ging es in dieser Studie?

Falls Sie eine Idee haben, seien Sie bitte möglichst spezifisch.

Es gibt keine negativen Konsequenzen für Sie, egal was Sie hier antworten. Es geht nur darum die Auswertung der Studie zu verbessern. Seien Sie deswegen bitte ehrlich, es würde uns bei der Auswertung der Studie sehr helfen.

Falls Sie keine Idee haben, ist es aber auch vollkommen in Ordnung, "Keine Ahnung" o.ä. anzugeben.

End of Block: DemographicsPsych

---

Start of Block: Auszahlung

Der erste Teil des Experiments ist nun vorüber.

**Bitte warten Sie an Ihrem Platz, bis alle fertig sind und achten Sie auf Ankündigungen durch die Studienleitung.**

**Klicken Sie nicht mehr weiter.**

Es werden gleich zufällig Versuchspersonen ausgewählt, die eine der von Ihnen gesehenen Spielsituationen tatsächlich zu Ende spielen.

Vielen Dank.

***Observer***

Identical to instructions for *Player 1* and *Player 2*, except for:

[...]

Page Break

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Das heutige Experiment dreht sich um das Brettspiel *Mensch ärgere dich nicht*.

Stellen Sie sich vor, Sie schauen anderen Versuchspersonen dabei zu, wie sie *Mensch ärgere dich nicht* spielen.

Am Ende des Experiments wird zufällig eine Versuchsperson ausgewählt, die tatsächlich anderen Versuchspersonen dabei zusehen wird, wie sie *Mensch ärgere dich nicht* gegeneinander spielen.

Es ist also sehr gut möglich, dass Sie gleich tatsächlich anderen Versuchspersonen beim *Mensch ärgere dich nicht*-Spielen zusehen werden.

Während dieses Spiels zwischen zufällig ausgewählten Versuchspersonen werden das Spielbrett und die Würfelwürfe von oben gefilmt. Auf den Videoaufnahmen sieht man nur den Tisch, d.h. man sieht von den zufällig ausgewählten Versuchspersonen, die das Spiel tatsächlich spielen, nur die Arme.

Hinweis: Ein Betrugsversuch einer der beiden Versuchspersonen, die das Brettspiel spielen, führt zu ihrem Ausschluss aus dem Experiment.

Page Break

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Ihre Auszahlung heute besteht aus zwei Teilen: Sie erhalten sicher 12 €. Falls Sie zufällig ausgewählt werden, anderen Versuchspersonen beim *Mensch ärgere dich nicht*-Spielen zuzuschauen, haben Sie die Möglichkeit, einen Bonus von 5 € zu gewinnen. Aus allen für die Auszahlung potentiell relevanten Antworten wird in diesem Fall genau eine zufällig ausgewählt, die tatsächlich für den Bonus relevant ist. Es ist also am besten für Sie, jede für die Auszahlung potentiell relevante Frage so zu beantworten, als wäre sie wirklich relevant.

Bei allen für Ihre Auszahlung potentiell relevanten Fragen ist es am besten für Sie, wenn Sie ehrlich antworten.

Falls Sie sich dafür interessieren, warum das der Fall ist, gehen Sie bitte am Ende der Studie auf die Studienleiter zu; sie erklären Ihnen gern die exakte Formel, die sicherstellt, dass es für Sie am besten ist, ehrlich zu antworten.

Page Break

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[...]

Im Laufe des Experiments müssen Sie mehrmals Antworten am Computer eingeben, von denen manche relevant sein können für den Bonus-Teil Ihrer Auszahlung. Falls Sie zufällig ausgewählt werden, am Ende wirklich *Mensch ärgere dich nicht* zu spielen, wie bestimmt sich aus diesen potentiell relevanten Antworten Ihr Bonus?

- Es ist immer die durchschnittliche Antwort relevant.
- Aus allen für die Auszahlung potentiell relevanten Antworten wird genau eine zufällig ausgewählt, die tatsächlich für den Bonus relevant ist.
- Von allen Antworten ist immer die größte relevant.

[...]

---

**Start of Block: BeliefElicitation**

*[looped for five situations in random order]*

Stellen Sie sich vor, zwei andere Versuchspersonen spielen diese Situation von *Mensch ärgere dich nicht* bis zum Ende:

*[ImageGameSituation]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

Am Ende des Experiments werden zufällig Versuchspersonen ausgewählt, wirklich *Mensch ärgere dich nicht* zu spielen bzw. bei dem Spiel zuzuschauen. Es ist also gut möglich, dass Sie zwei anderen Versuchspersonen gleich beim Spielen genau dieser Situation zuschauen werden.

**Was glauben Sie: Wie wahrscheinlich ist es, dass Rot das Spiel gewinnt? [Wie wahrscheinlich ist es, dass Rot das Spiel gewinnt?]**

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den Bonus über 5 € erhalten. Hinsichtlich Ihrer Auszahlung ist es für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Rot [Gelb] gewinnt.

Bitte geben Sie Ihre Antwort ein, indem Sie auf dem Regler zwischen 0% (ganz links) und 100% (ganz rechts) durch Klicken Ihre Schätzung auswählen. Sie können Ihre Antwort

beliebig verändern, bis Sie mit Ihrer Antwort zufrieden sind. Scrollen Sie gern nochmal nach oben, wenn Sie sich ein genaueres Bild der Spielsituation machen wollen.

*[before first click on slider:]*



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

*[after first click on slider:]*



Sie glauben, Rot gewinnt mit 60% Wahrscheinlichkeit.

*[slider could be adjusted as many times as desired with updating subtext]*

Page Break

---

Sie haben angegeben, dass Sie glauben, Rot [Gelb] gewinnt das Spiel beginnend ab dieser Situation mit *[previous answer]* % Wahrscheinlichkeit.

Wie sicher sind Sie sich darüber?



Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituation]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

Page Break

---

Zu welchem Grad glauben Sie hängt das Gewinnen des Spiels beginnend ab dieser konkreten Spielsituation vom Glück vs. vom Können ab?



Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituation]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

End of Block: BeliefElicitation

---

Start of Block: BeliefElicitationSimple

**Achtung: andere Frage als zuvor.**

Stellen Sie sich vor, zwei andere Versuchspersonen spielen diese Situation von *Mensch ärgere dich nicht* **für einen Zug**.

*[ImageGameSituationSimpleEnvironment]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

Am Ende des Experiments werden zufällig Versuchspersonen ausgewählt, wirklich *Mensch ärgere dich nicht* zu spielen bzw. bei dem Spiel zuzuschauen. Es ist also gut möglich, dass Sie zwei anderen Versuchspersonen gleich beim Spielen genau dieser Situation zuschauen werden.

**Was glauben Sie: Wie wahrscheinlich ist es, dass Rot [Gelb] im nächsten Wurf die gelbe Figur [rote Figur] schlägt?**

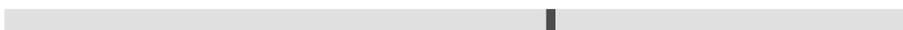
Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den Bonus über 5 € erhalten. Hinsichtlich Ihrer Auszahlung ist es für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Rot [Gelb] im nächsten Wurf die gelbe Figur [rote Figur] schlägt.

*[before first click on slider:]*



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

*[after first click on slider:]*



Sie glauben, Rot schlägt die gelbe Figur im nächsten Zug mit 60% Wahrscheinlichkeit.

*[slider could be adjusted as many times as desired with updating subtext]*

Page Break 

---

Sie haben angegeben, dass Sie glauben, Rot [Gelb] schlägt die gelbe [rote] Figur im nächsten Wurf mit *[previous answer]*% Wahrscheinlichkeit.

Wie sicher sind Sie sich darüber?

1 Sehr unsicher	2	3	4	5	6	7	8	9	10 Sehr sicher
--------------------	---	---	---	---	---	---	---	---	-------------------

Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituationSimpleEnvironment]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

Page Break 

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Zu welchem Grad glauben Sie hängt es vom Glück vs. vom Können ab, ob Rot [Gelb] im nächsten Zug die gelbe [rote] Figur schlägt?

1 Nur Glück	2	3	4	5	6	7	8	9	10 Nur Können
----------------	---	---	---	---	---	---	---	---	------------------

Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituationSimpleEnvironment]*

**Rot [Gelb] ist am Zug.**

*[Treated:]*

**Rot [Gelb] würfelt mit einem Würfel und Würfelbecher. Die Würfelwürfe von Gelb [Rot] werden zufällig von einem Würfelcomputer generiert.**

*[Control:]*

**Sowohl die Würfelwürfe von Rot [Gelb] als auch die von Gelb [Rot] werden jeweils zufällig von einem Würfelcomputer generiert.**

End of Block: BeliefElicitationSimple 

---

[no ,ImportanceWinning' block]

[...]

Page Break

---

Haben Sie an irgendeinem Punkt des Experiments das Gefühl gehabt, die Versuchspersonen, die tatsächlich *Mensch ärgere dich nicht* spielen werden, könnten bei den Würfelwürfen betrügen, ohne dass es der Studienleitung auffällt?

Es gibt keinerlei negativen Konsequenzen für Sie, wenn Sie "Ja" antworten.

Seien Sie deswegen bitte ehrlich, es würde uns bei der Auswertung der Studie sehr helfen.

Ja

Nein

**English (Translation)**

*Player 1 and Player 2*

Start of Block: Intro

**Hello.**

Please wait until you are instructed to start.

Page Break

---

Today's experiment revolves around the board game *Ludo*.

Imagine you are playing *Ludo* against another subject.

At the end of the experiment, subjects are randomly selected to actually play *Ludo* against each other.

It is therefore very possible that you will actually play *Ludo* against another subject.

During this game between randomly selected subjects, the game board and the dice rolls are filmed from above. Only the table can be seen on the livestream, i.e. only the arms of the randomly selected test subjects who are actually playing the game can be seen.

Note: Any attempt to cheat during the board game will result in exclusion from the experiment. To prevent cheating, participants must visibly shake the dice cup before rolling the die.

Page Break

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Your payout today consists of two parts: You are sure to receive €12. If you are randomly selected to play *Ludo* against another subject, you have the chance to win a bonus of 5 €. In this case, out of all the answers potentially relevant for the payout, exactly one will be randomly selected that is actually relevant for the bonus. It is therefore best for you to answer every question that is potentially relevant for the payout as if it were really relevant.

For all questions potentially relevant to your payout, it is best for you to answer honestly.

If you are interested in why this is the case, please approach the study leaders at the end of the study; they will be happy to explain the exact formula that will ensure that it is best for you to answer honestly.

Page Break

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Before continuing, please familiarize yourself with the rules of *Ludo*, which are printed out in front of you.

**Read the rules carefully, even if you think you already know them very well, as there are many slightly different variations.**

**You will have to answer comprehension questions about the rules during the study.**

The rules will stay with you all the time in printed form.

You can continue in: *[counter ticking down from 90 seconds]*

End of Block: Intro

---

Start of Block: Comprehension

You will now be asked comprehension questions. Consider this game situation:

*[ImageComprehensionSituation]*

Suppose it is **Black's** turn. What happens if **Black** rolls a 6?

- Black moves six squares and rolls the die again.
- Black moves six squares and it is Green's turn.
- Black does nothing and it is Green's turn.

Assume instead that it is **Green's** turn. What happens if **Green** rolls a 2?

- Green cannot move and it is Black's turn.
- Green takes the black piece, i.e. Green moves to the square where Black is and Black has to go back to the starting block.
- Green moves two squares, whereby the square with black does not count.

Suppose **Green** does indeed roll a 2 and then it is **Black's** turn. What happens when it is **Black's** turn?

- Black rolls the die until a 6 is rolled. Then it is Green's turn.
- Black rolls the dice once. If Black rolls a 6, Black places his piece on the starting square and moves six squares.
- Black rolls the die a maximum of three times to roll a 6. If this succeeds, Black places his piece on the starting square, rolls the dice again and moves the piece according to the second roll of the dice.

Page Break

---

*[only display if at least one answer wrong]*

**You have answered the following question(s) incorrectly, please try again:**

Consider this game situation:

*[ImageComprehensionSituation]*

*[display incorrectly answered questions again]*

Page Break

---

*[only display if at least one answer wrong after retry]*

**You have answered the following question(s) incorrectly, please note the correct answer(s).**

This was the game situation:

*[ImageComprehensionSituation]*

*[only display question answered incorrectly after retry]*

Suppose it is **Black's** turn. What happens if **Black** rolls a 6?

- Black moves six squares and rolls the die again.**
- Black moves six squares and it is Green's turn.
- Black does nothing and it is Green's turn.

Assume instead that it is **Green**'s turn. What happens if **Green** rolls a 2?

- Green cannot move and it is Black's turn.
- Green takes the black piece, i.e. Green moves to the square where Black is and Black has to go back to the starting block.**
- Green moves two squares, whereby the square with black does not count.

Suppose **Green** does indeed roll a 2 and then it is **Black**'s turn. What happens when it is **Black**'s turn?

- Black rolls the die until a 6 is rolled. Then it is Green's turn.
- Black rolls the dice once. If Black rolls a 6, Black places his piece on the starting square and moves six squares.
- Black rolls the die a maximum of three times to roll a 6. If this succeeds, Black places his piece on the starting square, rolls the dice again and moves the piece according to the second roll of the dice.**

Page Break

---

*[display if all answers correct]*

You have answered all questions correctly.

Page Break

---

You will now be asked further comprehension questions:

What parts does your payout consist of?

- I will receive 12 € in any case. Otherwise I will receive nothing.
- I will receive 12 € in any case and I will also potentially receive a bonus of 5 € depending on my answers.

During the experiment, you will have to enter answers on the computer several times, some of which may be relevant for the bonus part of your payout. If you are randomly

selected to actually play *Ludo*, how are these potentially relevant answers used to determine your bonus?

- The average answer is always relevant.
- From all potentially relevant answers for the payout, exactly one is randomly selected that is actually relevant for the bonus.
- Of all the answers, the largest is always relevant.

Page Break

---

*[only display if at least one answer wrong]*

**You have answered the following question(s) incorrectly, please try again:**

Consider this game situation:

*[ImageComprehensionSituation]*

*[display incorrectly answered questions again]*

Page Break

---

*[only display if at least one answer wrong after retry]*

**You have answered the following question(s) incorrectly, please note the correct answer(s).**

Here you can see the correct answers in **green**:

*[only display question answered incorrectly after retry]*

What parts does your payout consist of?

- I will receive 12 € in any case. Otherwise I will receive nothing.
- I will receive 12 € in any case and I will also potentially receive a bonus of 5 € depending on my answers.

During the experiment, you will have to enter answers on the computer several times, some of which may be relevant for the bonus part of your payout. If you are randomly

selected to actually play *Ludo*, how are these potentially relevant answers used to determine your bonus?

- The average answer is always relevant.
- From all potentially relevant answers for the payout, exactly one is randomly selected that is actually relevant for the bonus.
- Of all the answers, the largest is always relevant.

Page Break

---

*[display if all answers correct]*

You have answered all questions correctly.

Page Break

---

The actual experiment starts now and you will be asked questions.

**Please read each line carefully, as sometimes only small details change between questions, for example whether you are Red or Yellow, and this can be relevant to your payout.**

End of Block: Comprehension

---

Start of Block: BeliefElicitation

*[looped for five situations in random order]*

Imagine you play this situation of *Ludo* against another test person to the end:

*[ImageGameSituation]*

You are **Red** [**Yellow**]. It is your turn. [It is your opponent's turn.]

[Treated, Player1:]

**You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.**

[Treated, Player2:]

**Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.**

[Control, Player1 and Player2:]

**Both your dice rolls and those of your opponent are randomly generated by a dice computer.**

At the end of the experiment, subjects are randomly selected to play *Ludo*. It is therefore very possible that you will play exactly this situation in exactly this role.

**What do you think: How likely is it that you will win the game?**

Your answer to this question can be selected to determine whether you receive the 5 € bonus. Regarding your payout, it is best for you to honestly state how likely you think you are to win.

Please enter your answer by clicking on the slider between 0% (far left) and 100% (far right) to select your estimate. You can change your answer as you wish until you are satisfied with your answer. Feel free to scroll up again if you want to get a more detailed picture of the game situation.

[before first click on slider:]

(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)  
(The arrow appears as soon as you click on the slider.)

[after first click on slider:]

Sie glauben, Sie gewinnen mit 60% Wahrscheinlichkeit.  
You think you have a 60% probability of winning.

[slider could be adjusted as many times as desired with updating subtext]

Page Break

You have stated that you believe you have a [previous answer] % probability of winning the game starting from this situation.

How certain are you about that?

1    2    3    4    5    6    7    8    9    10  
Sehr unsicher    Sehr sicher

Very uncertain

Very certain

As a reminder, this was the game situation:

[ImageGameSituation]

You are **Red** [**Yellow**]. It is your turn. [It is your opponent's turn.]

*[Treated, Player1:]*

**You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.**

*[Treated, Player2:]*

**Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.**

*[Control, Player1 and Player2:]*

**Both your dice rolls and those of your opponent are randomly generated by a dice computer.**

Page Break

---

To what degree do you think winning the game depends on luck vs. skill, starting from this specific game situation?

1 Nur Glück	2	3	4	5	6	7	8	9	10 Nur Können
Just luck									Just skill

Zur Erinnerung, das war die Spielsituation:

*[ImageGameSituation]*

You are **Red** [**Yellow**]. It is your turn. [It is your opponent's turn.]

*[Treated, Player1:]*

**You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.**

*[Treated, Player2:]*

**Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.**

*[Control, Player1 and Player2:]*

**Both your dice rolls and those of your opponent are randomly generated by a dice computer.**

End of Block: BeliefElicitation

---

Start of Block: BeliefElicitationSimple

**Attention: different question than before.**

Imagine you are playing this situation of *Ludo* against another subject **for one turn**:

*[ImageGameSituationSimpleEnvironment]*

You are Red [Yellow]. It is your turn. [It is your opponent's turn.]

[Treated, Player1:]

You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.

[Treated, Player2:]

Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.

[Control, Player1 and Player2:]

Both your dice rolls and those of your opponent are randomly generated by a dice computer.

At the end of the experiment, subjects are randomly selected to play *Ludo*. It is therefore very possible that you will play exactly this situation in exactly this role.

[Player1:]

What do you think: How likely is it that you will take the yellow piece on the next roll [take the red piece on the next roll]?

[Player2:]

What do you think: How likely is it that your opponent will take your yellow piece on the next roll [will take your red piece on the next roll]?

Your answer to this question can be selected to determine whether you receive the 5 € bonus. Regarding your payout, it is best for you to honestly state how likely you think you are to win.

[before first click on slider:]



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

(The arrow appears as soon as you click on the slider.)

[after first click on slider:]



Sie glauben, Sie schlagen die gelbe Figur im nächsten Zug mit 60% Wahrscheinlichkeit.

You think you have a 60% probability of taking the yellow [red] piece on your next move.

[slider could be adjusted as many times as desired with updating subtext]

Page Break

---

[Player1:]

You have stated that you believe you will beat the yellow [red] piece in the next roll with [previous answer] % probability.

[Player2:]

You have stated that you believe your opponent will beat your red [yellow] piece in the next roll with [previous answer] % probability.

How certain are you about that?



As a reminder, this was the game situation:

*[ImageGameSituationSimpleEnvironment]*

**You are Red [Yellow]. It is your turn. [It is your opponent's turn.]**

*[Treated, Player1:]*

**You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.**

*[Treated, Player2:]*

**Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.**

*[Control, Player1 and Player2:]*

**Both your dice rolls and those of your opponent are randomly generated by a dice computer.**

Page Break

---

*[Player1:]*

To what degree do you think it depends on luck vs. skill whether you take the yellow [red] piece on your next move?

*[Player2:]*

To what degree do you think it depends on luck vs. skill whether your opponent captures your red [yellow] piece on the next move?



As a reminder, this was the game situation:

*[ImageGameSituationSimpleEnvironment]*

**You are Red [Yellow]. It is your turn. [It is your opponent's turn.]**

*[Treated, Player1:]*

**You roll the die with the dice cup. Your opponent's dice rolls are randomly generated by a dice computer.**

*[Treated, Player2:]*

**Your dice rolls are randomly generated by a dice computer. Your opponent rolls with a die and dice cup.**

*[Control, Player1 and Player2:]*

**Both your dice rolls and those of your opponent are randomly generated by a dice computer.**

End of Block: BeliefElicitationSimple

---

Start of Block: ImportanceWinning

Suppose you are randomly selected to play *Ludo* against another person.

How important is it to you to win?

1 Maximal unwichtig	2	3	4	5	6	7	8	9	10 Maximal wichtig
---------------------------	---	---	---	---	---	---	---	---	--------------------------

Maximally unimportant Maximally important

End of Block: ImportanceWinning

---

Start of Block: AbilityMeasurement

You will now be asked more questions about *Ludo*.

Page Break 

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*[looped for three puzzles in random order]*

Suppose **you are Red [Yellow]** and have rolled a [AbilityPuzzle1:] **2** [AbilityPuzzle2&3:] **3**.

Which of the two pieces would you move?

- 1
- 2

Your two pieces are labeled 1 and 2 in the photo.

Page Break 

---

Suppose **you are Red [Yellow]** and have rolled a [AbilityPuzzle1:] **2** [AbilityPuzzle2&3:] **3**.

You have stated that you would move the figure labeled *[previous answer]*.

How certain are you about that?

1 Sehr unsicher	2	3	4	5	6	7	8	9	10 Sehr sicher
--------------------	---	---	---	---	---	---	---	---	-------------------

Very uncertain Very certain

End of Block: AbilityMeasurement

---

Start of Block: DieRollBet

**Guessing Game**

With the subjects who end up playing Ludo, a guessing game can take place after the game to decide whether the bonus is won.

If you are selected to play *Ludo* at the end and the guessing game will indeed decide your bonus, it works like this: Separate from the *Ludo* game, a study director rolls the die six times with your dice and dice cup [die roll computer]. If you guess correctly how often a 6 will be rolled, you will receive the bonus.

**How often do you think a 6 will be rolled if the study leader rolls your die with the dice cup [dice computer] six times?**

0	1	2	3	4	5	6
---	---	---	---	---	---	---

If you are randomly selected to play *Ludo*, and if this question is then randomly selected to be relevant to your bonus, you will receive the bonus exactly when this guess is correct.

End of Block: DieRollBet

---

Start of Block: SurveyBlock

How familiar are you with *Ludo* in general?

Gar nicht	Ein bisschen	Mittel	Gut	Sehr gut
Not at all	A little	Medium	Good	Very good

Page Break

---

What is your gender?

Männlich	Weiblich	Divers / keine Angabe
Male	Weiblich	Divers / no answer

Page Break

---

How would you describe your religiosity?

Nicht religiös
Gewisse Religiosität, aber kein Besuch von Gottesdiensten / keine Interaktion mit Geistlichen
Moderate Religiosität (dann und wann Besuch von Gottesdiensten / Interaktion mit Geistlichen)
Hohe Religiosität (regelmäßiger Gottesdienstbesuch / regelmäßige Interaktion mit Geistlichen)

Not religious;  
Somewhat religious, but no attendance of church services / no interaction with clergy;  
Moderate religiosity, occasionally attending church services / interacting with clergy;  
High level of religiosity, regular attendance at church services / regular interaction with clergy

Page Break

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There are some areas in which processes that were previously carried out by humans are increasingly being taken over by machines. According to many voices, this development will continue in many areas of life. One frequently cited example is that artificial intelligence will play an increasingly important role.

Please abstract from the general social impact of this development, such as whether this automation will jeopardize existing jobs or create new ones, or whether it will have a positive or negative effect on the general economic situation.

Instead, we are interested in whether you find such a development positive or negative in terms of your own experience. In other words, do you find it positive or negative if you as an end user find yourself in more automated processes?

There is no right or wrong here. We are only interested in your opinion and it would help us if you are honest.

Sehr negativ	Negativ	Etwas negativ	Neutral	Etwas positiv	Positiv	Sehr positiv
Very negative	Negative	Somewhat negative	Neutral	Somewhat positive	Positive	Very positive

Page Break

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At any point during the experiment, did you have the feeling that you could cheat on the dice rolls without the study management noticing?

There are no negative consequences for you if you answer "Yes."

Please be honest about this, it would help us a lot in the evaluation of the study.

Yes

No

Page Break

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What do you think this study was about?

If you have an idea, please be as specific as possible.

There are no negative consequences for you, no matter what you answer here. It is only about improving the evaluation of the study. So please be honest, it would help us a lot in the evaluation of the study.

If you have no idea, it is also perfectly fine to state "I don't know" or similar.

End of Block: DemographicsPsych

---

Start of Block: Auszahlung

The first part of the experiment is now over.

**Please wait at your seat until everyone has finished and watch out for announcements from the study leader.**

**Do not click any further.**

Soon participants will be randomly selected to actually play one of the game situations you have seen.

Thank you very much.

***Observer***

Identical to instructions for *Player 1* and *Player 2*, except for:

[...]

Page Break

---

Today's experiment revolves around the board game *Ludo*.

Imagine you are watching other subjects playing *Ludo*.

At the end of the experiment, a subject is randomly selected to actually watch other subjects play *Ludo* against each other.

It is therefore very possible that you will actually watch other test subjects playing *Ludo*.

During this game between randomly selected test subjects, the game board and the dice rolls are filmed from above. Only the table can be seen on the video recordings, i.e. only the arms of the randomly selected test subjects who are actually playing the game can be seen.

Note: An attempt to cheat by one of the two test subjects playing the board game leads to their exclusion from the experiment.

Page Break

---

Your payout today consists of two parts: You are sure to receive €12. If you are randomly selected to watch other test subjects play *Ludo*, you have the chance to win a bonus of €5. In this case, out of all the answers potentially relevant for the payout, exactly one will be randomly selected that is actually relevant for the bonus. It is therefore best for you to answer every question that is potentially relevant for the payout as if it were really relevant.

For all questions potentially relevant to your payout, it is best for you to answer honestly.

If you are interested in why this is the case, please approach the study leaders at the end of the study; they will be happy to explain the exact formula that will ensure that it is best for you to answer honestly.

Page Break

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[...]

During the experiment, you will have to enter answers on the computer several times, some of which may be relevant to the bonus part of your payout. If you are randomly selected to actually end up playing *Ludo*, how are these potentially relevant answers used to determine your bonus?

- The average answer is always relevant.
- From all potentially relevant answers for the payout, exactly one is randomly selected that is actually relevant for the bonus.
- Of all the answers, the largest is always relevant.

[...]

---

**Start of Block: BeliefElicitation**

*[looped for five situations in random order]*

Imagine two other test subjects playing this situation of *Ludo* to the end:

*[ImageGameSituation]*

**Red [Yellow] has the move.**

*[Treated:]*

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

*[Control:]*

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

At the end of the experiment, test subjects are randomly selected to play or watch the game of *Ludo*. It is therefore quite possible that you will watch two other subjects playing exactly the same situation.

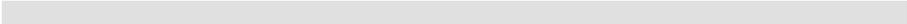
**What do you think: How likely is it that Red will win the game? [How likely is it that red will win the game?]**

Your answer to this question can be selected to determine whether you receive the €5 bonus. With regard to your payout, it is best for you to honestly state how likely you think it is that Red [Yellow] will win.

Please enter your answer by clicking on the slider between 0% (far left) and 100% (far right) to select your estimate. You can change your answer as you wish until you are satisfied with your answer.

Feel free to scroll up again if you want to get a more detailed picture of the game situation.

*[before first click on slider:]*



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

(The arrow appears as soon as you click on the slider.)

[after first click on slider:]



[slider could be adjusted as many times as desired with updating subtext]

Page Break

---

You have stated that you believe Red [Yellow] will win the game with [previous answer] % probability starting from this situation.

How certain are you about this?



As a reminder, that was the game situation:

[ImageGameSituation]

**Red [Yellow] has the move.**

[Treated:]

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

[Control:]

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

Page Break

---

To what degree do you think winning the game depends on luck vs. skill, starting from this specific game situation?



Zur Erinnerung, das war die Spielsituation:

[ImageGameSituation]

As a reminder, that was the game situation:

[ImageGameSituation]

**Red [Yellow] has the move.**

[Treated:]

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

[Control:]

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

End of Block: BeliefElicitation

---

Start of Block: BeliefElicitationSimple

**Attention: different question than before.**

Imagine that two other test subjects play this situation of *Ludo* **for one turn:**

[ImageGameSituationSimpleEnvironment]

**Red [Yellow] has the move.**

[Treated:]

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

[Control:]

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

At the end of the experiment, subjects are randomly selected to play or watch the game of *Ludo*. It is therefore quite possible that you will watch two other subjects playing exactly the same situation.

**What do you think: How likely is it that Red [Yellow] will take the yellow piece [red piece] in the next roll?**

Your answer to this question can be selected to determine whether you receive the €5 bonus. With regard to your payout, it is best for you to honestly state how likely you think it is that Red [Yellow] will win.

[before first click on slider:]



(Der Pfeil erscheint, sobald Sie auf den Regler klicken.)

(The arrow appears as soon as you click on the slider.)

[after first click on slider:]



Sie glauben, Rot schlägt die gelbe Figur im nächsten Zug mit 60% Wahrscheinlichkeit.

You believe that Red [Yellow] will take the yellow piece [red piece] with 60% probability.

[slider could be adjusted as many times as desired with updating subtext]

You have stated that you believe Red [Yellow] will take the yellow piece [red piece] in the next roll with *[previous answer]* % probability.

How certain are you about this?

1 Sehr unsicher	2	3	4	5	6	7	8	9	10 Sehr sicher
--------------------	---	---	---	---	---	---	---	---	-------------------

Very uncertain Very certain

As a reminder, that was the game situation:

*[ImageGameSituationSimpleEnvironment]*

**Red [Yellow] has the move.**

*[Treated:]*

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

*[Control:]*

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

Page Break

---

To what degree do you think it depends on luck vs. skill whether Red [Yellow] takes the yellow piece [red piece] in the next move?

1 Nur Glück	2	3	4	5	6	7	8	9	10 Nur Können
----------------	---	---	---	---	---	---	---	---	------------------

Just luck Just skill

As a reminder, that was the game situation:

*[ImageGameSituationSimpleEnvironment]*

**Red [Yellow] has the move.**

*[Treated:]*

**Red [Yellow] rolls a die and a dice cup. Yellow [Red]'s dice rolls are randomly generated by a dice computer.**

*[Control:]*

**Both the dice rolls of Red [Yellow] and Yellow [Red] are randomly generated by a dice computer.**

End of Block: BeliefElicitationSimple

---

[no ,ImportanceWinning' block]

[...]

Page Break

---

At any point in the experiment, did you have the feeling that the subjects who were actually going to play *Ludo* could cheat on the dice rolls without the study management noticing?

There are no negative consequences for you if you answer "yes".

Therefore, please be honest, as it would help us a lot in the evaluation of the study.

Yes

No

### **2.B.2 Previous Experiment 2 (Pilot Experiment): Instructions**

Instructions were identical to those of the main experiment, except as noted below.

In short, the differences were:

- (i) a revised description of the incentives in response to Danz, Vesterlund, and Wilson (2022),
- (ii) a shortened survey section, and
- (iii) a correction of a conceptual misphrasing in the die-roll bet.

**Start of Block: Intro**

[...]

Bei allen für Ihre Auszahlung potentiell relevanten Fragen ist es am besten für Sie, wenn Sie ehrlich antworten. Falls Sie sich dafür interessieren, warum das der Fall ist, klicken Sie bitte auf *Details*.

Details (klick mich).

[upon clicking Details:]

Sofern nicht anders angegeben, werden Sie bei den potentiell für Ihre Auszahlung relevanten Fragen gefragt, wie wahrscheinlich Sie ein bestimmtes Ereignis halten.

Abhängig von Ihrer Schätzung  $x$  (in %) und davon, ob das Ereignis tatsächlich eintritt, erhalten Sie den Bonus mit einer gewissen Wahrscheinlichkeit  $p$  (und mit Wahrscheinlichkeit  $100\% - p$  erhalten Sie keinen Bonus).  $p$  hängt folgendermaßen von Ihrer Schätzung  $x$  ab: Falls das Ereignis eintritt, gilt  $p = 100\% - (100\% - x)^2$ , und falls das Ereignis nicht eintritt gilt  $p = 100\% - x^2$ .

Machen Sie sich aber nicht zu viele Gedanken um die mathematischen Details: Sie maximieren die Wahrscheinlichkeit, den Bonus zu erhalten, indem Sie ehrlich sind.

[...]

---

**Start of Block: DieRollBet**

**Schätz-Spiel**

Mit den Versuchspersonen, die am Ende *Mensch ärgere dich nicht* spielen, kann nach dem Spiel ein Schätz-Spiel stattfinden, das entscheiden kann, ob der Bonus gewonnen wird.

Falls Sie ausgewählt werden, *Mensch ärgere dich nicht* am Ende zu spielen und das Schätz-Spiel in der Tat über Ihren Bonus entscheiden wird, funktioniert es so: Separat vom *Mensch ärgere dich nicht*-Spiel würfeln Sie sechs Mal mit Ihrem Würfel und Würfelbecher. Wenn Sie richtig schätzen, wie oft Sie eine 6 würfeln, erhalten Sie den Bonus.

**Was glauben Sie: Wie oft würfeln Sie eine 6, wenn Sie sechs Mal mit Ihrem Würfel und Würfelbecher [Würfelcomputer] würfeln?**

[...]

Welcher politischen Partei fühlen Sie sich am nächsten?

- SPD
- CDU / CSU
- Bündnis 90 / Die Grünen
- FDP
- AfD
- Die Linke
- Keine der genannten
- Möchte nicht antworten

Page Break

---

[...]

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

	Stimme überhaupt nicht zu	Stimme nicht zu	Stimme eher nicht zu	Stimme weder zu noch stimme nicht zu	Stimme eher zu	Stimme zu	Stimme voll und ganz zu
Wie mein Leben verläuft hängt von mir selbst ab.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Im Vergleich mit anderen habe ich nicht das erreicht, was ich verdient habe.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Was man im Leben erreicht, ist in erster Linie eine Frage von Schicksal oder Glück.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Wenn man sich sozial oder politisch engagiert, kann man die sozialen Verhältnisse beeinflussen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Ich mache häufig die Erfahrung, dass andere über mein Leben bestimmen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Erfolg muss man sich hart erarbeiten.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

	Stimme überhaupt nicht zu	Stimme nicht zu	Stimme eher nicht zu	Stimme weder zu noch stimme nicht zu	Stimme eher zu	Stimme zu	Stimme voll und ganz zu
Wenn ich im Leben auf Schwierigkeiten stoße, zweifle ich oft an meinen Fähigkeiten.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Welche Möglichkeiten ich im Leben habe, wird von den sozialen Umständen bestimmt.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Wichtiger als alle Anstrengungen sind die Fähigkeiten, die man mitbringt.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Ich habe wenig Kontrolle über die Dinge, die in meinem Leben passieren.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Ich übernehme gern Verantwortung.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Bei wichtigen Entscheidungen orientiere ich mich oft an dem Verhalten von anderen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Ich habe eine positive Einstellung zu mir selbst.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...

	Stimme überhaupt nicht zu	Stimme nicht zu	Stimme eher nicht zu	Stimme weder zu noch stimme nicht zu	Stimme eher zu	Stimme zu	Stimme voll und ganz zu
... muss man sich anstrengen und fleißig sein.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man andere ausnutzen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man begabt und intelligent sein.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man aus der richtigen Familie stammen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man gute Fachkenntnisse auf seinem Spezialgebiet haben.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man Geld und Vermögen haben.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...

	Stimme überhaupt nicht zu	Stimme nicht zu	Stimme eher nicht zu	Stimme weder zu noch stimme nicht zu	Stimme eher zu	Stimme zu	Stimme voll und ganz zu
... muss man einen möglichst guten Schulabschluss haben.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man rücksichtslos und hart sein.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man Beziehungen zu den richtigen Leuten haben.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man sich auf der 'richtigen' Seite politisch engagieren.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man das 'richtige' Geschlecht haben; Männer haben bessere Aufstiegschancen.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
... muss man dynamisch sein und Initiative haben.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Page Break

[...]

Zum Abschluss, haben Sie sonstige Kommentare?

Oder gibt es etwas, von dem Sie glauben, es wäre nützlich, für die Studienleitung zu wissen bei der Auswertung Ihrer Antworten?

Es gibt keine negativen Konsequenzen für Sie, egal was Sie hier antworten. Es geht nur darum die Auswertung der Studie zu verbessern.

Falls Sie keine Kommentare haben, ist es auch vollkommen in Ordnung "Nein" o.ä. zu antworten.

## 2.B.3 Previous Experiment 1 (Thesis): Instructions

### 2.B.3.1 Player

During the instructions (1. – 4.), subjects could freely move back and forth between pages, but once they finished reading the instructions, they could only move forward.

(1) *Instructions, page 1*

**Guten Tag**

Sie sehen neben sich das Brettspiel *Mensch ärgere dich nicht*.

Sie werden dieses Brettspiel gleich gegen eine andere Person spielen.

Das gesamte Experiment dauert **ca. 30 Minuten** und Ihre Auszahlung erhalten

Sie am Ende in bar.

(2) *Instructions, page 2*

**Mensch-ärgere-dich-nicht-Regeln**

Als nächstes lesen Sie bitte die Regeln des Spiels *Mensch ärgere dich nicht* durch, die neben Ihnen ausgedruckt liegen.

Bitte lesen Sie sich die Regeln aufmerksam durch, auch wenn Sie glauben, das Spiel bereits zu kennen.

Die Regeln werde die ganze Zeit über ausgedruckt bei Ihnen bleiben, falls Sie später noch einmal darauf zurückgreifen wollen.

(3) *Instructions, page 3*

**Zusatzinformationen**

Sie werden das Spiel allerdings **nicht** von Anfang an spielen, sondern von der Spielsituation, die Sie auf dem Spielbrett neben sich sehen.

**Sie spielen Rot [Gelb].**

**Sie sind am Zug [Ihr Gegenüber ist am Zug].**

[Condition Treated, player 1:] Sie würfeln mit einem physischen Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.

[Condition Treated, player 2:] Ihre Würfelwürfe werden zufällig von einem Würfelcomputer generiert. Ihr Gegenüber würfelt mit einem physischen Würfel und Würfelbecher.

[Condition Control:] Sowohl Sie als auch Ihr Gegenüber würfeln jeweils mit einem Würfelcomputer, der Ihre Würfelwürfe zufällig generiert.

Das Spielbrett und Ihr Würfelwurf werden von oben gefilmt, während Sie das Spiel spielen.

Auf den Videoaufnahmen sieht man nur den Tisch, d.h. man sieht von Ihnen nur Ihren Arm beim Würfeln und Ziehen der Figuren.

(4) *Instructions, page 4*

**Auszahlungsinformationen**

Ihre Auszahlung besteht aus einer sicheren Teilnahme-Entlohnung sowie zwei

variablen Teilen.

Die sichere Teilnahme-Entlohnung beträgt 5,00 € .

Den ersten variablen Teil erhalten Sie, wenn Sie das Spiel gewinnen. Diese Gewinnprämie beträgt 0,50 € .

Der zweite variable Teil beträgt potentiell 3,00 € :

- Während Sie das Spiel spielen, werden Sie mehrmals aufgefordert, auf diesem Bildschirm Fragen zu beantworten.
- Aus allen Ihren Antworten auf diese Fragen wird nach dem Experiment eine Antwort zufällig ausgewählt. Nur diese Antwort ist relevant für Ihre Auszahlung.
- Da es zufällig ist, welche Antwort ausgewählt wird, ist es für Sie am besten, jede Frage so zu beantworten, als wäre sie für die Auszahlung relevant.

(5) *[WaitPage, next page when all subjects were finished reading the rules]*

**Bitte warten Sie einen Moment.**

(6) *Belief Elicitation Information: Belief1*

**Eingabe**

Ihnen wird nun die erste Frage gestellt.

Das Spiel wird gleich von dem Spielstand starten, den Sie auf dem Brett neben sich sehen. Das Spiel wird dann von Ihnen und einer anderen Person zu Ende gespielt.

[Condition Treated, player 1:] Sie würfeln mit einem physischen Würfel und Würfelbecher. Die Würfelwürfe Ihres Gegenübers werden von einem Würfelcomputer zufällig generiert.

[Condition Treated, player 2:] Ihre Würfelwürfe werden von einem Würfelcomputer zufällig generiert. Ihr Gegenüber würfeln mit einem physischen Würfel und Würfelbecher.

[Condition Control:] Die Würfelwürfe von sowohl Ihnen als auch Ihrem Gegenüber werden jeweils von einem Würfelcomputer zufällig generiert.

**Sie spielen Rot [Gelb].**

**Sie sind am Zug [Ihr Gegenüber ist am Zug].**

**Was ist Ihrer Meinung nach die Wahrscheinlichkeit, dass Sie das Spiel gewinnen?**

Auf der nächsten Seite haben Sie die Möglichkeit, diese Wahrscheinlichkeit an einem Regler einzugeben.

Wenn Sie möchten, gehen Sie gern zum Spielbrett und schauen Sie sich die Spielsituation genauer an, um bei einer besseren Einschätzung anzukommen.

Die Regeln finden Sie zur Referenz neben Ihnen ausgedruckt.

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den variablen Teil Ihrer Auszahlung über 3,00 € erhalten.

Hinsichtlich Ihrer Auszahlung ist für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie gewinnen.

Interessieren Sie sich detaillierter dafür, wie Ihre Antwort für Ihre Auszahlung relevant ist? Dann klicken Sie bitte *hier*.

[When clicked:]

{Am Ende des Experiments wird zufällig eine Ihrer Antworten für Ihre Auszahlung relevant sein.

Da Sie nicht wissen, ob diese Antwort ausgewählt wird, ist es optimal für Sie, diese Frage so zu beantworten, als wäre Sie relevant.

Falls Ihre Antwort auf diese Frage ausgewählt wird, erhalten Sie die zweite variable Auszahlung von 3,00 € mit Wahrscheinlichkeit  $p$  (und mit Wahrscheinlichkeit  $100\% - p$  keine zweite variable Auszahlung). Falls Sie gewinnen, gilt  $p = 100\% - (100\% - x)^2$ , und falls Sie verlieren, gilt  $p = 100\% - x^2$ , wobei  $x$  beide Male die von Ihnen eingegebene Schätzung ist.

Beispiel: Sie geben an, dass Sie mit Wahrscheinlichkeit 35 % gewinnen, und Sie gewinnen. Sie erhalten dann die zweite variable Auszahlung über 3,00 € mit Wahrscheinlichkeit  $p = 100\% - (100\% - 35\%)^2 = 57,75\%$ .

Wenn Sie bereit sind, diese Einschätzung Ihrer Gewinnwahrscheinlichkeit einzugeben, klicken Sie bitte auf *Weiter*.

### (7) *Belief Elicitation Slider: Belief 1*

[Initial view (exemplary for player 1):]

Was ist die Wahrscheinlichkeit, dass Sie das Spiel gewinnen?

**Sie spielen Rot.**

**Sie sind am Zug.**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0% (ganz links) und 100% (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.



[As soon as blue bar was clicked:]

Was ist die Wahrscheinlichkeit, dass Sie das Spiel gewinnen?

**Sie spielen Rot.**

**Sie sind am Zug.**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0% (ganz links) und 100% (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.



[Slider could be readjusted.]

(8) *WaitPage [next page when all subjects submitted belief]*

**Bitte warten Sie einen Moment vor dem Computer.**

Sie erhalten gleich weitere Anweisungen.

(9) *WaitPage [next page when each player has made three turns]*

**Bitte spielen Sie jetzt weiter, bis jeder drei Mal dran war.**

(10) *Belief Elicitation Information: Belief 2*

**Eingabe**

Ihnen wird nun eine weitere Frage gestellt. Das Spiel wird gleich von Ihnen und Ihrem Gegenüber weitergespielt.

Zur Erinnerung: **Sie spielen Rot [Gelb].**

**Sie sind am Zug [Ihr Gegenüber ist am Zug].**

**Was ist Ihrer Meinung nach die Wahrscheinlichkeit, dass Sie das Spiel gewinnen?**

Auf der nächsten Seite haben Sie die Möglichkeit, diese Wahrscheinlichkeit an einem Regler einzugeben.

Wenn Sie möchten, gehen Sie gern zum Spielbrett und schauen Sie sich die Spielsituation genauer an, um bei einer besseren Einschätzung anzukommen.

Die Regeln finden Sie zur Referenz neben Ihnen ausgedruckt.

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den variablen Teil Ihrer Auszahlung über 3,00 € erhalten.

Hinsichtlich Ihrer Auszahlung ist für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie gewinnen.

Interessieren Sie sich detaillierter dafür, wie Ihre Antwort für Ihre Auszahlung relevant ist? Dann klicken Sie bitte *hier*.

[When clicked:]

{Am Ende des Experiments wird zufällig eine Ihrer Antworten für Ihre Auszahlung relevant sein.

Da Sie nicht wissen, ob diese Antwort ausgewählt wird, ist es optimal für Sie, diese Frage so zu beantworten, als wäre Sie relevant.

Falls Ihre Antwort auf diese Frage ausgewählt wird, erhalten Sie die zweite variable Auszahlung von 3,00 € mit Wahrscheinlichkeit  $p$  (und mit Wahrscheinlichkeit  $100\% - p$  keine zweite variable Auszahlung). Falls Sie gewinnen, gilt  $p = 100\% - (100\% - x)^2$ , und falls Sie verlieren, gilt  $p = 100\% - x^2$ , wobei  $x$  beide Male die von Ihnen eingegebene Schätzung ist.

Beispiel: Sie geben an, dass Sie mit Wahrscheinlichkeit 35% gewinnen, und Sie gewinnen. Sie erhalten dann die zweite variable Auszahlung über 3,00 € mit Wahrscheinlichkeit  $p = 100\% - (100\% - 35\%)^2 = 57,75\%$

Wenn Sie bereit sind, diese Einschätzung Ihrer Gewinnwahrscheinlichkeit einzugeben, klicken Sie bitte auf *Weiter*.

- (11) *Belief Elicitation Slider: Belief 2*  
[see 7.]
- (12) *WaitPage [next page when all subjects submitted belief]*  
[see 8.]
- (13) *WaitPage [next page when one player can win with the next die roll]*  
**Bitte spielen Sie jetzt weiter, bis ein Spieler mit dem nächsten Wurf gewinnen kann.**
- (14) *Belief Elicitation Information: Belief 3*  
[see 10.]
- (15) *Belief Elicitation Slider: Belief 3*  
[see 7.]
- (16) *WaitPage [next page when game is over]*  
**Bitte spielen Sie das Spiel jetzt zu Ende.**
- (17) *Post-game Questions: Information*  
**Weiterführende Fragen**  
Das Spiel ist nun vorbei und Rot [Gelb] hat gewonnen.  
[Sie haben sich dadurch bereits eine Gewinnprämie von 0,50 € verdient.]  
Es folgen nun noch weitere Fragen.  
Diese Fragen sind nicht für Ihre Auszahlung relevant, aber bitte beantworten Sie sie trotzdem ehrlich
- (18) *Post-game Questions: BeliefBackward*  
**Weiterführende Fragen**  
Versetzen Sie sich noch einmal in die Situation des Spielanfangs.  
Zur Erinnerung sehen Sie hier den Spielstand nochmal dargestellt:



Sie waren Rot und Sie waren am Zug [Sie waren Gelb und Ihr Gegenüber war am Zug].

[Condition Treated, player 1:] **Angenommen**, Sie würden noch einmal neu von diesem Spielstand starten und Sie und Ihr Gegenüber würfeln erneut (Sie mit einem physischen Würfel und Würfelbecher, Ihr Gegenüber durch den Würfelcomputer).

[Condition Treated, player 2:] **Angenommen**, Sie würden noch einmal neu von diesem Spielstand starten und Sie und Ihr Gegenüber würfeln erneut (Sie durch den Würfelcomputer und Ihr Gegenüber mit einem physischen Würfel und Würfelbecher).

[Condition Control:] **Angenommen**, Sie würden noch einmal neu von diesem Spielstand starten und Sie und Ihr Gegenüber würfeln erneut beide jeweils mit einem Würfelcomputer.

**Wie hoch würden Sie jetzt die Wahrscheinlichkeit einschätzen, dass Sie das Spiel gewinnen?**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0 % (ganz links) und 100 % (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.

Wenn Sie mit Ihrer Eingabe zufrieden sind, klicken Sie bitte auf *Weiter*.

[Slider, same as before]

(19) *Post-game Questions: Demographics, page 1*

**Weiterführende Fragen**

Wie alt sind Sie?

[field]

Was ist Ihr Geschlecht?

[radio buttons:] 'Weiblich', 'Männlich', 'Divers / keine Angabe'

Was beschreibt am besten Ihren Beschäftigungsstatus?

[radio buttons:] 'Arbeit in Vollzeit', 'Arbeit in Teilzeit', 'Arbeitslos / Arbeitssuchend', 'Studierend', 'In einer Ausbildung', 'In der Schule', 'In einem Praktikum', 'Pensioniert', 'Arbeitsunfähig (Behinderung o.ä.)', 'Nicht trifft zu'

(20) *Post-game Questions: Demographics, page 2*

**Weiterführende Fragen**

Was ist Ihr höchster (geplanter) Bildungsabschluss? Falls Sie noch studieren, geben Sie bitte Ihren höchsten geplanten Bildungsabschluss an.

[radio buttons:] 'Grundschule', 'Hauptschule', 'Realschule', 'Gymnasium', 'Bachelor', 'Master', 'Promotion'

Wie viel Geld steht Ihnen in € monatlich ca. zur Verfügung (inklusive Miete, Essen etc., abzüglich Steuern und Abgaben)?

[field]

Was ist Ihre Wohnform?

[radio buttons:] 'Allein', 'Mit Lebenspartner(in)', 'Mit den Eltern', 'In Wohngemeinschaft (nicht mit Geschwistern)', 'Mit Geschwistern', 'Alleinerziehend mit Kind(ern)'

(21) *Post-game Questions: Demographics, page 3*

**Weiterführende Fragen**

Falls Sie studieren bzw. studiert haben, was ist bzw. war Ihr (hauptsächlicher) Fachbereich?

[radio buttons:] 'Ich studiere nicht', 'Agrar- und Forstwissenschaften', 'Ingenieurwissenschaften', 'Naturwissenschaften, Mathematik, Informatik', 'Medizin und Gesundheitswesen', 'Politik- und Gesellschaftswissenschaften', 'Rechtswissenschaften', 'Wirtschaftswissenschaften', 'Sprach- und Kulturwissenschaften', 'Geisteswissenschaften', 'Kunst, Gestaltung, Musik', 'Lehramt und Pädagogik', 'Beamtenstudiengang'

(22) *Post-game Questions: Demographics, page 4*

**Weiterführende Fragen**

Wie würden Sie Ihre Religiosität beschreiben?

[radio buttons:] 'Nicht religiös', 'Gewisse Religiosität, aber kein Besuch von Gottesdiensten/ keine Interaktion mit Geistlichen', 'Moderate Religiosität (dann und wann Besuch von Gottesdiensten, Interaktion mit Geistlichen)', 'Hohe Religiosität (regelmäßiger Gottesdienstbesuch, regelmäßige Interaktion mit Geistlichen etc.)'

Wie viele Kinder haben Sie?

[radio buttons:] '0', '1', '2', '3', '4+'

(23) *Post-game Questions: Demographics, page 5*

**Weiterführende Fragen**

Wie viele Geschwister haben Sie?

[radio buttons:] '0', '1', '2', '3', '4', '5+'

Welcher politischen Partei fühlen Sie sich am nächsten?

[radio buttons:] 'CDU', 'SPD', 'AfD', 'FDP', 'Die Linke', 'Bündnis 90 / Die Grünen', 'CSU', 'Keine der oben genannten', 'Möchte nicht antworten'

(24) *Post-game Questions: Psychology, page 1*

**Weiterführende Fragen**

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

**1:** Stimme überhaupt nicht zu. **2:** Stimme nicht zu. **3:** Stimme eher nicht zu. **4:** Stimme weder zu noch stimme nicht zu. **5:** Stimme eher zu. **6:** Stimme zu. **7:** Stimme voll und ganz zu.

[every statement with horizontal radio buttons labelled '1' - '7']

Wie mein Leben verläuft hängt von mir selbst ab.

Im Vergleich mit anderen habe ich nicht das erreicht, was ich verdient habe.

Was man im Leben erreicht, ist in erster Linie eine Frage von Schicksal oder Glück.

Wenn man sich sozial oder politisch engagiert, kann man die sozialen Verhältnisse beeinflussen.

Ich mache häufig die Erfahrung, dass andere über mein Leben bestimmen.

Erfolg muss man sich hart erarbeiten.

(25) *Post-game Questions: Psychology, page 2*

**Weiterführende Fragen**

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

**1:** Stimme überhaupt nicht zu. **2:** Stimme nicht zu. **3:** Stimme eher nicht zu. **4:** Stimme weder zu noch stimme nicht zu. **5:** Stimme eher zu. **6:** Stimme zu. **7:** Stimme voll und ganz zu.

[every statement with horizontal radio buttons labelled '1' - '7']

Wenn ich im Leben auf Schwierigkeiten stoße, zweifle ich oft an meinen Fähigkeiten.

Welche Möglichkeiten ich im Leben habe, wird von den sozialen Umständen bestimmt.

Wichtiger als alle Anstrengungen sind die Fähigkeiten, die man mitbringt.

Ich habe wenig Kontrolle über die Dinge, die in meinem Leben passieren. Ich übernehme gern Verantwortung.

Bei wichtigen Entscheidungen orientiere ich mich oft an dem Verhalten von anderen.

Ich habe eine positive Einstellung zu mir selbst.

(26) *Post-game Questions: Psychology, page 3*

**Weiterführende Fragen**

**Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...**

[every statement with horizontal radio buttons labelled 'Stimme überhaupt nicht zu', 'Stimme eher nicht zu', 'Stimme eher zu', 'Stimme voll zu']

- ... muss man sich anstrengen und fleißig sein.
- ... muss man andere ausnutzen.
- ... muss man begabt und intelligent sein.
- ... muss man aus der richtigen Familie stammen.
- ... muss man gute Fachkenntnisse auf seinem Spezialgebiet haben.
- ... muss man Geld und Vermögen haben.

(27) *Post-game Questions: Psychology, page 4*

**Weiterführende Fragen**

**Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...**

[every statement with horizontal radio buttons labelled 'Stimme überhaupt nicht zu', 'Stimme eher nicht zu', 'Stimme eher zu', 'Stimme voll zu']

- ... muss man einen möglichst guten Schulabschluss haben.
- ... muss man rücksichtslos und hart sein.
- ... muss man Beziehungen zu den richtigen Leuten haben.
- ... muss man sich auf der richtigen Seite politisch engagieren.
- ... muss man das 'richtige' Geschlecht haben; Männer haben bessere Aufstiegschancen.
- ... muss man dynamisch sein und Initiative haben.

(28) *Post-game Questions: BeliefCounterfactual*

**Weiterführende Fragen**

Versetzen Sie sich noch einmal in die Situation des Spielanfangs. Zur Erinnerung sehen Sie hier den Spielstand nochmal dargestellt:



**Sie waren Rot und Sie waren am Zug [Sie waren Gelb und Ihr Gegenüber am Zug].**

**Angenommen, Sie würden noch einmal neu von diesem Spielstand starten und Sie und Ihr Gegenüber würfeln erneut.**

[Condition Treated, player 1:] **Nehmen Sie außerdem an, Sie würfeln nicht**

mit einem tatsächlichen Würfel und Würfelbecher, sondern die Würfelwürfe sowohl von Ihnen als auch von Ihrem Gegenüber werden durch einen Würfelcomputer generiert.

[Condition Treated, player 2:] Nehmen Sie außerdem an, Ihr Gegenüber würfelt nicht mit einem Würfel und Würfelbecher, sondern die Würfelwürfe sowohl von Ihnen als auch von Ihrem Gegenüber werden durch einen Würfelcomputer generiert.

[Condition Control, player 1:] Nehmen Sie außerdem an, Sie würfeln selbst mit einem tatsächlichen Würfel und einem Würfelbecher, aber die Würfelwürfe Ihres Gegenübers werden weiterhin mit einem Würfelcomputer generiert.

[Condition Control, player 2:] Nehmen Sie außerdem an, Ihre Würfelwürfe werden weiterhin mit einem Würfelcomputer generiert, aber Ihr Gegenüber würfelt mit einem tatsächlichen Würfel und einem Würfelbecher.

**Wie hoch würden Sie jetzt die Wahrscheinlichkeit einschätzen, dass Sie das Spiel gewinnen?**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0 % (ganz links) und 100 % (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.

Wenn Sie mit Ihrer Eingabe zufrieden sind, klicken Sie bitte auf *Weiter*.

[Slider, same as before]

#### (29) Results

##### **Vielen Dank für Ihre Teilnahme**

Hier erhalten Sie nun Ihre Auszahlungsinformationen.

Sie erhalten 5,00 € Teilnahme-Entlohnung.

[Außerdem haben Sie das Spiel gewonnen und erhalten 0,50 € Gewinnprämie.]

Von Ihren Antworten wurde Antwort [1, 2, 3] zufällig ausgewählt, wo Sie angegeben haben, dass Sie glauben, mit Wahrscheinlichkeit [belief] zu gewinnen.

Da Sie [nicht] gewonnen haben, erhalten Sie den zweiten variablen Teil über potentiell 3,00 € mit Wahrscheinlichkeit [value] %.

- Es wurde eine zufällige Zahl zwischen 0 und 100 generiert. Wenn die Zahl kleiner ist als [value], erhalten Sie die Lotterie-Prämie.

- Die zufällige Zahl ist: [rv]. Das heißt, Sie gewinnen den zweiten variablen Teil über 3,00 € [leider nicht]!

**Ihre Gesamtauszahlung beträgt daher: [payoff] € .**

**Bitte bleiben Sie sitzen. Sie werden gleich am Platz ausgezahlt.**

### 2.B.3.2 Observer

During the instructions (1. – 4.), subjects could freely move back and forth between pages, but once they finished reading the instructions, they could only move forward.

(1) *Instructions, page 1*

**Guten Tag**

Auf dem Bildschirm neben Ihnen sehen Sie einen Livestream des Brettspiels *Mensch ärgere dich nicht*.

Dieses Brettspiel wird gleich von zwei Personen gespielt, die wie Sie am Experiment teilnehmen.

Das gesamte Experiment dauert **ca. 30 Minuten** und Ihre Auszahlung erhalten Sie am Ende in bar.

(2) *Instructions, page 2*

**Mensch-ärgere-dich-nicht-Regeln**

Als nächstes lesen Sie bitte die Regeln des Spiels *Mensch ärgere dich nicht* durch, die neben Ihnen ausgedruckt liegen.

Bitte lesen Sie sich die Regeln aufmerksam durch, auch wenn Sie glauben, das Spiel bereits zu kennen.

Die Regeln werde die ganze Zeit über ausgedruckt bei Ihnen bleiben, falls Sie später noch einmal darauf zurückgreifen wollen.

(3) *Instructions, page 3*

**Zusatzinformationen**

Das Spiel wird allerdings **nicht** von Anfang an gespielt, sondern ab dem Spielstand, wie Sie ihn die ganze Zeit schon im Livestream sehen.

Sie werden die ganze Zeit über das Spielbrett und die Würfelwürfe der Spieler im Livestream sehen.

[Condition Treated:] Rot würfelt mit einem physischen Würfel und Würfelbecher.

[Condition Treated:] Die Würfelwürfe von Gelb werden zufällig von einem Würfelcomputer generiert.

[Condition Control:] Sowohl Rot als auch Gelb würfeln jeweils mit einem Würfelcomputer, der die Würfelwürfe zufällig generiert.

(4) *Instructions, page 4*

**Auszahlungsinformationen**

Ihre Auszahlung besteht aus einer sicheren Teilnahme-Entlohnung sowie einem variablen Teil.

Die sichere Teilnahme-Entlohnung beträgt 5,00 € .

Der variable Teil beträgt potentiell 3,00 € :

- Während Sie das Spiel spielen, werden Sie mehrmals aufgefordert, auf diesem Bildschirm Fragen zu beantworten.

- Aus allen Ihren Antworten auf diese Fragen wird nach dem Experiment eine Antwort zufällig ausgewählt. Nur diese Antwort ist relevant für Ihre Auszahlung.
- Da es zufällig ist, welche Antwort ausgewählt wird, ist es für Sie am besten, jede Frage so zu beantworten, als wäre sie für die Auszahlung relevant.

(5) *WaitPage [next page when all subjects were finished reading the rules]*

**Bitte warten Sie einen Moment.**

(6) *Belief Elicitation Information: Belief1*

**Eingabe**

Ihnen wird nun die erste Frage gestellt.

Zur Erinnerung: Das Spiel wird gleich von diesem Spielstand starten, den Sie im Livestream sehen. Das Spiel wird dann von von zwei Personen, die wie Sie am Experiment teilnehmen, zu Ende gespielt.

**Rot ist am Zug.**

[Condition Treated:] Rot würfelt mit einem physischen Würfel und einem Würfelbecher. Die Würfelwürfe von Gelb werden von einem Würfelcomputer zufällig generiert.

Condition Control:] Die Würfelwürfe von sowohl Rot als auch Gelb werden jeweils von einem Würfelcomputer zufällig generiert.

**Was ist Ihrer Meinung nach die Wahrscheinlichkeit, dass Rot [Gelb] das Spiel gewinnt?**

Auf der nächsten Seite haben Sie die Möglichkeit, diese Wahrscheinlichkeit an einem Regler einzugeben.

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den variablen Teil Ihrer Auszahlung über 3,00 € erhalten.

Hinsichtlich Ihrer Auszahlung ist für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie gewinnen.

Interessieren Sie sich detaillierter dafür, wie Ihre Antwort für Ihre Auszahlung relevant ist? Dann klicken Sie bitte *hier*.

[When clicked:]

{Am Ende des Experiments wird zufällig eine Ihrer Antworten für Ihre Auszahlung relevant sein.

Da Sie nicht wissen, ob diese Antwort ausgewählt wird, ist es optimal für Sie, diese Frage so zu beantworten, als wäre Sie relevant.

Falls Ihre Antwort auf diese Frage ausgewählt wird, erhalten Sie die variable Auszahlung von 3,00 € mit Wahrscheinlichkeit  $p$  (und mit Wahrscheinlichkeit  $100\% - p$  keine zweite variable Auszahlung). Falls Rot [Gelb] gewinnt, gilt  $p = 100\% - (100\% - x)^2$ , und falls Rot [Gelb] verliert, gilt  $p = 100\% - x^2$ , wobei  $x$  beide Male die von Ihnen eingegebene Schätzung ist.

Beispiel: Sie geben an, dass Rot [Gelb] mit Wahrscheinlichkeit 35 % gewinnt, und Rot [Gelb] gewinnt. Sie erhalten dann die variable Auszahlung über 3,00

€ mit Wahrscheinlichkeit  $p = 100\% - (100\% - 35\%)^2 = 57,75\%$ .

Wenn Sie bereit sind, diese Einschätzung Ihrer Gewinnwahrscheinlichkeit einzugeben, klicken Sie bitte auf *Weiter*.

(7) *Belief Elicitation Slider: Belief 1*

[Initial view (exemplary for observer 2):]

Was ist die Wahrscheinlichkeit, dass Gelb das Spiel gewinnt?

**Rot ist am Zug.**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0% (ganz links) und 100% (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.



[As soon as blue bar was clicked:]

Was ist die Wahrscheinlichkeit, dass Gelb das Spiel gewinnt?

**Rot ist am Zug.**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0% (ganz links) und 100% (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.



**Weiter**

[Slider could be readjusted.]

(8) *WaitPage [next page when all subjects submitted belief]*

**Das Spiel wird gleich weitergespielt.**

(9) *WaitPage [next page when each player has made three turns]*

(10) *Belief Elicitation Information: Belief 2*

**Eingabe**

Ihnen wird nun eine weitere Frage gestellt. Das Spiel wird gleich weitergespielt.

**Rot [Gelb] ist am Zug.**

[Condition Treated:] Rot würfelt mit einem physischen Würfel und einem Würfelbecher. Die Würfelwürfe von Gelb werden von einem Würfelcomputer zufällig generiert.

[Condition Control:] Die Würfelwürfe von sowohl Rot als auch Gelb werden jeweils von einem Würfelcomputer zufällig generiert.

**Was ist Ihrer Meinung nach die Wahrscheinlichkeit, dass Rot [Gelb] das**

**Spiel gewinnt?**

Auf der nächsten Seite haben Sie die Möglichkeit, diese Wahrscheinlichkeit an einem Regler einzugeben.

Ihre Antwort auf diese Frage kann ausgewählt werden, um zu bestimmen, ob Sie den variablen Teil Ihrer Auszahlung über 3,00 € erhalten.

Hinsichtlich Ihrer Auszahlung ist für Sie am besten, wenn Sie ehrlich angeben, wie hoch Sie die Wahrscheinlichkeit einschätzen, dass Sie gewinnen.

Interessieren Sie sich detaillierter dafür, wie Ihre Antwort für Ihre Auszahlung relevant ist? Dann klicken Sie bitte *hier*.

[When clicked:]

{Am Ende des Experiments wird zufällig eine Ihrer Antworten für Ihre Auszahlung relevant sein.

Da Sie nicht wissen, ob diese Antwort ausgewählt wird, ist es optimal für Sie, diese Frage so zu beantworten, als wäre Sie relevant.

Falls Ihre Antwort auf diese Frage ausgewählt wird, erhalten Sie die variable Auszahlung von 3,00 € mit Wahrscheinlichkeit  $p$  (und mit Wahrscheinlichkeit  $100\% - p$  keine zweite variable Auszahlung). Falls Rot [Gelb] gewinnt, gilt  $p = 100\% - (100\% - x)^2$ , und falls Rot [Gelb] verliert, gilt  $p = 100\% - x^2$ , wobei  $x$  beide Male die von Ihnen eingegebene Schätzung ist.

Beispiel: Sie geben an, dass Rot [Gelb] mit Wahrscheinlichkeit 35 % gewinnt, und Rot [Gelb] gewinnt. Sie erhalten dann die variable Auszahlung mit Wahrscheinlichkeit }

Wenn Sie bereit sind, diese Einschätzung Ihrer Gewinnwahrscheinlichkeit einzugeben, klicken Sie bitte auf *Weiter*.

- (11) *Belief Elicitation Slider: Belief 2*

[see 7.]

- (12) *WaitPage [next page when all subjects submitted belief]*

[see 8.]

- (13) *WaitPage [next page when one player can win with the next die roll]*

**Das Spiel wird gleich weitergespielt.**

- (14) *Belief Elicitation Information: Belief 3*

[see 10.]

- (15) *Belief Elicitation Slider: Belief 3*

[see 7.]

- (16) *WaitPage [next page when game is over]*

**Das Spiel wird gleich weitergespielt.**

- (17) *Post-game Questions: Information*

**Weiterführende Fragen**

Das Spiel ist nun vorbei und Rot [Gelb] hat gewonnen.

Es folgen nun noch weitere Fragen.

Diese Fragen sind nicht für Ihre Auszahlung relevant, aber bitte beantworten Sie sie trotzdem ehrlich

(18) *Post-game Questions: BeliefBackward*

**Weiterführende Fragen**

Versetzen Sie sich noch einmal in die Situation des Spielanfangs.

Zur Erinnerung sehen Sie hier den Spielstand nochmal dargestellt:



Rot [Gelb] war am Zug.

[Condition Treated:] Angenommen, das Spiel würde noch einmal neu von diesem Spielstand starten und es wird erneut gewürfelt (Rot mit einem physischen Würfel und Würfelbecher und Gelb mit einem Würfelcomputer).

[Condition Control:] Angenommen, das Spiel würde noch einmal neu von diesem Spielstand starten und sowohl Rot als auch Gelb würfeln erneut jeweils mit einem Würfelcomputer.

Wie hoch würden Sie jetzt die Wahrscheinlichkeit einschätzen, dass Rot [Gelb] das Spiel gewinnt?

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0 % (ganz links) und 100 % (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.

Wenn Sie mit Ihrer Eingabe zufrieden sind, klicken Sie bitte auf *Weiter*.

[Slider, same as before]

(19) *Post-game Questions: Demographics, page 1*

**Weiterführende Fragen**

Wie alt sind Sie?

[field]

Was ist Ihr Geschlecht?

[radio buttons:] 'Weiblich', 'Männlich', 'Divers / keine Angabe'

Was beschreibt am besten Ihren Beschäftigungsstatus?

[radio buttons:] 'Arbeit in Vollzeit', 'Arbeit in Teilzeit', 'Arbeitslos / Arbeitssuchend', 'Studierend', 'In einer Ausbildung', 'In der Schule', 'In einem Praktikum', 'Pensioniert', 'Arbeitsunfähig (Behinderung o.ä.)', 'Nicht trifft zu'

(20) *Post-game Questions: Demographics, page 2*

**Weiterführende Fragen**

Was ist Ihr höchster (geplanter) Bildungsabschluss? Falls Sie noch studieren, geben Sie bitte Ihren höchsten geplanten Bildungsabschluss an.

[radio buttons:] 'Grundschule', 'Hauptschule', 'Realschule', 'Gymnasium', 'Bachelor', 'Master', 'Promotion'

Wie viel Geld steht Ihnen in € monatlich ca. zur Verfügung (inklusive Miete, Essen etc., abzüglich Steuern und Abgaben)?

[field]

Was ist Ihre Wohnform?

[radio buttons:] 'Allein', 'Mit Lebenspartner(in)', 'Mit den Eltern', 'In Wohngemeinschaft (nicht mit Geschwistern)', 'Mit Geschwistern', 'Alleinerziehend mit Kind(ern)'

(21) *Post-game Questions: Demographics, page 3*

**Weiterführende Fragen**

Falls Sie studieren bzw. studiert haben, was ist bzw. war Ihr (hauptsächlicher) Fachbereich?

[radio buttons:] 'Ich studiere nicht', 'Agrar- und Forstwissenschaften', 'Ingenieurwissenschaften', 'Naturwissenschaften, Mathematik, Informatik', 'Medizin und Gesundheitswesen', 'Politik- und Gesellschaftswissenschaften', 'Rechtswissenschaften', 'Wirtschaftswissenschaften', 'Sprach- und Kulturwissenschaften', 'Geisteswissenschaften', 'Kunst, Gestaltung, Musik', 'Lehramt und Pädagogik', 'Beamtenstudiengang'

(22) *Post-game Questions: Demographics, page 4*

**Weiterführende Fragen**

Wie würden Sie Ihre Religiosität beschreiben?

[radio buttons:] 'Nicht religiös', 'Gewisse Religiosität, aber kein Besuch von Gottesdiensten/ keine Interaktion mit Geistlichen', 'Moderate Religiosität (dann und wann Besuch von Gottesdiensten, Interaktion mit Geistlichen)', 'Hohe Religiosität (regelmäßiger Gottesdienstbesuch, regelmäßige Interaktion mit Geistlichen etc.)'

Wie viele Kinder haben Sie?

[radio buttons:] '0', '1', '2', '3', '4+'

(23) *Post-game Questions: Demographics, page 5*

**Weiterführende Fragen**

Wie viele Geschwister haben Sie?

[radio buttons:] '0', '1', '2', '3', '4', '5+'

Welcher politischen Partei fühlen Sie sich am nächsten?

[radio buttons:] 'CDU', 'SPD', 'AfD', 'FDP', 'Die Linke', 'Bündnis 90 / Die Grünen', 'CSU', 'Keine der oben genannten', 'Möchte nicht antworten'

(24) *Post-game Questions: Psychology, page 1*

**Weiterführende Fragen**

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

**1:** Stimme überhaupt nicht zu. **2:** Stimme nicht zu. **3:** Stimme eher nicht zu. **4:** Stimme weder zu noch stimme nicht zu. **5:** Stimme eher zu. **6:** Stimme zu. **7:** Stimme voll und ganz zu.

[every statement with horizontal radio buttons labelled '1' - '7']

Wie mein Leben verläuft hängt von mir selbst ab.

Im Vergleich mit anderen habe ich nicht das erreicht, was ich verdient habe.

Was man im Leben erreicht, ist in erster Linie eine Frage von Schicksal oder Glück.

Wenn man sich sozial oder politisch engagiert, kann man die sozialen Verhältnisse beeinflussen.

Ich mache häufig die Erfahrung, dass andere über mein Leben bestimmen.

Erfolg muss man sich hart erarbeiten.

(25) *Post-game Questions: Psychology, page 2*

**Weiterführende Fragen**

Wie sehr stimmen Sie jeder der folgenden Aussagen zu?

**1:** Stimme überhaupt nicht zu. **2:** Stimme nicht zu. **3:** Stimme eher nicht zu. **4:** Stimme weder zu noch stimme nicht zu. **5:** Stimme eher zu. **6:** Stimme zu. **7:** Stimme voll und ganz zu.

[every statement with horizontal radio buttons labelled '1' - '7']

Wenn ich im Leben auf Schwierigkeiten stoße, zweifle ich oft an meinen Fähigkeiten.

Welche Möglichkeiten ich im Leben habe, wird von den sozialen Umständen bestimmt.

Wichtiger als alle Anstrengungen sind die Fähigkeiten, die man mitbringt.

Ich habe wenig Kontrolle über die Dinge, die in meinem Leben passieren. Ich übernehme gern Verantwortung.

Bei wichtigen Entscheidungen orientiere ich mich oft an dem Verhalten von anderen.

Ich habe eine positive Einstellung zu mir selbst.

(26) *Post-game Questions: Psychology, page 3*

**Weiterführende Fragen**

**Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...**

[every statement with horizontal radio buttons labelled 'Stimme überhaupt nicht zu', 'Stimme eher nicht zu', 'Stimme eher zu', 'Stimme voll zu']

- ... muss man sich anstrengen und fleißig sein.
- ... muss man andere ausnutzen.
- ... muss man begabt und intelligent sein.
- ... muss man aus der richtigen Familie stammen.
- ... muss man gute Fachkenntnisse auf seinem Spezialgebiet haben.
- ... muss man Geld und Vermögen haben.

(27) *Post-game Questions: Psychology, page 4*

**Weiterführende Fragen**

**Um in Deutschland tatsächlich Erfolg zu haben und sozial aufzusteigen, ...**

[every statement with horizontal radio buttons labelled ‘Stimme überhaupt nicht zu’, ‘Stimme eher nicht zu’, ‘Stimme eher zu’, ‘Stimme voll zu’]

- ... muss man einen möglichst guten Schulabschluss haben.
- ... muss man rücksichtslos und hart sein.
- ... muss man Beziehungen zu den richtigen Leuten haben.
- ... muss man sich auf der richtigen Seite politisch engagieren.
- ... muss man das ‘richtige’ Geschlecht haben; Männer haben bessere Aufstiegschancen.
- ... muss man dynamisch sein und Initiative haben.

(28) *Post-game Questions: BeliefCounterfactual*

**Weiterführende Fragen**

Versetzen Sie sich noch einmal in die Situation des Spielanfangs. Zur Erinnerung sehen Sie hier den Spielstand nochmal dargestellt:



**Rot [Gelb] war am Zug.**

**Angenommen, das Spiel würde noch einmal neu von diesem Spielstand starten und es wird erneut gewürfelt.**

[Condition Treated:] **Nehmen Sie außerdem an, Rot würfelt nicht selbst, sondern die Würfelwürfe sowohl von Rot als auch von Gelb werden jeweils**

**zufällig durch einen Würfelcomputer generiert.**

[Condition Control:] **Nehmen Sie außerdem an, Rot würfelt selbst mit einem tatsächlichen Würfel und einem Würfelbecher, aber die Würfelwürfe von Gelb werden weiterhin mit einem Würfelcomputer generiert.**

**Wie hoch würden Sie jetzt die Wahrscheinlichkeit einschätzen, dass Rot [Gelb] das Spiel gewinnt?**

Bitte geben Sie Ihre Einschätzung dieser Wahrscheinlichkeit ein, indem Sie unten auf dem blauen Regler zwischen 0 % (ganz links) und 100 % (ganz rechts) Ihre Schätzung durch klicken auswählen.

Nachdem Sie geklickt haben, sehen Sie Ihre Eingabe und können den Regler beliebig nachjustieren.

Wenn Sie mit Ihrer Eingabe zufrieden sind, klicken Sie bitte auf *Weiter*.

[Slider, same as before]

(29) *Results*

**Vielen Dank für Ihre Teilnahme**

Hier erhalten Sie nun Ihre Auszahlungsinformationen.

Sie erhalten 5,00 € Teilnahme-Entlohnung.

Von Ihren Antworten wurde Antwort [1, 2, 3] zufällig ausgewählt, wo Sie angegeben haben, dass Sie glauben, mit Wahrscheinlichkeit [belief] zu gewinnen.

Da Rot [Gelb] gewonnen hat, erhalten Sie den zweiten variablen Teil über potentiell 3,00 € mit Wahrscheinlichkeit [value] %.

- Es wurde eine zufällige Zahl zwischen 0 und 100 generiert. Wenn die Zahl kleiner ist als [value], erhalten Sie die Lotterie-Prämie.

- Die zufällige Zahl ist: [rv]. Das heißt, Sie gewinnen den zweiten variablen Teil über 3,00 € [leider nicht]!

**Ihre Gesamtauszahlung beträgt daher: [payoff] € .**

**Bitte bleiben Sie sitzen. Sie werden gleich am Platz ausgezahlt.**

### 2.B.3.3 Verbal Announcements to Subjects

(1) After arriving in the laboratory, players were told:

Willkommen.

Jenseits von Verständnisfragen verzichten Sie bitte auf Kommunikation sowohl mit uns Experimentatoren als auch mit den anderen Teilnehmerinnen und Teilnehmern.

Falls Sie Verständnisfragen haben, machen Sie sich bitte bemerkbar, indem Sie laut 'Ich habe eine Frage', 'Entschuldigung' o.ä. sagen. Es kommt dann jemand zu Ihnen. Stellen Sie die Frage bitte **nicht** laut. Sollte die Antwort auf die Frage für alle relevant sein, werden wir die Antwort natürlich an alle weiterleiten.

Sie werden nun gleich zufällig auf diese vier Räume verteilt.

Anweisungen erhalten Sie an Ihrem Platz über einen Computerbildschirm sowie

unterstützend über Durchsagen von uns. Wir werden an einigen Stellen die Anweisungen des Bildschirms auch noch einmal verbal durchsagen.

Wenn Sie möchten, können Sie Ihre Maske an Ihrem Platz ausziehen. Sie können Sie aber natürlich auch gerne anlassen.

Am Ende des Experiments bleiben Sie bitte sitzen. Es kommt jemand zu Ihnen und zahlt Sie am Platz aus.

Schalten Sie Ihre Handys bitte aus.

Für die beiden Personen in den ersten beiden Räumen: bitte lassen Sie das Brett und die Spielfiguren auf dem Tisch so stehen, wie sie sind.

[Condition: Treated] Auf dem Tisch befindet sich außerdem ein Tablett und ein Würfelbecher mit einem Würfel. Bitte lassen Sie das Tablett beim Bedienen liegen und würfeln Sie neben dem Brett in dem markierten Viereck.

[Condition: Control] Auf dem Tisch befinden sich außerdem zwei Tablets. Bitte lassen Sie die Tablets beim Bedienen liegen.

Bitte ziehen Sie nun hier eine Karte.

- (2) Every time the subjects in the *player* role had to switch from the screen to the board or vice versa, this information – which was also displayed on the screen – was announced verbally by saying (in that order, respectively):
- a. Bitte spielen Sie nun das Spiel, bis jeder drei Mal dran war.
  - b. Bitte kehren Sie nun zu Ihrem Bildschirm zurück.
  - c. Bitte spielen Sie nun das Spiel, bis ein Spieler mit dem nächsten Wurf gewinnen kann.
  - d. Bitte kehren Sie nun zu Ihrem Bildschirm zurück.
  - e. Bitte spielen Sie das Spiel jetzt zu Ende.
  - f. Bitte kehren Sie nun zu Ihrem Bildschirm zurück.

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## Chapter 3

# Parents' Beliefs About Cultural Transmission<sup>\*</sup>

*Joint with Paul Behler*

### 3.1 Introduction

A large literature across the social sciences has established that cultural traits are transmitted from one generation to the next. This has been documented in multiple settings, such as long-term studies that show the persistence of cultural traits in societies (Nunn and Wantchekon, 2011; Alesina, Giuliano, and Nunn, 2013) as well as surveys that reveal correlations between the traits of parents and their children (Duncan et al., 2005; Dohmen et al., 2012).

Despite this growing body of evidence, surprisingly little is known about the mechanisms through which cultural transmission operates. The empirical evidence documenting intergenerational persistence in traits is largely correlational and consistent with multiple explanations: genetic inheritance, similar environmental circumstances, or active transmission. This raises the following questions: Is cultural transmission an active process, deliberately pursued by parents, or a passive by-product of shared environments, peer groups, and institutions? Do parents intentionally seek to transmit certain traits, and if so, how do they think about the transmission process? More specifically, which traits do they want to transmit and which

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methods do they use? What are their beliefs about key parameters of the transmission process, such as the influence of other agents, the effectiveness of active transmission, or the age at which cultural traits are most malleable? Despite the rapid growth of the field of cultural economics in recent decades, these questions remain largely unanswered.

In this paper, we aim to make progress in opening the 'black box' of cultural transmission by directly measuring how parents think about the shaping of attitudes and character traits of their children. To this end, we conducted a large-scale online survey with a sample of 1,200 parents (50% male, 50% female) in the United States.

Our study consists of two main parts. In the first part, we collect novel descriptive data on how parents perceive the process of cultural transmission. We ask questions regarding the transmission of seven economically relevant cultural traits: patience, altruism, work ethic, political attitude, religious faith, universalism, and risk attitude.

Specifically, we elicit parents' beliefs to what extent traits are transmitted, who the key transmitters are, and at what age traits are most malleable. Furthermore, we also ask parents how strongly and in what direction they want to shape each trait, which methods they use and what goal they have in mind when transmitting each trait.<sup>1</sup>

In total, this provides us with a unique dataset that sheds light on an important but understudied aspect of cultural transmission: parents' beliefs and preferences.

In the second part, we empirically assess two key assumptions from theoretical models of cultural transmission (Bisin and Verdier, 2001, 2011). First, we test the assumption of *imperfect empathy*, denoting the idea that while parents care about their children's outcomes, they evaluate these outcomes using their own preferences rather than those of their children. We implement this test by presenting parents with two stylized job options and eliciting both their preferred choice for themselves (in hindsight) and the job they would choose for their child, assuming that the child prefers the other job. This allows us to detect whether parents override their child's preferences.

Second, we test the assumption of *substitutability* which posits that cultural transmission efforts by different agents act as substitutes. To do so, we randomly assign parents to hypothetical scenarios in which schools and peers promote political traits that are either aligned or misaligned with the parents' own preferences, and measure parents' intended transmission effort and extremity in response. This allows us to test whether parents adjust their transmission effort based on the traits that are transmitted by others.

In the descriptive part of our study, we document several novel findings. First, parents overwhelmingly believe that they themselves are the most important trans-

1. To avoid ambiguity, we consistently refer to this process as 'shaping' in the survey, and explicitly exclude any notion of genetic transmission.

mitters of traits, especially for traits like work ethic, altruism, and patience. Schools are also considered important, especially for patience and universalism, while peers and social media are seen as particularly influential for traits such as risk attitude and political orientation.

Second, parents express a strong desire to shape most traits, especially patience, altruism, and work ethic. That is, parents want to shape the traits for which the economic literature has found particularly strong positive effects (Heckman, Stixrud, and Urzua, 2006; Kosse et al., 2020; Sunde et al., 2022). These preferences are also accompanied by relatively high confidence in knowing what the ‘optimal’ level of each trait is, suggesting that parents’ confidence guides their intention to shape specific traits.

Third, parents believe that most traits are shaped very early in childhood. While precise objective benchmarks are rarely available, parents’ beliefs appear broadly consistent with findings from the child development literature that emphasize the importance of early childhood for the development of non-cognitive skills (Heckman, Pinto, and Savelyev, 2013). For political attitudes, however, research in sociology and political science highlights the so-called *impressionable years* – ages 18 to 25 – as especially formative (Sears and Brown, 2023). Parents appear to recognize that political attitudes are shaped somewhat later than other traits, but still place greater importance on the 12–18 age range than on the impressionable years. Thus, even though benchmarks are lacking for many traits, parents’ beliefs about timing are broadly consistent with patterns identified in the literature.

In the second part, we find some – but not overwhelming – support for the imperfect-empathy hypothesis: 60 percent of parents choose the job for their child that they themselves would have wanted, even though this conflicts with their child’s preference. This suggests that many parents evaluate outcomes for their children through the lens of their own preferences – a core assumption in the theoretical literature. Interestingly, imperfect empathy seems to be unrelated to most socio-economic characteristics. Neither gender, education, nor income significantly predicts displaying imperfect empathy, with coefficient point estimates being close to zero.

Finally, we find no evidence for substitutability. When confronted with hypothetical scenarios in which their child’s school and peers promote opposing political traits, parents do not significantly increase their transmission effort or change the extremity of the traits they seek to transmit. If anything, we find that opposing traits being transmitted may even reduce the extremity of the traits parents intend to transmit. This tendency is mostly driven by liberal parents.<sup>2</sup> Potential explanations for this finding could be the desire to adjust to one’s environment (Brown,

2. For liberal parents, we observe a sizable and statistically significant anti-substitutability effect. Among conservative parents, the coefficients are directionally similar but notably smaller in magnitude and not statistically significant. For more details, see Section 3.4.

Lewandowsky, and Huang, 2022) or the intention to avoid causing cognitive dissonance in one's children. Whichever explanation holds, our results challenge a central assumption in cultural transmission models and suggest that the dynamics of parental transmission efforts may be more rigid or less responsive to the social environment than often assumed.<sup>3</sup> At least in the context of political attitudes in the US, parents do not attempt to counteract other transmitters by adjusting their own transmission.

Taken together, our findings provide new empirical insights into how parents think about the transmission of cultural traits to their children. By combining detailed descriptive evidence with theory-driven tests, we contribute to a deeper understanding of the microfoundations of cultural transmission and offer new directions for both empirical and theoretical research in cultural economics.

The remainder of this paper is structured as follows: Section 3.2 reviews the related literature, Section 3.3 outlines the research design, Section 3.4 presents the results, and Section 3.5 concludes.

This study was pre-registered under AEARCTR-0016099.

## 3.2 Literature

In this section we review the various strands of literature to which our study contributes.

Most importantly, we add to the growing literature in cultural economics.

As economists have sought to understand the deep-rooted drivers of economic behavior and long-term development, cultural values have increasingly emerged as fundamental explanatory variables. The field of cultural economics has grown rapidly in recent decades, demonstrating that culture not only co-evolves with institutions and economic outcomes but also serves as an independent force shaping economic trajectories (see, e.g., Nunn, 2012; Valencia Caicedo, 2019). Today, there is a broad consensus that understanding economic behavior and disparities requires a careful study of culture (see Alesina and Giuliano, 2015, for a comprehensive overview).

Given the importance of culture for economic outcomes, it is essential to understand what shapes cultural values. One of the most significant mechanisms in this context is cultural transmission.<sup>4</sup> A large body of research across various contexts

3. There is indeed only very limited empirical evidence for substitutability. An exception is Fouka (2020), which finds that German immigrants to the US were more likely to choose German names (a measure of transmission efforts) when faced with strict assimilation policies in the aftermath of World War I.

4. In some definitions, intergenerational transmission is even considered a defining characteristic of what constitutes a cultural trait (Alesina, Giuliano, and Nunn, 2013).

has documented that cultural values are transmitted from one generation to the next.

On the one hand, numerous studies trace cultural variation back to historical roots, showing its persistence over time. For example, Alesina, Giuliano, and Nunn (2013) demonstrate how ancient agricultural practices continue to shape modern gender norms, while Nunn and Wantchekon (2011) link contemporary mistrust to the legacy of the historical slave trade.<sup>5</sup>

On the other hand, cultural transmission has also been documented through surveys involving parents and their children. These studies document intergenerational correlations in various cultural traits. For instance, correlations have been found in risk and trust attitudes (Dohmen et al., 2012), as well as in personality traits (Duncan et al., 2005).

However, while these studies highlight the importance of cultural transmission, they remain largely correlational and offer limited insights into the mechanisms underlying it. Their findings are consistent with alternative explanations, such as genetic inheritance or the influence of shared environmental circumstances, making it unclear what role active transmission by parents actually plays.

Only a few studies have taken initial steps toward unpacking the mechanisms of cultural transmission. For example, Zumbuehl, Dohmen, and Pfann (2021) show that greater parental involvement is associated with more favorable attitudes and traits in children, suggesting that parents can actively shape their children's character.

Despite these initial insights, many fundamental questions remain unanswered: Do parents actively seek to transmit traits? If so, which traits do they prioritize, and by what methods do they want to influence their children? These and many other questions remain largely unexplored within the field of cultural economics.

This gap is surprising, given that a deeper understanding of parental intentions and practices regarding cultural transmission is essential for analyzing persistent inequality and designing effective policy interventions. For example, if parents differ significantly in their approaches to transmitting traits, such heterogeneity could have long-term implications for inequality and open avenues for targeted policies, such as informational campaigns or parental support programs.

This chapter advances the literature on cultural transmission by taking a first step toward opening the 'black box' of cultural transmission. We provide new empirical evidence on how parents think about transmitting values to their children, thereby deepening our understanding of this foundational process.

Furthermore, this chapter also adds to the literature on the determinants and consequences of several individual cultural values. Specifically, we investigate seven traits that have been shown to be highly relevant for economic outcomes: patience,

5. For an overview and critical discussion of persistence studies, see Voth (2021).

risk attitude, altruism, moral universalism, religious faith, work ethic, and political attitude.

In the following, we will briefly summarize the literature on the determinants and consequences of each of these traits and describe how our study adds to this literature.

Patience refers to the willingness to forgo early benefits in order to achieve higher benefits later. Recent research identifies it as a key driver of long-term economic development (Sunde et al., 2022) and suggests that it can be shaped through formal education, particularly in schools (Dohmen, Holler, and Sunde, 2022). However, much less is known about how patience is transmitted within families. Our study addresses this gap by examining parents' belief about the transmission of patience in detail.

Risk attitude (or risk preference) is another economically significant attitude, influencing a wide range of behaviors such as investment, career choice, and entrepreneurship. Prior research shows that risk preferences are not only stable over time but also transmitted from parents to children (Dohmen et al., 2011; Dohmen et al., 2012). However, existing studies provide limited insight into whether this transmission is a deliberate and conscious process. Our study explores whether parents intentionally seek to shape their children's attitudes toward risk and how they perceive its significance.

Social preferences have been among the most extensively studied preferences in behavioral economics. We focus on two particularly salient traits: altruism and moral universalism. Altruism, the concern for the welfare of others, plays a critical role in cooperation and social cohesion (Fehr and Charness, 2025). Moral universalism, the extent to which individuals apply moral concern equally to all people, has also been shown to influence important economic and political outcomes (Enke, Rodriguez-Padilla, and Zimmermann, 2022). Despite the rich literature on the behavioral implications of social preferences (Andreoni, 1989; Fehr and Schmidt, 1999), little is known about their intergenerational transmission. Our study addresses this by investigating whether and how parents seek to instill these values in their children.

Religious faith is another key cultural trait with well-documented effects on economic behavior, social trust, and educational attainment (Becker and Woessmann, 2009; Becker et al., 2025). It is also one of the most salient values in the context of intergenerational transmission, frequently cited by parents as a core aspect of their value system. Given its centrality in many people's lives, understanding how parents think about transmitting religious faith is essential, not only for the economics of religion but also for broader inquiries into cultural persistence. Our study contributes by examining parents' beliefs and preferences regarding the transmission of religious faith.

Work ethic has long been viewed by social scientists as a fundamental cultural driver of economic development (Weber, 1904, 1905). Recent meta-analyses sug-

gest a significant intergenerational correlation in work-related values (Cemalcilar, Secinti, and Sümer, 2018), indicating that parents do influence their children’s attitudes toward effort and productivity. However, existing studies have not adequately addressed whether this influence stems from active transmission. Our study seeks to clarify this by studying beliefs about the transmission of work ethic in more detail.

Finally, we investigate political attitude. Studying the transmission of political attitude is particularly interesting in the context of polarization. A large literature in political science has documented the implications of polarization for democratic stability and civic trust (Putnam, 2000). Active transmission by parents can provide one potential reason for increasing polarization, especially if it reflects misperceptions about societal political attitude. This chapter adds to this literature in two aspects: First, the descriptive part of our study provides general evidence on how parents think about transmission of political attitude. Second, the test of substitutability in the context of political attitude directly investigates a mechanism by which cultural transmission efforts can potentially lead to increasing polarization within society.

This chapter also contributes to the theoretical literature on cultural transmission by testing two foundational assumptions: *imperfect empathy* and *substitutability*. These assumptions are central to economic models of cultural transmission (Bisin and Verdier, 2001, 2011), yet they have rarely been subjected to direct empirical validation. By examining them empirically in a carefully designed survey, we aim to bridge the gap between theoretical constructs and observed parental behavior in the transmission of cultural values.

*Imperfect empathy* refers to the idea that while parents are altruistic in the sense that they want the best for their children, they fail to fully account for differences between their own preferences and those of their children. In Bisin and Verdier’s own words, imperfect empathy denotes “a form of altruism biased towards the parents’ own cultural traits: parents care about their children’s choices, but they evaluate them using their own (the parents’ – not the children’s) preferences” (Bisin and Verdier, 2011, p. 343).

Formally, let  $X$  denote the choice set of a child. Cultural traits are represented by preferences: each individual (parent or child) chooses  $x \in X$  to maximize  $u^i : X \rightarrow \mathbb{R}$ , for cultural trait  $i \in \{a, b\}$ . If  $V^{ij}$  denotes the utility to a cultural trait  $i$  parent of a type  $j$  child,  $i, j \in \{a, b\}$ , then *imperfect empathy* denotes that for all  $i, j$ ,

$$V^{ij} = u^i(x^j), \quad \text{where } x^j = \arg \max_{x \in X} u^j(x).$$

In other words, parents assess their children’s outcomes based on their own utility function.

This type of paternalism is a central assumption in common models of cultural transmission (Bisin and Verdier, 2001, 2011), but has rarely been tested empirically.

The papers that come closest to such a test are studies on paternalism (e.g., Ambuehl, Bernheim, and Ockenfels, 2021; Tungodden and Willén, 2023), though

these studies are usually restricted to controlled experimental settings with limited economic and cultural relevance. In contrast, our study involves a more realistic – albeit hypothetical – decision context: the choice of a job, which is both economically significant and culturally salient.

*Substitutability* is the second theoretical assumption we examine. It refers to the idea that parents adjust the cultural transmission efforts to their beliefs about the transmission by other agents in society (e.g., peers, schools, media). In Bisin and Verdier's own words, substitutability occurs “when parents have fewer incentives to socialize their children the more widely dominant are their traits in the population” (Bisin and Verdier, 2011, p. 345).

To illustrate this formally, assume a binary trait (as in Bisin and Verdier, 2011). Parents face an optimization problem: They want their child to have their preferred trait, but face transmission costs. Based on the transmission by other transmitters,  $q_i$ , parents choose their own transmission effort  $d_i$ , taking into account transmission costs.

Substitutability implies that  $d_i(q_i)$  is decreasing in  $q_i$ : as others transmit the parent's preferred trait more strongly, the parent can reduce their own effort. Conversely, if others promote opposing traits, parents increase their investment in transmission. This assumption, which emerges from convex transmission costs and diminishing marginal returns to effort (see Bisin and Verdier, 2011) has yet to be tested in an experimental setting – a gap our study seeks to fill.

In the case of a continuous trait (such as political attitude), substitutability can take two forms. If other transmitters promote a trait that parents disapprove of, parents may respond by either increasing their transmission effort intensity or by pushing their child toward more extreme positions when they believe their preferred trait is underrepresented (‘overshooting’). Such dynamics can, in turn, contribute to polarization within a society.

In our study, we measure both the intended transmission effort intensity and the intended extremeness of transmission. This design allows us to distinguish between substitution through increased effort and substitution through overshooting.

The idea of a *cultural backlash* is not new to the literature. For instance, Fouka (2020) finds the German immigrants to the US were more likely to choose German names (a measure of transmission efforts) when faced with strict assimilation policies in the aftermath of World War I. However, because this finding arises from a complex historical context in which many confounding influences cannot be ruled out, it remains unclear whether it truly constitutes evidence of substitutability as defined in standard models of cultural transmission. We address this gap by using a vignette study to provide the first experimental test of substitutability in the context of cultural transmission.

Our study also contributes to the literature on child development, which has extensively examined the formation of both cognitive and non-cognitive skills (Cunha and Heckman, 2007; Cunha, Heckman, and Schennach, 2010). For each of the cul-

tural traits analyzed in this chapter, prior research has explored their developmental trajectories. For example, studies have investigated the development of altruism (Fehr, Glätzle-Rützler, and Sutter, 2013), risk preferences and patience (Sutter, Zoller, and Glätzle-Rützler, 2019), work ethic (Ter Bogt, Raaijmakers, and Van Wel, 2005), political attitude (Torney-Purta, 2006), and religious faith (Boyatzis, 2009). However, less is known about how parents think about the development of their child. While recent work has begun to consider parental beliefs (Boneva and Rauh, 2018; Attanasio, Boneva, and Rauh, 2022; Attanasio, Cunha, and Jervis, 2024), the primary focus of this literature has been on the acquisition of skills and their broad development over time. In contrast, our research centers on cultural values, with particular emphasis on their active transmission – that is, the role that others play in shaping and developing these values.

Finally, this chapter contributes to the literature on beliefs and mental models. A growing literature investigates how people reason about economic and societal processes. Recent work in this area includes studies on subjective models of the macroeconomy (Andre et al., 2022) and mental representations of financial markets (Andre, Schirmer, and Wohlfart, 2025). We extend this line of inquiry by focusing on beliefs about cultural transmission – specifically, how parents understand their role in shaping their children’s values, how they perceive the influence of other transmitters, and how these beliefs inform their transmission strategies. In doing so, we highlight cultural transmission as a domain where subjective beliefs have far-reaching implications for economic behavior and societal dynamics.

### 3.3 Research Design and Hypotheses

#### 3.3.1 Research Design

To study the process of cultural transmission, we conducted a large-scale online survey exclusively recruiting parents. The sample size of 1,200 parents is 50% male and 50% female.

Participants who failed any of the following criteria were directly screened-out and replaced by a new participant:

- **No Parents:** Only participants who had indicated on Prolific that they have children could start the survey. At the start of the survey, participants were again asked to report how many children they have. If they answered zero (contradicting their Prolific profile), they were excluded from the survey.
- **Attention Check:** Participants were presented with two attention checks. In line with the policy of Prolific, they were excluded only if they failed both.
- **Bot Detection:** The survey programming language Qualtrics provides built-in automatic bot detection via *Google Invisible reCAPTCHA* that estimates the like-

likelihood that the respondent is a human.<sup>6</sup> Following the suggestion by Qualtrics, participants were excluded if this likelihood was below 50%.

- **Time-out:** Prolific automatically screens-out participants who take longer than a duration threshold.<sup>7</sup>

The actual survey consists of two distinct parts. In the first part, we investigate beliefs about transmission descriptively. In the second part, we test two predictions from theoretical models of cultural transmission.

The complete survey text is provided verbatim in Section 3.B.1.

### 3.3.2 Part 1: Descriptive Evidence

In Part 1, the aim was to collect a large-scale dataset of relevant parental beliefs in the domain of cultural transmission. To avoid ambiguity, we consistently used the term 'shaping' throughout the survey and clarified at multiple points that it referred exclusively to *non-genetic* shaping.<sup>8</sup> In other words, this survey was not intended to be about genetic inheritance in any sense.

Each subject answered multiple questions for four economically-relevant attitudes or traits randomly selected from seven possible ones:<sup>9</sup> patience, altruism, work ethic, political attitude, religious faith, universalism, and risk attitude. These attitudes and character traits were chosen to cover a wide array of traits relevant for economics; some prominently studied in economics for a long time (e.g., risk attitude, patience), others more recently emphasized (e.g., universalism), and others with straightforward practical relevance for socio-economic outcomes (e.g., work ethic, religious faith). See Section 3.2 for more details.

Throughout the survey, these traits were referred to as 'attitudes or character traits'; for brevity, we will refer to them simply as 'traits' hereafter.

Each trait was introduced with a brief one-sentence description, see Table 3.1. For each of the four randomly selected traits, participants were then asked a series of questions about their first child. We focused on their actual child (and not 'children in general') to make the questions more tangible. Furthermore, we instructed them to focus on their *first* child to ensure comparability between parents. See Table 3.2 for an overview of the questions and how they were elicited, or Section 3.B.1 for the verbatim survey text.

6. Qualtrics. "Fraud Detection." Retrieved September 25, 2025, from <https://www.qualtrics.com/support/survey-platform/survey-module/survey-checker/fraud-detection/>

7. For this specific survey, the intended median completion time was 23 minutes, and the threshold for time-out was 73 minutes.

8. We suspected that the term 'transmission' might leave it (more) ambiguous whether (i) it includes genetic transmission, and (ii) it is intentional. We explicitly wanted to exclude genetic transmission but include both intentional and unintentional shaping. We thought 'shaping' captures this best, striking a balance between clarity and avoiding overcomplicated terms or unintended priming.

9. This random selection was solely to reduce duration and survey-fatigue concerns.

**Table 3.1.** Traits and Their Description

<b>Trait</b>	<b>Description</b>
Patience	A person with high patience is generally willing to give up something today in order to benefit from that in the future.
Altruism	A person with high altruism is generally willing to share with others without expecting anything in return.
Work Ethic	A person with high work ethic values hard work and believes that individual effort is crucial for success.
Political Attitude	A person with strong political attitude has a clear and staunch set of political opinions (e.g., liberal or conservative).
Religious Faith	A person with high religious faith strongly adheres to religious beliefs that guide both worldview and daily life.
Universalism	A person with high universalism exhibits the same level of altruism and trust towards strangers as towards members of their own community (e.g., nationality, race, religion).
Risk Attitude	A person's risk attitude indicates whether a person is generally willing to take risks or tries to avoid taking risks.

Notes: This table displays the exact one-sentence descriptions of each trait as shown to participants in the survey.

Finally, if risk attitude was among the randomly selected traits, participants were asked the following free-text question: “When you were answering these questions, what kinds of risks were you thinking about? Please write down a few key words” (60-character limit). This question was intended to capture whether the types of risks participants had in mind differed from those typically considered in economics. (For the other traits, this was considered less of a concern.) For example, they might think of juvenile risk-taking, substance abuse, or safety concerns rather than financial risks or labor market decisions that involve trade-offs between risk and expected returns. In this sense, the question serves not only to validate our measure but also to shed light on how parents understand the concept of risk attitude more broadly. It thus offers insight into how a core economic concept is interpreted in the context of intergenerational cultural transmission.

These responses were then coded by a research assistant along two dimensions: *valence* and *domain*.

**Table 3.2.** Overview of Part 1 Questions and Elicitation Methods

#	Measure	Conceptual Question	Elicitation
(1)	DeFacto	To what extent do participants think each trait is shaped by (a) parents, (b) other relatives, (c) schools/teachers, (d) peer group/friends, (e) social media/influencers?	5-point Likert scale from <i>not at all</i> to <i>extremely</i> ; plus binary direction: <i>increase</i> or <i>decrease</i> (for political attitudes: <i>liberal / conservative</i> )
(2)	Time	For each agent (a–e) above that participants believe to shape the trait at least slightly: At what age of their first child does this shaping occur predominantly?	Categorical: <i>0–6 years</i> ; <i>6–12 years</i> ; <i>12–18 years</i> ; <i>18–25 years</i>
(3)	Preference	For each trait that participants think is at least slightly shaped by them: How strongly do they want to shape it?	5-point Likert scale from <i>not at all</i> to <i>extremely</i> ; plus binary direction: <i>increase</i> or <i>decrease</i> (or <i>liberal / conservative</i> )
(4)	ConfOptLev	For each trait: How confident are participants about what the 'optimal' level is their first child should possess?	5-point Likert scale from <i>not at all</i> to <i>extremely</i>
(5)	Goal	For each trait that participants want to shape at least slightly: What goal motivates them to shape the trait?	Categorical: <i>Job Success</i> ; <i>Personal Well-Being / Happiness</i> ; <i>Moral Values / Ethics</i> ; <i>Cultural Identity / Heritage</i> ; <i>Social Acceptance / Relationships</i> ; <i>Other</i>
(6)	Method	For each trait that participants want to shape at least slightly: By which means do they want to shape the trait?	Categorical: <i>Leading by Example / Role Model</i> ; <i>Communication</i> ; <i>Rules</i> ; <i>Rewards and Punishments</i> ; <i>School Choice</i> ; <i>Other</i>

*Notes:* This table presents the questions asked in the first part. All items were asked for each trait individually. To ensure comparability across parents, all questions referred to their first child. For risk attitude, the increase referred to 'more risk-seeking.' For the exact wording of the questions, see Section 3.B.1.

**Valence:** Single-choice and coded as one of the following: *Positive*; *Negative*; *Other (Ambiguous or Neither)*; or *Unclear (Nonsensical Answer)*.

**Domain:** Multiple responses possible (except when marked *Unclear*), with the following categories: *Financial / Career*; *Physical / Psychological / Health*; *Social / Interpersonal*; *Other*; and *Unclear*.

For brevity, we refer to the categories as *financial*, *health*, and *social*, respectively, hereafter.

For the precise coding instructions, see Section 3.B.2.

### 3.3.3 Part 2: Testing Theoretical Assumptions

In Part 2, data to test two theoretical assumptions – *imperfect empathy* and *substitutability* – were collected. For the theoretical background, see Bisin and Verdier (2001, 2011), as well as the discussion in Section 3.2.

To briefly recall, imperfect empathy denotes that while parents are altruistic in the sense that they want the best for their children, they fail to fully account for differences between their own preferences and those of their children. Substitutability, on the other hand, refers to the idea that parents adjust the cultural transmission efforts to their beliefs about the transmission by other agents in society (e.g., peers, schools, media).

As the theoretical assumptions were not devised with the goal of empirical testability in mind, some form of conceptual transfer had to be made by us to make an empirical assessment of them possible. We discuss the design choices and their motivation below.

### 3.3.3.1 Imperfect Empathy

We first describe the research design to test imperfect empathy, and then justify the design choices.

Participants were presented with two stylized job descriptions. In short, job 1 offered stability, predictable working hours, less travel, and more time with family and friends. Job 2, on the other hand, was fast-paced, more career-oriented, and offered a higher salary. See Section 3.B.1 for the full survey text, including the exact job descriptions.

Then, participants were asked:

1. **ImpEmpParent:** Looking back now, which job do you wish you had chosen as your first job?

On the next page, participants were told to assume their first child would want to choose the *other* job as their first job. That is, the information on this page dynamically adjusted based on the subject's previous answer and always stipulated that the child would choose, e.g., Job 2 if the parent had chosen Job 1 for themselves. On the same page, participants were then asked:

2. **ImpEmpChoice:** Which job would you choose for your first child as their first job?
3. **ImpEmpReason:** Why would you choose this job for your first child?<sup>10</sup>

On the following screen, two follow-up questions asked participants how relevant each of the following ideas was when answering these questions:

4. **LifeCycle:** Maybe people change their preference regarding their first job over the course of their life. That is, someone might choose one job when they are

10. Elicited categorically with the same categories as the transmission goal from Part 1: *Job Success; Personal Well-Being/Happiness; Moral Values/Ethics; Cultural Identity/Heritage; Social Acceptance/Relationships; Other*.

younger, but later in life, looking back, they might wish they had chosen a different one.

5. **WorldChange:** Maybe the world has changed between when you chose your first job and when your first child is choosing their job. That is, job opportunities, societal values, or labor market conditions may have shifted in ways that affect what kind of jobs are desirable.

Both ideas were introduced with exactly these descriptions, and participants were asked: “How relevant do you think that such a change is in the context of your previous answers about your and your first child’s job preference?”<sup>11</sup>

We will now justify these design choices.

Staying closest to the theoretical assumption when testing it would have required to elicit not only observed choices (i.e., arguments that maximize utility functions), but entire utility functions. In the simplest formal description, imperfect empathy assumes that utility functions can be described as depending on a type parameter and a choice variable. For example, religious faith may serve as a type parameter and marriage choice as a choice variable. The assumption is that parents evaluate their child’s utility *as if* their child had the same type as they do, and hence they (the parents) make a cultural-transmission decision based on this perceived *as-if* utility.

In the theoretical models by Bisin and Verdier (2001, 2011), it is neither specified nor microfounded whether this counterfactual *as-if* utility should be interpreted as reflecting mistaken parental beliefs about the child’s type. Instead, they take this evaluative behavior as a primitive assumption and remain agnostic as to whether it reflects, e.g., cognitive constraints, mistaken inference, normative attitudes, or strategic reasoning. In particular, parental learning or belief updating about the child’s type does not feature in these models.

Testing this assumption empirically raises both practical and identification challenges. Practically, eliciting full utility functions is notoriously difficult, particularly in a survey context with limited complexity and time. Identification-wise, in order to detect imperfect empathy, there must be a divergence between parent and child types as otherwise the *as-if* utility coincides with the actual utility. When relying on elicited (rather than induced) beliefs about the child’s preferences, this further raises concerns about statistical power, as many parents may simply expect their child to choose similarly to themselves.

To address the practical challenge, we chose not to elicit utility functions but rather choices (that are assumed to constitute the arguments maximizing utility functions). To address the identification issue, we induced the preference conflict necessary for imperfect empathy to become observable: participants were told their child would prefer the opposite job.

11. Both were elicited on an 11-point scale from 0 ‘not relevant at all’ to 10 ‘extremely relevant.’

The precise logical step from theory to empirical design is as follows. Under imperfect empathy, parents neglect type differences between themselves and their child when evaluating the child's welfare. By eliciting the parental job preference, we obtain a revealed preference for what maximizes the parent's utility function. Then, by introducing the exogenous preference conflict we place parents in a position where the model predicts divergence. We assume that parents interpret this disagreement as stemming from type differences in the spirit of the theory. If they evaluate their child's welfare as if the child shared their own type, they should consider their own preferred job to also be best for the child. Thus, under imperfect empathy, parents will override the child's stated preference and choose their own.

Importantly, we remain faithful to the reduced-form spirit of the theory in that we take an agnostic stance on *why* parents override their child's preference. In particular, our empirical design is not intended to identify the precise rationale behind the override decision. What matters for the theory is not *why* the parent overrides the child's preference, but *that* they do so in a way aligned with their own type, rather than the child's.

We chose the job-choice setting because, in a contemporary Western context like the United States, job choice (and corresponding lifestyle choices) are particularly well-suited to give rise to high-stakes trade-offs that involve intergenerational conflict. At the same time, this domain is sufficiently neutral to minimize experimenter-demand or social-desirability concerns (compared to, e.g., domains like intercultural marriage).

To gain deeper insights into parents' reasoning, we decided to ask for their reasons in choosing this job for their child. This question was elicited categorically offering the same possible answers as in the *Transmission Goal* question of Part 1.<sup>12</sup>

Finally, we included the life-cycle and world-change items that go beyond the scope of the formal theory. In the theoretical model, such considerations – e.g., evolving preferences over the life cycle or changes in labor market conditions – are simply not modeled. When asking real parents about these matters, however, we think it is straightforward that they might be relevant to them.<sup>13</sup>

Beyond offering additional insights into participants' thought processes, these questions were motivated by concerns about potential confounds. By asking parents what they *now* wish they had chosen as their *first* job, and what they would *now* choose for their child's *first* job, we attempted to hold constant the 'self' of the parent (the current self) and the life stage at which the decision takes place (the first job).

However, this leaves open the possibility that responses reflect differing beliefs about changes in labor markets. In that case, variation in override behavior may be

12. That is, *Job Success*; *Personal Well-Being/Happiness*; *Moral Values/Ethics*; *Cultural Identity/Heritage*; *Social Acceptance/Relationships*; *Other*.

13. This is confirmed in the data, see Section 3.4.

driven not by imperfect empathy, but by perceptions of how the world has changed. Holding world factors constant would have required a substantially more complex and artificial counterfactual (e.g., asking parents to imagine being young when their child was young and choosing a first job under these conditions).

A related concern arises from life-cycle dynamics. Parents are asked to reflect on a decision they would have wanted to make in their youth, but from the perspective of their older, current self. Meanwhile, the child's preference is framed as one formulated during youth. To better understand the relevance of this asymmetry, we chose to elicit parents' perceived importance of life-cycle reasoning.

The final wording was also designed with clarity and accessibility in mind, particularly given the Prolific participant pool.

### 3.3.3.2 Substitutability

The key idea behind substitutability in the Bisin-Verdier framework is that the more likely children are to acquire a cultural trait that parents value through non-parental transmission (e.g., from teachers or peers), the less effort parents will invest in transmission.<sup>14</sup>

We decided to study this in a hypothetical vignette-style design with exogenous variation to cleanly identify effects. More concretely, we focused on the political domain and asked parents to imagine that both their child's school/teachers and friends/peer group shared the same, clearly defined political leaning. Between-subjects, this leaning was randomly assigned to be either *liberal* or *conservative*. We selected the political domain because it is particularly likely to involve conflicting values between parents and other transmitters.

We then combined this treatment variable with parents' 'target' for their child's political leaning – that is, the political leaning they *want* their child to have eventually –, to define a binary variable *misaligned*. *misaligned* is equal to 1 if the political leaning of the environment (treatment) is opposite to the parent's stated target for their child, and 0 otherwise. To minimize experimenter demand and priming effects as much as possible, we did *not* elicit this target value at this point in the survey or earlier, but instead at the end within the socio-demographic block, alongside other political preference questions.

After confronting participants with the hypothetical scenario that introduces the treatment variation, parents were then asked two questions capturing two distinct dimensions of the substitutability outcome:

1. **SubstChoice:** Assuming this scenario to be true, which values would you emphasize in the interaction with your child?

14. This is because transmission is costly and exhibits diminishing returns; see Section 3.2.

2. **SubstIntensity:** Assuming this scenario to be true, how much effort would you put in emphasizing these values?

Responses were elicited on 11-point scales: from 0 (highly liberal) to 10 (highly conservative) for *SubstChoice*, and from 0 (no effort at all) to 10 (maximum effort) for *SubstIntensity*.

### 3.3.4 Demographic and Socio-Economic Covariates

At the end of the survey, a large number of demographic and socio-economic covariates were collected. The covariates fall into the following thematic categories:

- **Subject characteristics:** Birth year, gender, US state of residence, nativity (whether born in the U.S.), ethnicity, education level, employment status, number of siblings.
- **Household and relationship context:** Current relationship status with the other parent; relationship status with a new partner (if applicable); general household income; urban vs. rural (agglomeration) type.
- **Parenting roles and styles:** Parenting style used by the subject, the subject's mother (with the subject), the subject's father (with the subject), the other parent, and (if applicable) a new partner; division of general parenting responsibilities; division of trait-shaping responsibilities.
- **Personality assessments:** Self-reported personality of the subject, other parent, new partner (if applicable), and the first child.
- **Parent-child relationship:** Self-rated quality of the relationship with the first child.
- **Child-specific information:** First child's birth year, gender and school type. Gender of the first child's siblings.
- **Political attitudes:** Subject's desired political leaning for the first child (used as target variable in the *substitutability* part); political orientation of the subject, the other parent, the new partner (if applicable), the first child's friends/peer group, and the first child's school/teachers.
- **Value orientation:** Perceived importance of various life domains (e.g., family, leisure, work), as asked in the World Value Survey (Haerpfer et al., 2020).

For the exact questions and how they were elicited, see the full survey text in Section 3.B.1.

The idea behind eliciting so many covariates was threefold. As especially the aim of the first part of the study is to shed light on a subject matter about which little is known (i.e., parents' beliefs and preferences about cultural transmission), the analysis of this part is more exploratory in nature, and a larger number of covariates might help detect insightful patterns. Closely related, one goal of this research

project was to construct a data set that could be useful for future research. As such, it is desirable to collect as much relevant information as possible about respondents. Finally, in the second part, a wide array of covariates enables the study of heterogeneous effects (albeit only in a correlational sense).

### 3.3.5 Analysis and Hypotheses

The following analyses and hypotheses were pre-registered.

Regarding Part 1, we did not have specific hypotheses, but rather aim to provide an empirical overview in a largely uncharted domain.

In Part 2, the imperfect-empathy assumption made in the theoretical literature corresponds to  $ImpEmpParent = ImpEmpChoice$ . We report what fraction of parents exhibit this property and analyze their characteristics.

Regarding the substitutability assumption, the binary *misaligned* variable is constructed in the following way. First a categorical variable *PolTargetChildCat* is constructed from the substitutability target variable *PolTargetChild*, which measures how liberal or conservative participants want their child to be (from 0 'highly liberal' to 10 'highly conservative'):

$$PolTargetChildCat = \begin{cases} \text{liberal} & \text{if } PolTargetChild \leq 4 \\ \text{neutral} & \text{if } PolTargetChild = 5 \\ \text{conservative} & \text{if } PolTargetChild \geq 6 \end{cases}$$

Then, two *misaligned* variables are constructed combining *PolTargetChildCat* with the treatment variable (i.e., whether the child's friends and school emphasize liberal vs. conservative values). If *PolTargetChildCat*  $\neq$  *neutral*, the two variables are identical: 1 if *PolTargetChildCat*  $\neq$  *Treatment*, 0 otherwise. The two *misaligned* variables differ only in how they treat responses of parents who want their child to be politically neutral:  $misaligned_{excl\ neutral}$  is undefined for these responses (hence, drops them from the analysis), where  $misaligned_{incl\ neutral} = 1$  in both treatments (hence, treats both as *misaligned*).

We pre-registered that substitutability in this setting corresponds to a positive beta coefficient in either (or both) of the following regressions:

$$|SubstChoice_i - 5| = \alpha + \beta \cdot misaligned_i + \varepsilon_i$$

where  $|SubstChoice_i - 5|$  captures the extremeness of *SubstChoice<sub>i</sub>*, and

$$SubstIntensity_i = \alpha + \beta \cdot misaligned_i + \varepsilon_i$$

Further, it was pre-registered that we report each of these regressions for each definition of *misaligned* as described above (as a robustness check), and both for the full sample and for each subsample according to *PolTargetChildCat*.

After data collection, we realized that our definition of  $misaligned_{incl\ neutral}$  may introduce a bias in the estimates as there is no randomization for the participants with  $PolTargetChildCat = neutral$ . That is, if these parents are inherently different from the others, it biases the treatment coefficient in the direction of their inherent characteristics, potentially confounding the treatment effect.

We will nevertheless report all analyses as pre-registered, but want to highlight that we believe  $misaligned_{excl\ neutral}$  to be the more sensible measure.

## 3.4 Data and Results

### 3.4.1 Data

As pre-registered, we collected 1,200 responses that pass the exclusion criteria.<sup>15</sup> A summary of sample characteristics is provided in the appendix (Table 3.A.1).

We emphasize that the only pre-specified criterion for representativeness was a roughly equal gender split (50% male, 50% female). No further demographic balancing was intended, primarily for reasons of feasibility. Note also that being a parent might naturally introduce a skew and hence comparison with the general US population is not always preferred. That being said, it is also not trivial to find reliable population statistics of US parents. In particular, US census data does not allow for conditioning on someone having children. Relative to the US general population, our sample is roughly representative of non-Hispanic White individuals, with Black respondents overrepresented and Hispanic respondents underrepresented. Additionally, the sample skews more conservative, more educated, wealthier, and more likely to be US-born than the broader population. Regarding participants' children, male children and privately educated children are overrepresented. In terms of geographic distribution, the sample is fairly balanced, though the South is slightly overrepresented, while the West and Midwest are underrepresented.

### 3.4.2 Analyses

#### 3.4.2.1 Part 1

##### Risk Understanding

Before discussing the descriptive beliefs, we first present how parents understood the trait risk attitude. Although this question was asked at the end of Part 1, in order to understand the data related to risk attitude, we think it is a prerequisite to know how participants actually understood the concept risk attitude.

As a reminder, if a subject encountered risk attitude as one of the traits, they were asked at the end of Part 1 what kinds of risks they had been thinking about.

15. See Section 3.3 for details on the exclusion criteria.

They were instructed to enter a few keywords (maximum 60 characters). These answers were then coded by a research assistant along the two dimensions *valence* and *domain* of the risks mentioned.

Figure 3.1 reports the relative frequencies. Note that the numbers denote how many participants answered a certain combination and, due to the fact that the domain dimension allowed for multiple choices, add up to more than 100% row-wise.<sup>16</sup>

		70.1% (N = 476)	43.9% (N = 298)	26.2% (N = 178)	15.3% (N = 104)	5.9% (N = 40)	
Valence	Positive	8.7% (N = 59)	6.6% (N = 45)	3.7% (N = 25)	5.2% (N = 35)	1.5% (N = 10)	25.6% (N = 174)
	Other	53.0% (N = 360)	23.3% (N = 158)	17.2% (N = 117)	8.5% (N = 58)	2.5% (N = 17)	104.6% (N = 710)
	Negative	8.1% (N = 55)	13.8% (N = 94)	5.0% (N = 34)	1.6% (N = 11)	0.7% (N = 5)	29.3% (N = 199)
	Unclear	0.3% (N = 2)	0.1% (N = 1)	0.3% (N = 2)	0.0% (N = 0)	1.2% (N = 8)	1.9% (N = 13)
		Financial / Career	Physical / Psychological / Health	Social / Interpersonal	Other	Unclear	
		Risk Domain					

**Figure 3.1.** Risk Understanding by Valence and Domain

*Note:* This figure is based on participants' responses to how they interpreted 'risk attitude' in the context of the survey. A research assistant coded these responses. *Valence* was a single-choice variable, whereas *Risk Domain* allowed multiple selections (unless *Unclear* was chosen). The figure reports both relative percentages and absolute counts. Relative percentages are normalized by the number of respondents and because *Risk Domain* allowed for multiple domains, row totals may exceed 100%. See Figure 3.A.1 for a version normalized by number of responses (so that rows sum to 100%).

First, consider the domain dimension. Strikingly, 70% of the participants thought about financial risks while less than half of them thought about health risks, and only about a quarter thought about social risks. This dominance of financial risks is not simply an artifact of the multiple-response format. For example, 2.5 times as many respondents mention financial but not health risks, compared to vice versa.<sup>17</sup> Moreover, the categories provided seem to cover most domains mentioned, as only 15% of respondents mention risks that are classified as *Other*.

16. For an alternative normalization (by total number of responses rather than respondents, so that all cells sum to 100%), see Figure 3.A.1.

17. 43.7% and 17.5%, respectively. 26% mentioned both, 12% neither. Note that this is not visible in Figure 3.1.

Next, considering the valence dimension, we see that the vast majority of participants think about risks that are neither exclusively positive nor exclusively negative. For financial risks, there are roughly equal shares of people thinking about positive and negative risks.<sup>18</sup>

Finally, the data appear to be of high quality. Only 5.9% of the responses were unclear in the domain dimension and 1.9% in the valence dimension (1.2% in both dimensions).

Given these results, we are confident that the beliefs we measured for risk attitude by and large reflect risks in the economic sense. We will report all results concerning risk attitude in the main text for all respondents, and where it is relevant also for the subsample of those who were thinking about financial risks (approximately 70% of respondents).

### De Facto Transmission

We now turn to the actual cultural-transmission beliefs. First, parents' beliefs about how much each transmitter de facto shapes each trait in their first child were elicited. This belief was elicited for all of the four presented traits and all of the transmitters (parents; other relatives; friends/peer group; school/teachers and social media/influencers). To improve readability, we will refer to these transmitters using the shortened labels: parents, relatives, friends, school, and social media.

The de facto transmission belief was measured on a 5-point Likert scale for magnitude and additionally a binary direction variable. We combine both into a single numerical variable by coding the magnitude as an integer (0 to 4) and multiplying it by the direction (increase +1, decrease -1; for risk attitude, 'increase' means more *risk-seeking*). For political attitude, *conservative* is coded as +1 and *liberal* as -1.

In Table 3.3, we report for each trait-transmitter pair the average of this 'signed de facto belief'.<sup>19</sup> In the following, we will discuss a few patterns that we consider worth highlighting.

First, for almost all traits (except political attitude, where the directional notion is different), parents believe almost all transmitters to have an effect to *increase* a trait on average. Only for social media a few traits are on average believed to be shaped almost not at all (patience) or slightly negatively (religious faith).

Further, we see that parents dominate for almost all traits (with the exceptions of political attitude, but only for moderates, and risk attitude). Work ethic (3.08), altruism (2.88) and patience (2.83) are believed to be shaped strongest by parents.

School is believed to have the second highest influence after parents for many traits and, like parents, it is thought to shape patience and work ethic the most.

18. For health risks, roughly twice as many people thought about negative risks compared to positive risks (but all with low overall frequencies).

19. For the 'unsigned' version – i.e., only considering the magnitude component –, see Table 3.A.4.

**Table 3.3.** DeFacto Transmission Average Values

Trait	Parents	Other Relatives	Friends/ Peer Group	Schools/ Teachers	Soc Media/ Influencers	N
Pat	2.83 [2.70, 2.96]	1.59 [1.47, 1.72]	0.50 [0.31, 0.69]	2.24 [2.11, 2.36]	-0.02 [-0.17, 0.13]	687
Alt	2.88 [2.75, 3.00]	1.60 [1.46, 1.73]	1.05 [0.87, 1.23]	1.85 [1.71, 1.99]	0.37 [0.22, 0.51]	682
Work	3.08 [2.98, 3.19]	1.69 [1.55, 1.83]	0.88 [0.70, 1.07]	2.34 [2.21, 2.47]	0.17 [0.01, 0.32]	679
Rel	2.62 [2.45, 2.79]	1.93 [1.79, 2.07]	0.54 [0.37, 0.71]	1.02 [0.87, 1.17]	-0.11 [-0.26, 0.03]	697
Univ	2.16 [1.99, 2.33]	1.29 [1.14, 1.44]	1.37 [1.19, 1.55]	1.80 [1.66, 1.95]	1.05 [0.89, 1.21]	690
Risk	0.54 [0.32, 0.76]	0.16 [-0.01, 0.33]	1.90 [1.74, 2.06]	0.40 [0.23, 0.57]	1.63 [1.49, 1.77]	681
Pol (L)	-2.16 [-2.46, -1.86]	-1.03 [-1.30, -0.76]	-1.97 [-2.23, -1.72]	-1.03 [-1.30, -0.76]	-1.68 [-1.93, -1.43]	216
Pol (M)	0.43 [-0.04, 0.90]	0.57 [0.24, 0.89]	-0.96 [-1.35, -0.58]	-0.47 [-0.82, -0.12]	-0.83 [-1.20, -0.47]	137
Pol (C)	2.41 [2.23, 2.60]	1.67 [1.48, 1.86]	0.32 [0.03, 0.62]	0.55 [0.29, 0.82]	0.04 [-0.24, 0.32]	331

Notes: This table reports how much participants think a trait is de facto shaped by each transmitter. Displayed are means, with 95% confidence intervals shown in gray. This belief was elicited from 0 'not at all' to 4 'extremely' and multiplied by a binary direction variable (-1 for decrease and +1 for increase, except for political attitude where -1 'more liberal' and +1 'more conservative'). For risk attitude, positive values denote 'more risk-seeking.' Results for political attitude are reported separately for self-identified liberals, moderates and conservatives. This political leaning was measured on an 11-point scale from 0 ('highly liberal') to 10 ('highly conservative') and categories were defined as follows: 0-3 = Liberal, 4-6 = Moderate, 7-10 = Conservative.

Friends and social media are believed to shape risk attitude the most (1.90 and 1.63, respectively). In particular, participants on average believe that these transmitters make their children more risk-seeking. When restricting attention to the subsample of participants who mentioned financial/career risks (as assessed by the research assistant), all signed averages increase but only marginally.<sup>20</sup>

We report the results for political attitude separately for participants' political orientation as it makes a big difference for the estimates in this part.<sup>21</sup> (As can be seen in Table 3.A.3, political orientation does not significantly affect beliefs for the other traits.) Strikingly, for both partisan groups (liberals and conservatives), re-

20. See Table 3.A.2. The increases are between 0.07 and 0.21.

21. Political orientation was measured on an 11-point scale from 0 ('highly liberal') to 10 ('highly conservative'). Categories were defined as follows: 0-3 = Liberal, 4-6 = Moderate, 7-10 = Conservative.

spondents believe all shaping to go (weakly) in ‘their’ direction. For liberals, this tendency holds strictly and sizably across all transmitters, whereas for conservatives it is strongest for parents and relatives and fades for friends and school (but both still weakly conservative). For social media the effect among conservatives is believed to be close to zero. Among moderates, shaping is believed to occur in both directions, with friends and social media having an effect toward more liberal, and family (parents and other relatives) toward more conservative. In contrast to almost all other traits and both other political orientations, among moderates parents are not believed to be the most important group, as friends and social media are believed to have a stronger influence than parents or relatives on average.

In the interest of a uniform presentation, in what follows, we report results for political attitude separately for each of the three political categories whenever it makes a difference for estimates, or alternatively state that it does not. In this case, the split results can be found in the appendix.

We next consider correlations across beliefs in the following two ways: (i) Figure 3.A.2 shows correlations across transmitters (holding the trait constant) and (ii) Figure 3.A.3 shows correlations across traits (holding the transmitter constant), both within-subject. Due to space constraints, these figures can be found in the appendix.

Regarding (i), most correlations are moderate or low, suggesting that respondents tend to distinguish between transmitters as it is *not* the case that seeing one as influential means that other transmitters are likely to be thought to be influential, too. Overall, correlations tend to be highest between parents and relatives, as well as between friends and social media.

Beyond this general picture, a few other patterns are worth noting: for altruism and work ethic, beyond parents-relatives and friends-social media, the correlations are also high for parents-school (0.30 and 0.27, respectively). For both, the parents-social media correlation is lowest and very close to zero.

For patience, the friends-social media correlation is clearly the highest (0.41), but parents-relatives is also relatively strong (0.37).

For universalism and religious faith, parents-relatives has the highest correlation (0.46 and 0.48, respectively), followed by friends-social media (both 0.37) and school-parents (0.37; 0.27).

For risk attitude, while still the parents-relatives and friends-social media correlations are high, the correlation of school with both parents and relatives is high, too (0.40 and 0.30, respectively). Apart from that, all the other correlations concerning risk attitude are (very) close to zero. This is in some contrast with the other traits, where more values are medium-low. These results are virtually unchanged when only considering the subsample of respondents who mention financial/career risks.

Regarding political attitude, when pooling all participants, we observe relatively high correlations compared to the other traits (many 0.3–0.6), especially parents-

relatives (0.59) and friends-social media (0.52). When looking at each political category separately, there is a slight level shift (but without sign changes) with liberals having somewhat lower correlations overall, and conservatives slightly higher ones with moderates in the middle. Besides, there are no major relative changes.

Moving to (ii) Figure 3.A.3, the correlations across traits (holding the transmitter constant), we see slightly higher values than in (i). This suggests that respondents still think selectively about which traits each transmitter shapes, but if anything (on average) seem to cluster them slightly more in this regard. In particular, there is some clustering across patience, altruism and work ethic.<sup>22</sup>

Beyond the patience-altruism-work ethic cluster, for parents, the universalism-altruism correlation is also relatively high (0.46), with risk-work ethic being the lowest and close to zero (0.05).

For relatives, beyond the patience-altruism-work ethic cluster, we observe overall slightly higher correlations than for parents, with religious faith-patience, universalism-altruism, universalism-religious faith all being 0.40–0.41.

For friends, in addition to the patience-altruism-work ethic cluster, religious faith-work ethic (0.33) and religious faith-patience (0.26) are among the most highly correlated.

For school, not only is the patience-altruism-work ethic cluster quite pronounced (0.44–0.53), but correlations are also overall higher, with all correlations involving universalism (except risk attitude) in the 0.32–0.52 range, and all those involving religious faith in the 0.28–0.36 range.

For social media, the patience-altruism-work ethic cluster is somewhat weaker than for other transmitters (0.33–0.41), but several other correlations are notably higher, including religious faith-work ethic (0.51), patience-work ethic (0.43), and patience-religious faith (0.41). All trait pairings involving altruism, except altruism-risk, exceed 0.30.

For each transmitter, risk attitude has the lowest correlation with the other traits (often close to zero).

We decided to exclude political attitude here, as it would have increased the number of comparisons by a lot when considering each political orientation separately.

### Transmission Time

Next, participants reported for each trait-transmitter pair at what age they believe the shaping to occur predominantly. This question was answered for each transmitter they believe to shape a trait at least slightly (i.e., not 'not at all'). We report the results taking into account that people assigned different importance to

22. For example, 0.47–0.55 for parents, 0.41–0.45 for relatives, 0.44–0.53 for school, 0.33–0.41 for social media.

each transmitter for each trait by weighing each transmission-time answer by the aforementioned de facto importance belief, normalizing appropriately.<sup>23</sup> As a result, the bars differ in height; they are taller when a transmitter is considered more important for a given trait. See Figure 3.2 for the results. In Figure 3.A.4 we also report the corresponding ‘unweighted’ version.<sup>24</sup>

To highlight a few patterns, across almost all traits, parental transmission is thought to happen (very) early. For all traits except political attitude, at least two-thirds of the importance-weighted shaping happens before the age 12. For patience, altruism and religious faith this is more than 80% even, with more than half of the importance-weighted shaping happening before age 6. That is, for patience, altruism and religious faith *0–6 years* constitute both median and mode, whereas for work ethic, universalism and risk attitude *6–12 years* is both median and mode.

Regarding relatives, we find similar patterns with transmission believed to occur slightly later. That is, for all traits except for political attitude, *6–12 years* constitute both mode and median. Also note that the importance-weighted bars are generally lower than for parents, reflecting the generally lower de facto importance belief for relatives compared to parents.

Schools are thought to likewise shape earlier rather than later (within the time frame in which they can potentially shape), with *6–12 years* always being larger than *12–18 years*, except for political attitude. We consider noteworthy that for *6–12 years*, schools are thought to be the most important transmitter for patience and altruism (i.e., more important than parents), and just slightly less important than parents for universalism and risk attitude.

Friends are thought to matter later, with *12–18 years* dominating.<sup>25</sup>

For social media, a very similar picture emerges: *12–18 years* is both mode and median for all of the traits. Furthermore, within the *18–25 years* bracket, social media has the highest weighted-importance score of all transmitters for all traits (with friends only slightly behind for work ethic and political attitude, but everywhere else clearly first). However, *18–25 years* is generally thought to be of far less importance relative to the other age brackets for all traits.

When looking for differences across traits, often all traits except political attitude exhibit similar patterns. For political attitude, for each transmitter the mode and median of transmission time is *12–18 years*. Importantly, this result does *not*

23. That is, each answer is weighted by the absolute value of the corresponding de facto belief (i.e., magnitude only, excluding direction). To normalize across traits, we divide by the total number of people who encountered this question of each trait.

24. That is, for each trait, one stacked bar per transmitter with the ‘plain’ percentages of different time answers for this trait-transmitter pair. Each bar therefore sums up to 100%.

25. When friends are believed to matter the most (second most important transmitter for political attitude, risk attitude and universalism), the *12–18 years* category is dominant, and likewise so for work ethic and (shared with *6–12 years*) for religious faith. Only for patience and altruism is the weighted score of *6–12 years* higher, with *12–18 years* not far behind as second-highest, however.

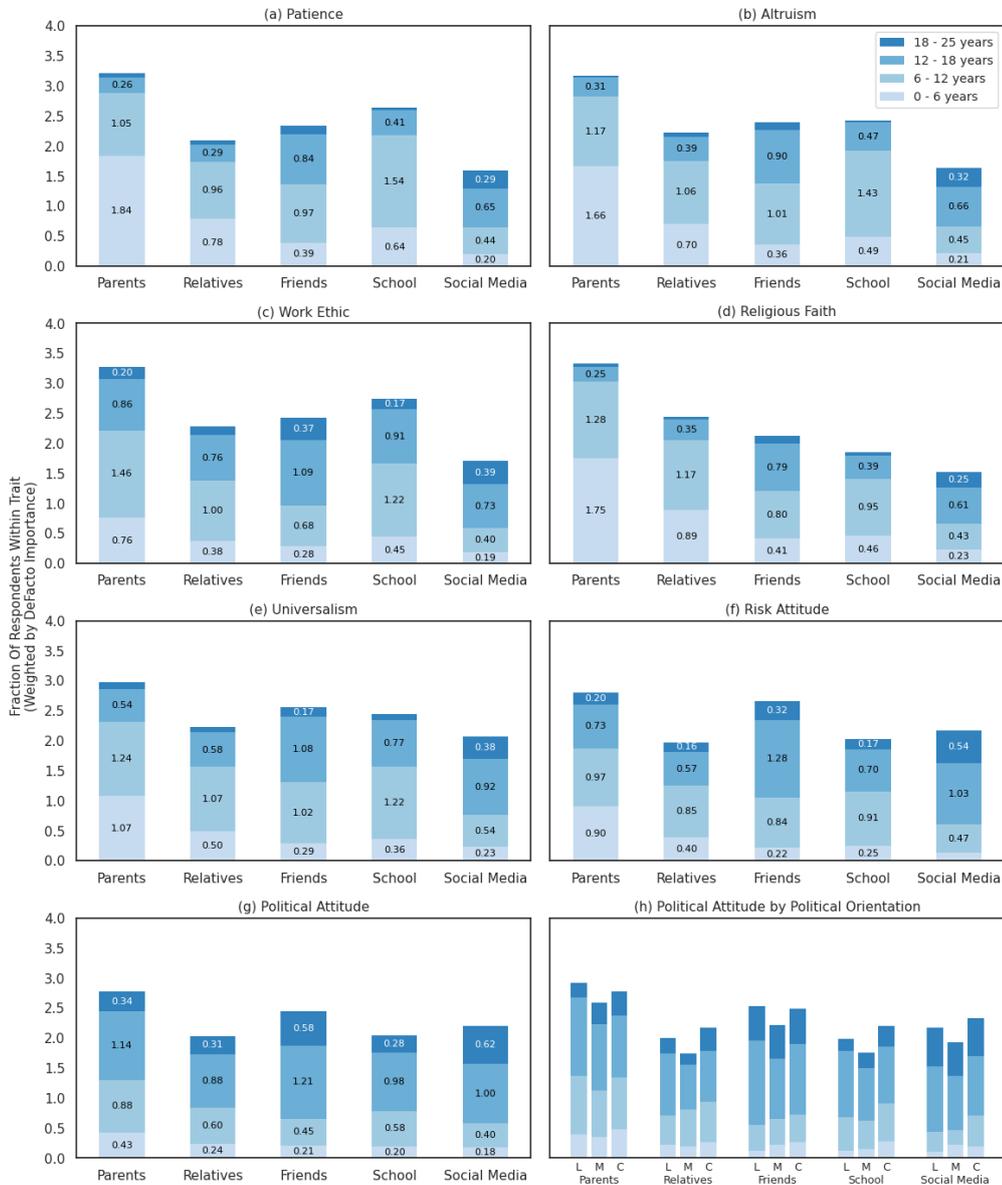


Figure 3.2. Transmission Time

Note: This figure shows the ages at which participants believe each trait is predominantly shaped. This question was answered for each transmitter they believed shaped the trait at least slightly (i.e., not 'not at all'). Responses are weighted by the magnitude of participants' de facto importance belief for the respective trait-transmitter pair. This means that more influential transmitters contribute more to the overall distribution. The height of each bar reflects the transmitter's average perceived importance for that trait, normalized by the number of participants who encountered the trait in the first place. Bars are broken down into the four age bins: 0-6, 6-12, 12-18, and 18-25 years. For the unweighted version, in which each bar reflects raw percentages and sums to 100%, see Figure 3.A.4.

depend on self-reported political orientation, but holds for both liberals, moderates, and conservatives (see panel (h) of Figure 3.2).

To summarize, parents largely subscribe to the view that shaping occurs earlier rather than later. While the objective benchmark is often unclear, this belief aligns with findings from the child development literature that emphasize the importance of early childhood for the development of non-cognitive skills (Heckman, Pinto, and Savelyev, 2013).

On the other hand – particularly for political attitudes – sociological research highlights the so-called *impressionable years* from ages 18 to 25 as especially influential (Sears and Brown, 2023).<sup>26</sup> While parents do recognize that political attitudes are shaped later than other traits, they do not adjust their timing accordingly: they continue to place greater importance on the 12–18 age range than on the impressionable years.

Overall, parents assign very little importance on the 18–25 period. Across all transmitter–trait pairs, the *18–25 years* category accounts for at most 28%, and in many cases, it is negligibly small.

### Transmission Preference

Next, parents were asked how much they *want* to transmit each trait – like the de facto belief on a 5-point Likert scale for magnitude with an additional binary direction variable. Unlike the questions before, this question (and the following ones) were only elicited for the transmitter parents and, like transmission time, this question was answered for each trait participants believe parents to shape at least slightly. See Figure 3.3 for the results of all traits except political attitude and Figure 3.4 for political attitude. We deliberately chose a different color scheme between the two figures to highlight that for all traits except political attitude the direction denotes increase/decrease (for risk attitude increase/decrease of risk *seeking*). For political attitude the direction was liberal (negative values) vs. conservative (positive values).

Considering Figure 3.3, there is a very strong skew toward a preference to increase the trait. This holds particularly strongly for patience, altruism and work ethic (more than 94% want to increase these traits at least moderately). Still more than 80% want to increase universalism and religious faith, with the modal response being to ‘highly’ increase them. Notably, more than two-thirds of participants want to make their children more *risk-seeking*.<sup>27</sup> This result is very similar for the sub-

26. While some studies suggest that impressionable years may also matter for traits beyond political attitudes (Bietenbeck, Sunde, and Thiemann, 2025; Cvetković et al., 2025), most of the evidence focuses on political or politically adjacent domains (e.g., Roth and Wohlfart, 2018; Falco and Corbi, 2023).

27. Note, though, that this question was only shown to participants who believed a trait is at least slightly (i.e., not ‘not at all’) shaped by them.

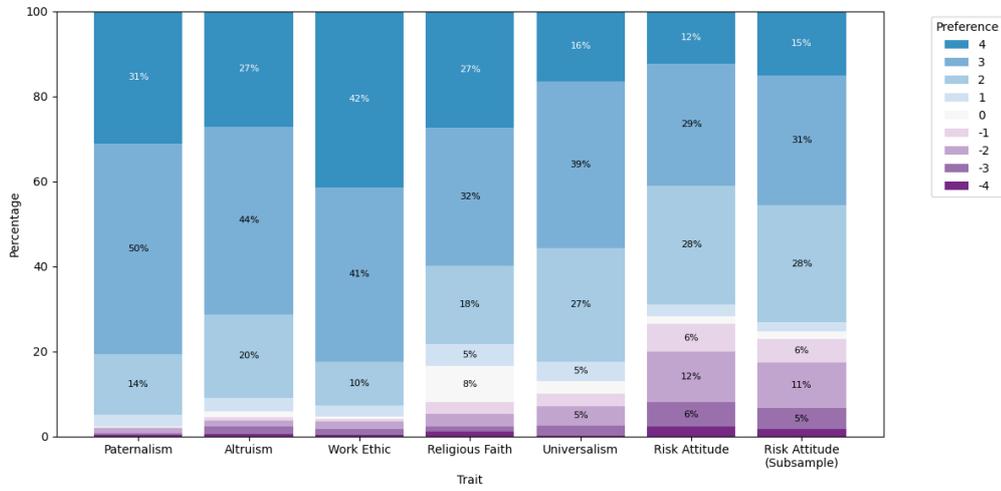


Figure 3.3. Transmission Preference

Note: This figure shows how strongly participants want to shape each trait, conditional on believing they de facto shape it at least slightly. Responses were given on a 5-point Likert scale (from 'not at all' to 'extremely') along with a binary direction (increase vs. decrease; for risk attitude: more or less risk-seeking). Due to the different direction, political attitude is reported separately in Figure 3.4. 'Risk Attitude (Subsample)' refers to the subsample of respondents who mentioned financial or career risks in the risk-understanding question.

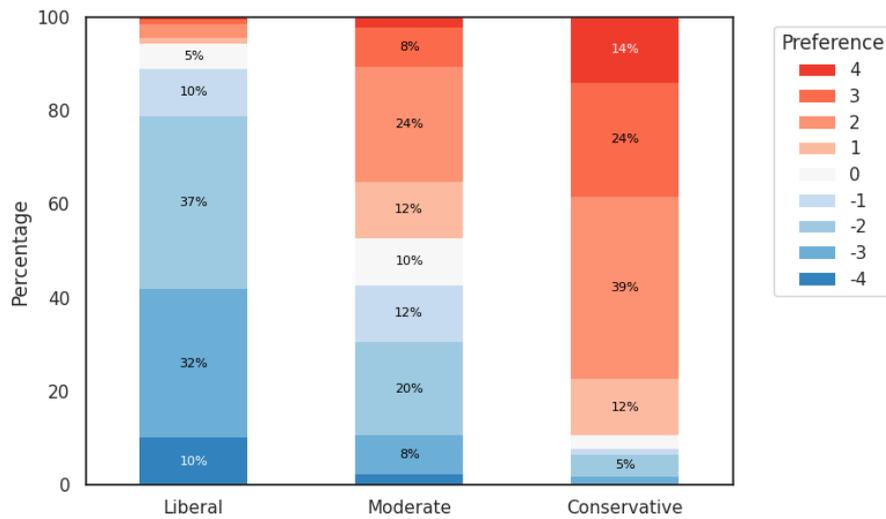


Figure 3.4. Transmission Preference (Political Attitude)

Note: This figure complements Figure 3.3 and presents transmission preferences for political attitude by political ideology, conditional on the participant believing they de facto shape it at least slightly. Responses were given on a 5-point Likert scale (from 'not at all' to 'extremely') along with a binary direction: negative values denote a preference toward more liberal attitudes, and positive values toward more conservative ones. A different color scheme from Figure 3.3 is used to reflect this conceptual difference.

sample of participants who considered financial and career risks (Figure 3.A.5). If anything, a slightly higher proportion expressed highly or extremely wanting to shape in the risk-seeking direction.<sup>28</sup>

Political attitude is naturally distinct from the other traits, and we report the results in Figure 3.4 according to the three-way split by political orientation. Notably, few respondents fall into the neutral and adjacent ‘slightly’ liberal/conservative categories – not only among liberals and conservatives, but also among moderates (10–12% each).<sup>29</sup> For all three political orientations approximately 40% of participants want to *moderately* shape their child’s political attitude. For liberals and conservatives, this is unsurprisingly in ‘their’ political direction. Furthermore, beyond modes, for liberals and conservatives, around twice as many participants want to shape it highly compared to slightly and not at all combined. For moderates, the biggest fractions of respondents want to shape their child moderately conservatively (24%) and moderately liberally (20%). The distribution for moderates is also remarkably symmetric beyond modes.<sup>30</sup>

Next, consider the correlation between parents’ belief how much a trait is de facto shaped by them and their preference to shape it (Table 3.4, column 1).

**Table 3.4.** DeFacto-Pref-OptLev Correlation

Trait	(1) (DeFacto,Pref)	(2) (Pref,OptLev)	N
Patience	0.33	0.43	684
Altruism	0.41	0.50	666
Work Ethic	0.41	0.51	677
Religious Faith	0.53	0.61	681
Universalism	0.46	0.53	681
Risk Attitude	0.55	0.53	668
<i>Subsample: Financial Risks</i>	0.57	0.60	471
Political Attitude			
<i>Liberal</i>	0.33	0.58	208
<i>Moderate</i>	0.50	0.43	131
<i>Conservative</i>	0.30	0.50	321

*Notes:* This table reports within-subject Spearman correlations between the de facto belief and the transmission preference (column 1), and between the transmission preference and confidence in knowing the optimal level (column 2). In column 1, both variables are measured on a scale from –4 to 4. For column 2, only the magnitude of the preference (ignoring direction) is used (0 ‘not at all’ to 4 ‘extremely’), as the optimal level is measured on the same 0–4 scale.

28. Compared to the full sample, 3 percentage points more respondents chose ‘extremely’ and 2 percentage points more chose ‘highly.’

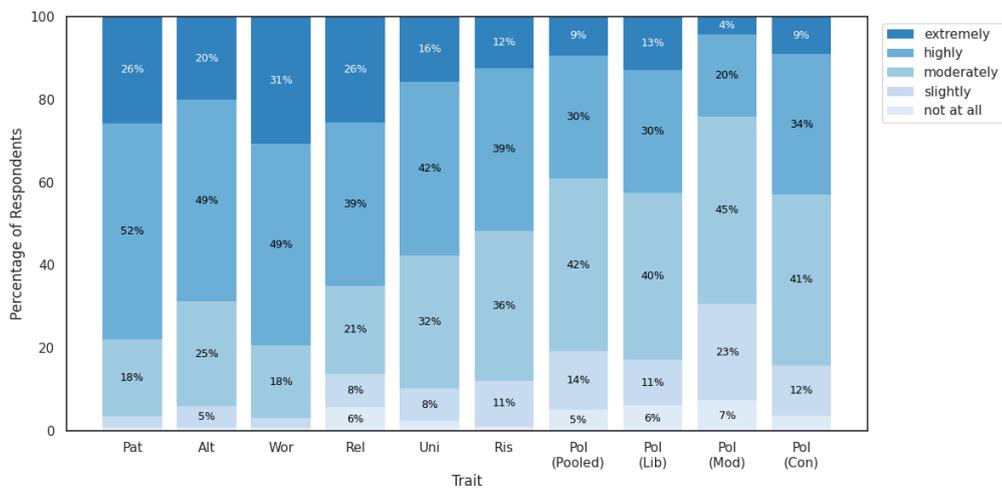
29. This does not hinge on the definition of moderates that includes values 4 and 6 in the 0–10 liberal-conservative spectrum, but also holds when only considering ‘strict moderates’ with exactly the middle value 5. See Figure 3.A.6.

30. Again, this does not hinge on the definition of moderates. See Figure 3.A.6.

Overall, we find strongest alignment for risk attitude (0.55–0.57) and religious faith (0.53), followed by political attitude for moderates (0.50). Universalism, altruism and work ethic take a second tier (0.41–0.46), while patience, and political attitude for liberals and conservatives exhibit moderately low correlations (0.30–0.33).

### Confidence in Knowing Optimal Level

Next, parents were asked how confident they are that they know the optimal or 'right' amount their child should possess of each trait. See Figure 3.5 for the results.



**Figure 3.5.** Confidence in Knowing Optimal Level

*Note:* This figure summarizes how confident participants are that they know the 'right' or optimal level of each trait for their first child. Confidence was rated on a 5-point scale ranging from 'not at all' to 'extremely.' For political attitude, responses are reported both pooled and separately for each of the three political orientations (liberal, moderate and conservative).

Overall, parents are very confident. For all traits except political attitude, more than half of the parents are at least *highly* confident, and confidence is highest for work ethic, patience, and altruism. Within political attitude there are no differences between liberals and conservatives, only moderates are somewhat less confident. The fraction of parents who is less than moderately confident never exceeds 15% when excluding political attitude.<sup>31</sup>

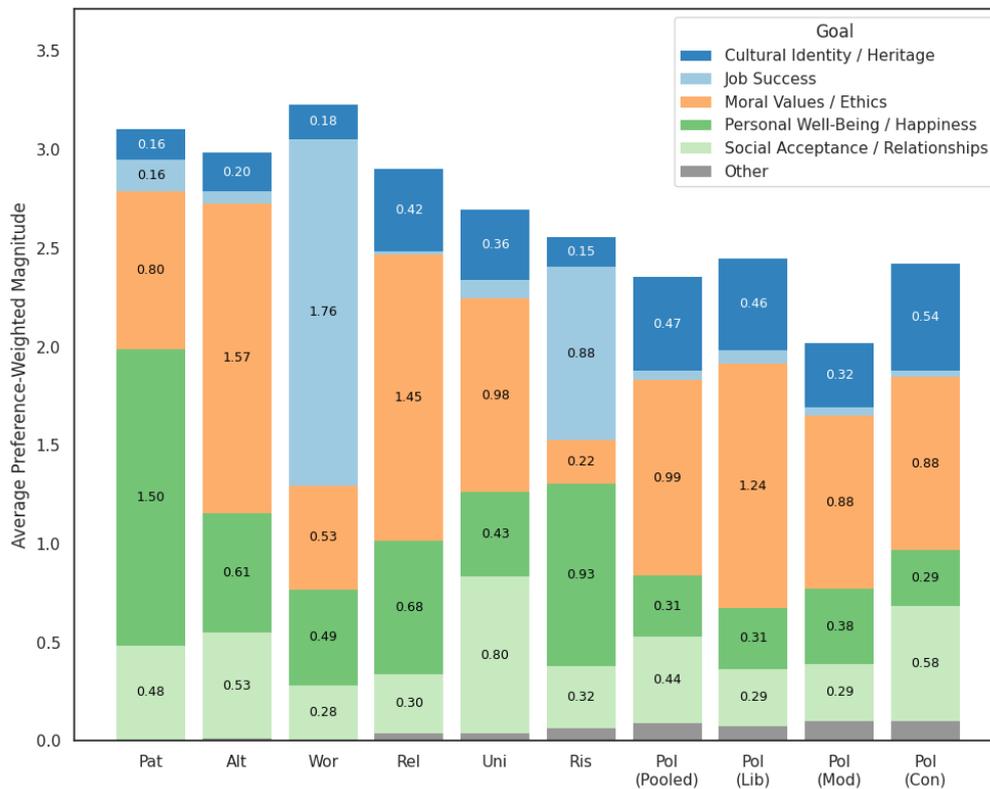
Next, consider the correlation between parents' confidence and their preference to shape a trait (Table 3.4, column 2). Here, we use the unsigned preference coding as the optimal-level confidence belief is also measured only in magnitude.

31. When including political attitude pooling the three political orientations, this fraction is still below 20%. Only for moderates, 30% of respondents are at most slightly confident.

The correlations, ranging from 0.43 to 0.61, are moderately strong (especially compared to the other correlations presented so far) and support the idea that, on average, confidence and preference go hand-in-hand: The more confident parents are that they know the optimal level of a trait, the stronger the desire to shape it (and vice versa).

### Transmission Goal

Next, parents were asked with which goal in mind they want to transmit a certain trait. This question was answered for each trait they want to shape at least slightly. Figure 3.6 weighs responses by their preference (analogously to how the transmission-time responses were weighted by the de facto transmission belief). For the unweighted version, see Figure 3.A.7.



**Figure 3.6.** Transmission Goal

*Note:* This figure shows which goals motivate participants' desire to shape specific traits. Participants answered this question for each trait they wanted to shape at least slightly. The responses are preference-weighted, such that stronger transmission preferences contribute more (analogous to how transmission-time responses in Figure 3.2 were weighted by de facto transmission belief). The height of each stacked bar reflects the average preference-weighted magnitude, and the color segments indicate the distribution of goals. For the unweighted version (i.e., with each bar adding up to 100%), see Figure 3.A.7. For a version of the risk attitude bar that includes only respondents who mentioned financial or career-related risks in the risk-understanding question, see Figure 3.A.8.

The dominant (i.e., modal) goal varies strongly by trait.

Work ethic is clearly dominated by the goal *Job Success*. Altruism and religious faith, and to lesser extents universalism and political attitude, are dominated by *Moral Values / Ethics*.

Patience is the only trait for which *Personal Well-Being / Happiness* is clearly dominant; for risk attitude it is also the mode, but only slightly ahead of *Job Success*. The corresponding plot for the subsample of respondents who mentioned financial risks shows a very similar distribution (Figure 3.A.8).

*Cultural Identity / Heritage* and *Social Acceptance / Relationships* seem to be less important goals in general. They only seem to matter for universalism, where *Social Acceptance / Relationships* is the second most important goal, and for political attitude where both are roughly equally important in second and third place. For political attitude, there are no stark differences between the three political orientations. *Moral Values / Ethics* is the modal goal for all groups – albeit somewhat less so for conservatives, with slightly more people responding *Social Acceptance / Relationships*; otherwise, there are no major differences.

Note also that the response *Other* was selected rarely for all traits, suggesting that the goals parents actually had in mind are captured well by the given categories.

In Figure 3.A.9, we also report goal-preference combination frequencies.

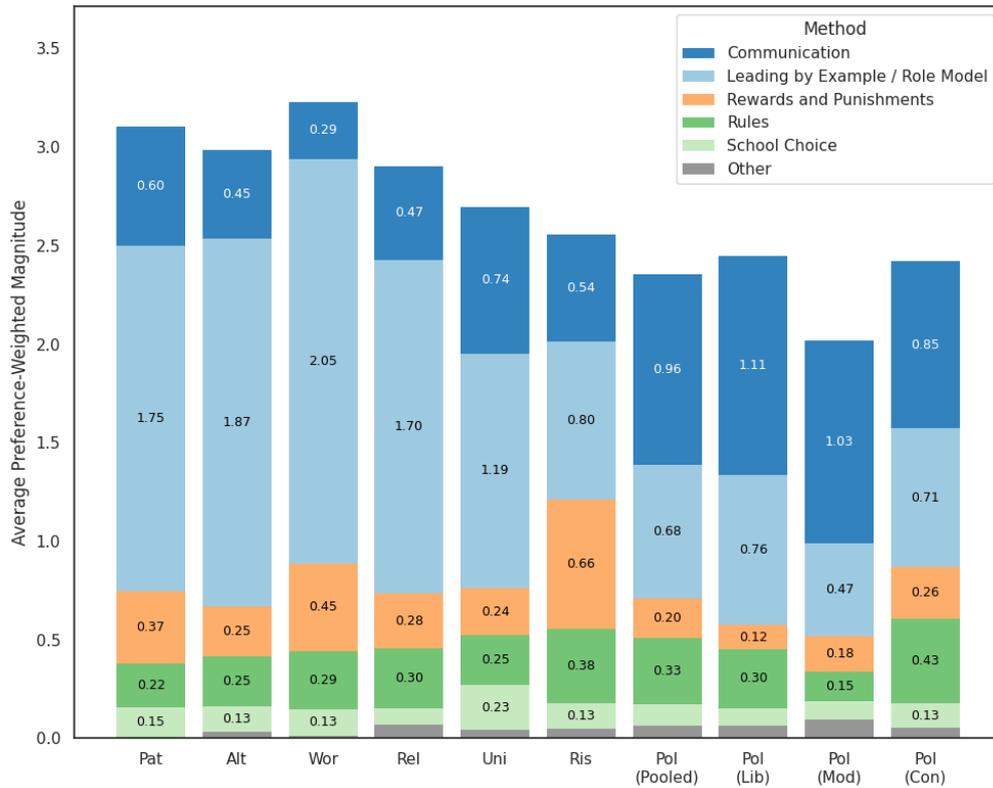
### Transmission Method

Last, parents were asked with which method they intend to shape each trait. Like transmission goal, this question was answered for each trait they want to shape at least slightly.

See Figure 3.7 for a preference-weighted stacked bar plot, analogous to Figure 3.6 for transmission goal. For the unweighted version, see Figure 3.A.10.

*Leading by Example / Role Model* stands out as the dominant method across all traits, except for political attitude, where it still ranks second. For political attitude, *Communication* is the modal method, and this holds for all three political orientations.<sup>32</sup> The dominance of *Leading by Example / Role Model* is particularly pronounced for work ethic, altruism, patience, and religious faith, somewhat weaker for universalism, and more balanced in the case of risk attitude. For the latter, although *Leading by Example / Role Model* remains the most frequently mentioned method, *Rewards and Punishments* and *Communication* follow closely. The corresponding plot for the subsample of respondents who mentioned financial risks

32. There are only slight differences in that conservatives mention *Communication* slightly less and *Rules* slightly more. Moderates mention *Leading by Example / Role Model* slightly less than the other two groups.



**Figure 3.7.** Transmission Method

*Note:* This figure shows which methods participants intend to use to shape specific traits. Participants answered this question for each trait they wanted to shape at least slightly. The responses are preference-weighted, such that stronger transmission preferences contribute more (analogous to how transmission-time responses in Figure 3.2 were weighted by de facto transmission belief). The height of each stacked bar reflects the average preference-weighted magnitude, and the color segments indicate the distribution of reported methods. For the unweighted version (i.e., with each bar adding up to 100%), see Figure 3.A.10. For a version of the risk attitude bar that includes only respondents who mentioned financial or career-related risks in the risk-understanding question, see Figure 3.A.11.

shows a very similar distribution (Figure 3.A.11). *Rules*, and even more so *School Choice*, generally play only a minor role.

Note also that *Other* was selected even more rarely than for transmission goal, suggesting that the again the given categories capture the breadth of methods parents intend to employ well.

Like for transmission goal, we also report method-preference frequency combinations in the appendix (Figure 3.A.12).

## Outlook

We consider it very interesting to examine how the patterns just described change with respect to variation in several covariates. These could include the gen-

der of the parent or the child, their birth year, parenting styles and many more. Certain combinations of covariates also appear particularly interesting, for example, whether the parent and child share the same gender. While we believe that multiple such follow-up analyses are both feasible and valuable using this dataset, they are beyond the scope of the present paper, whose primary aim is to offer an initial overview of parental beliefs about cultural transmission.

### 3.4.2.2 Part 2

In the second part of the study, two theoretical assumptions were tested: *imperfect empathy* and *substitutability*.

#### Imperfect Empathy

In this part, we test whether participants exhibit imperfect empathy by looking at whether they make paternalistic choices when their child's job preference diverges from their own (i.e., the parents' job preference for themselves). Recall that this preference conflict was exogenously stipulated.

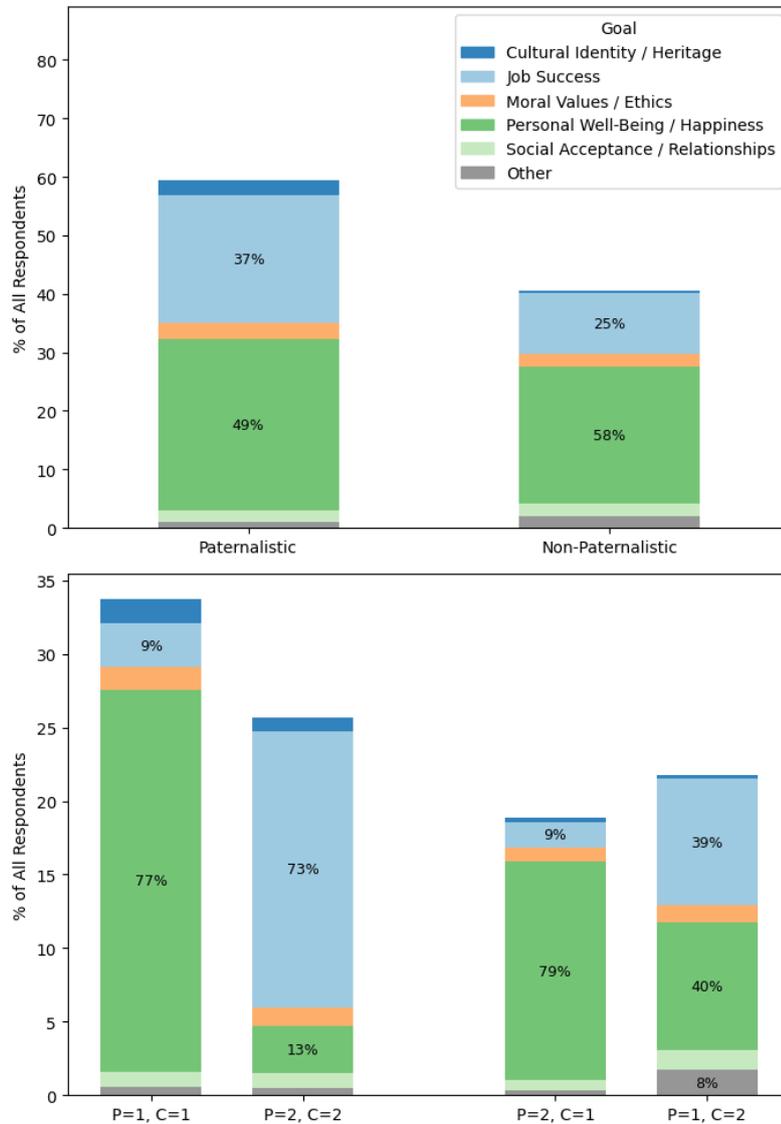
See Figure 3.8 for an overview of the data. To shorten labels, we call the 'imperfect empathy choice' *paternalistic* – i.e., a subject choosing the same job for their child as they themselves would have chosen, despite the hypothetical scenario being that the child prefers the *other* job. The upper panel reports relative frequencies of paternalistic and non-paternalistic choices. The lower panel splits each of these two bars by the two respective possibilities that constitute paternalistic and non-paternalistic choices.<sup>33</sup> Additionally, both in the upper and in the lower panel, the bars are stacked to inform about the reason a parent selected.

First, focus on the bigger picture in the upper panel. Overall, 60% of respondents are paternalistic (i.e., exhibit imperfect empathy) and 40% are not. The composition of reasons given is similar between these two groups. That is, the conditional frequency of each reason among paternalistic respondents closely matches that among non-paternalistic respondents.<sup>34</sup> In both groups, *Personal Well-Being / Happiness* is the most important one, *Job Success* the second most-important one and all other reasons are negligible. The main difference seems to be a slight shift in mass from *Job Success* to *Personal Well-Being / Happiness* when moving from the paternalistic group to the non-paternalistic group.

In the lower panel, the two groups (paternalistic and non-paternalistic) are split according to the exact combination of parental choice for the parent on the one hand and for the child on the other hand.

33. For example, for paternalistic whether the parent chose Job 1 for both or Job 2 for both.

34. Note that the numbers within each bar category refer to the conditional frequency of that reason within this group. For example, 37% for *Job Success* in the *paternalistic* group denotes that from all participants who made a paternalistic choice, 37% state *Job Success* as a reason.



**Figure 3.8.** Imperfect Empathy

*Note:* This figure summarizes evidence for imperfect empathy. Participants were asked to choose a job for their child in a scenario where the child’s preference diverged from the parent’s own. The upper panel groups respondents by whether they made a *paternalistic* choice – i.e., choosing the same job for the child that they would choose for themselves despite the conflict – or a *non-paternalistic* one. The lower panel breaks down each of these two categories into their constituent decision types: for example, choosing Job 1 for both self and child vs. choosing Job 2 for both. In both panels, bars are stacked to show the stated goal underlying each choice, using the same goal categories as in Figure 3.6.

First, we see that both paternalistic combinations occur more frequently than either non-paternalistic one. In terms of reasons given, (Parent: Job 1, Choice: Job 1) and (Parent: Job 2, Choice: Job 1) exhibit very similar (conditional) likelihoods of reasons given. For the other two, we see that the aforementioned slight shift on

the aggregate stems from these respondents who chose Job 2 for their child. That is, for parents who chose Job 2 (the more career-oriented job) for their child, parents choose this job mostly out of *Job Success* reasons when they would have chosen it for themselves, too (but the child would have preferred the other job). When the parent would have chosen Job 1 but the child prefers Job 2, the reason for following the child's choice are more balanced with equal likelihood of doing this with the motives *Job Success* or *Happiness / Well-Being* in mind.

Next, consider the questions asked about life-cycle and world-change shifts.<sup>35</sup> See Figure 3.9 for a heatmap showing how the share of paternalistic responses varies across combinations of perceived importance of life-cycle and world-change considerations.

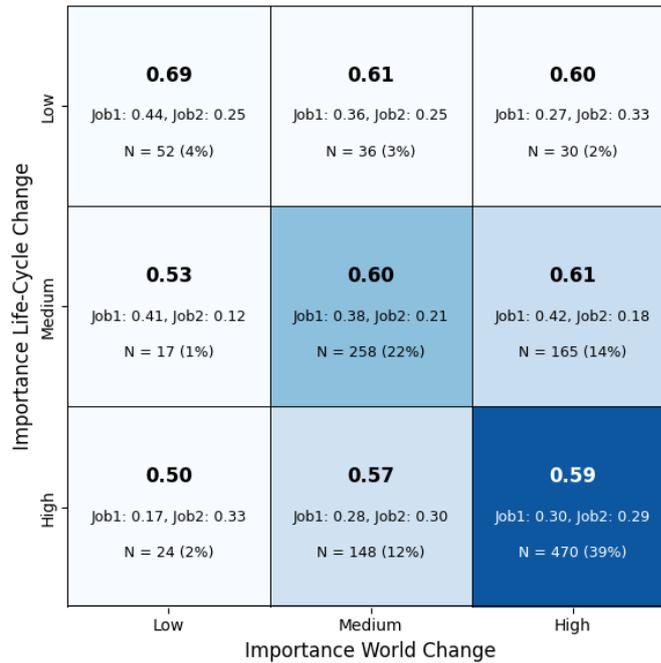
First, we see that participants generally attach high importance to both dimensions. Approximately two-thirds of participants assign high importance to at least one of the two, and 39% assign high importance to both. (Only 13% assign low importance to at least one.) The rank correlation (Spearman) between the two is moderately strong: 0.52 for the underlying 11-point scale and 0.41 for the binned categories.

Looking at the frequency of paternalistic vs. non-paternalistic choices, strikingly, the fractions are very stable around 60%, with the only major deviations occurring for cells with very few observations. From this, we infer that aggregate within-group paternalism seems to be largely invariant with respect to stated importance of either idea. In other words, '60% paternalistic, 40% non-paternalistic' holds very generally.

To explore more systematically whether these or other covariates predict imperfect empathy, we regressed *paternalistic* on a broad set of control variables. For the full output of an OLS and a Logit specification, see Table 3.A.5. We did pre-register to analyze the characteristics of this paternalistic vs. non-paternalistics split, but as this precise regression was not pre-registered we refrain from drawing strong conclusions and report the results primarily for exploratory purposes.

We find that only two covariates are significantly associated with imperfect empathy at the  $p < 0.05$  level in both the linear probability and Logit models: having at least a college degree ( $p = 0.018$  OLS,  $p = 0.017$  Logit) and being born in the US ( $p = 0.020$  OLS,  $p = 0.019$  Logit). The coefficients are of substantial magnitude (10.7pp and 13.6pp, respectively, in the OLS specification). However, these binary variables are highly imbalanced: only 14.2% of participants (130 individuals) do not have at least a college degree, and only 7.5% (90 individuals) were not born in the US. While the results are robust to heteroskedasticity-consistent standard errors and consistent across model types (OLS and Logit), we remain cautious about

35. That is, participants' responses regarding how much the ideas "Maybe people change their preferences regarding their first job over the course of their life" and "Maybe the world has changed between when you chose your first job and when your first child is choosing theirs," respectively, mattered when answering the previous questions.



**Figure 3.9.** Imperfect Empathy: Relevance of Life-Cycle and World-Change Ideas

Note: This figure shows how the likelihood of making a paternalistic choice varies with participants’ stated importance of life-cycle and world-change considerations. Each cell represents a tercile combination of the two beliefs: the perceived importance of (i) people changing job preferences over their life course and (ii) societal or environmental change across generations. The large number in each cell indicates the overall share of paternalistic choices for that group. The smaller numbers below it decompose this share by which job (Job 1 vs. Job 2) the parent chose for the child, conditional on making a paternalistic choice (i.e., choosing the same job for child and self). These two values sum to the total paternalism rate shown above. Sample sizes and their proportions relative to the full sample are listed below and visually represented by the intensity of the blue shading in each cell.

interpreting these associations as generalizable or stable across samples, given the strong imbalance in the underlying covariates. That said, at least in our sample, participants who were born in the US or have at least a college degree on average exhibit imperfect empathy more frequently.

Beyond these two predictors, only one additional variable – self-reported importance of religion – reaches marginal significance at the  $p < 0.10$  level ( $p = 0.077$  OLS and Logit). Participants who rate religion as more important are slightly more likely to be paternalistic.<sup>36</sup>

36. The OLS coefficient indicates an increase of 2.8 percentage points per unit on the four-point scale.

Notably, we find no significant associations with participants' or their children's gender (or their interaction), ethnicity, political orientation, or parenting style. Furthermore, the estimated effects for these variables are close to zero.<sup>37</sup>

Referring back to the previous figure, the variables capturing life-cycle and world-change relevance also yield insignificant results with point estimates close to zero.

We therefore conclude that the aggregate *paternalistic* ratio of 60% remains remarkably stable across variations in covariates.

### Substitutability

In this part, we test whether parents engage more in cultural transmission when their goals are misaligned with their child's social and school environment, *ceteris paribus*.

To this end, a between-subjects treatment variation was introduced, asking parents to imagine that their child's friends/peers and school/teachers are either *liberal* or *conservative*. Combined with the parents' target for the child's political orientation, this allows us to define a *misaligned* variable in two ways (excluding or including, respectively, parents who want their child to be politically neutral; see Section 3.3 for more details).

We measure the magnitude of cultural transmission in two ways: extremeness and intensity. As pre-registered, substitutability in this setting corresponds to a positive beta coefficient in either (or both) of the following regressions:

$$|\text{SubstChoice}_i - 5| = \alpha + \beta \cdot \text{Misaligned}_i + \varepsilon_i$$

where  $|\text{SubstChoice}_i - 5|$  captures the extremeness of  $\text{SubstChoice}_i$ , and

$$\text{SubstIntensity}_i = \alpha + \beta \cdot \text{Misaligned}_i + \varepsilon_i$$

with both regressions run twice each for either *misaligned* definition.

As discussed in Section 3.3, we now believe the definition that excludes neutral-target parents is more appropriate, since the alternative that includes neutral-target parents does *not* induce random variation among these additional participants. However, we report results for both definitions, as pre-registered.

See Table 3.5 and Table 3.6 for the results on intensity and extremeness, respectively.

We do not find evidence to support the substitutability assumption.

First, consider the intensity outcome (Table 3.5). The estimated treatment effects are not significantly different from zero across all four specifications

37. For example, absolute effects for ethnicity and political orientation are smaller than 1 percentage point, and for subject and child gender smaller than 3 percentage points.

**Table 3.5.** Substitutability Intensity

	(1)	(2)	(3)	(4)
Misaligned Measure	excl. neutral		incl. neutral	
Misaligned	0.1232 (0.1232)	0.0791 (0.1218)	-0.0890 (0.1170)	0.0860 (0.1214)
SubstTarget	0.0532 (0.0183)	0.0170 (0.0501)	0.0592 (0.0185)	0.0170 (0.0496)
Constant	7.2311 (0.1431)	7.4409 (0.5500)	7.1975 (0.1442)	7.5217 (0.5363)
Controls		✓		✓
Outcome SD	2.002	2.002	2.025	2.025
$R^2$	0.010	0.114	0.010	0.129
$p$ (Misaligned)	0.317	0.516	0.447	0.4789
Observations	1046	1038	1200	1189

Notes: This table reports OLS regression estimates. The outcome is *SubstIntensity* (parental transmission effort). *Misaligned* is binary and equal to one if and only if parents' target for the child's political leaning (*SubstTarget*) and the treatment (hypothetical leaning of child's school and friends) are misaligned. Robust standard errors (HC1) in parentheses. Columns (2) and (4) include a large number of controls (see Table 3.A.5 for the full list).

**Table 3.6.** Substitutability Extremeness

	(1)	(2)	(3)	(4)
Misaligned Measure	excl. neutral		incl. neutral	
Misaligned	-0.1346 (0.0948)	-0.2279 (0.0880)	-0.5501 (0.0930)	-0.2232 (0.0880)
SubstTarget	0.0279 (0.0144)	0.1130 (0.0406)	0.0396 (0.0153)	0.1192 (0.0399)
Constant	2.8842 (0.1093)	2.0705 (0.4401)	2.8185 (0.1140)	2.0602 (0.4227)
Controls		✓		✓
Outcome SD	1.537	1.537	1.642	1.642
$R^2$	0.006	0.177	0.034	0.291
$p$ (Misaligned)	0.157	0.010	0.000	0.011
Observations	1046	1038	1200	1189

Notes: This table reports OLS regression estimates. The outcome is *SubstExtremeness* (extremeness of parental transmission). *Misaligned* is binary and equal to one if and only if parents' target for the child's political leaning (*SubstTarget*) and the treatment (hypothetical leaning of child's school and friends) are misaligned. Robust standard errors (HC1) in parentheses. Columns (2) and (4) include a large number of controls (see Table 3.A.5 for the full list).

( $p = 0.32\text{--}0.51$ ). While in three of the four specifications the treatment effect is positive – including those based on the definition we find more sensible (excluding neutral) –, the magnitudes are small (0.04–0.06 SD).

As pre-registered, we also report these estimates separately for parents with liberal and conservative targets (Table 3.A.6). The slightly positive average effect is entirely driven by parents with liberal targets, but even here the coefficient is insignificant and small ( $p > 0.37$ , coefficient  $< 0.08$  SD).

Turning to the extremeness outcome (Table 3.6), three out of four specifications yield a highly significant negative treatment effect, with sizable magnitudes (0.15–0.34 SD). In the remaining specification (1), the estimated treatment effect is not significant at conventional levels ( $p = 0.157$ ), but also negative.

Examining the liberal and conservative target subsamples separately (Table 3.A.7), we find that although coefficients are negative for both groups, the effect is roughly four times larger for liberals and only significant in that group ( $p < 0.01$ , coefficient  $\approx 0.25$  SD).<sup>38</sup>

To better understand this pattern, consider Figure 3.10 that plots the average choice deviation from the target for each possible target value, disaggregated by treatment. Strikingly, for all but one target level, the treatment pattern exhibits this ‘anti-substitutability’ tendency of the conservative value being larger than the liberal one. In other words, for the same target, parents on average want to shape their children *more* conservatively (liberally) if their child’s friends and school are also conservative (liberal). We can also visually see that the significant differences exist only for participants with (highly) liberal targets (as also reported in Table 3.A.7).

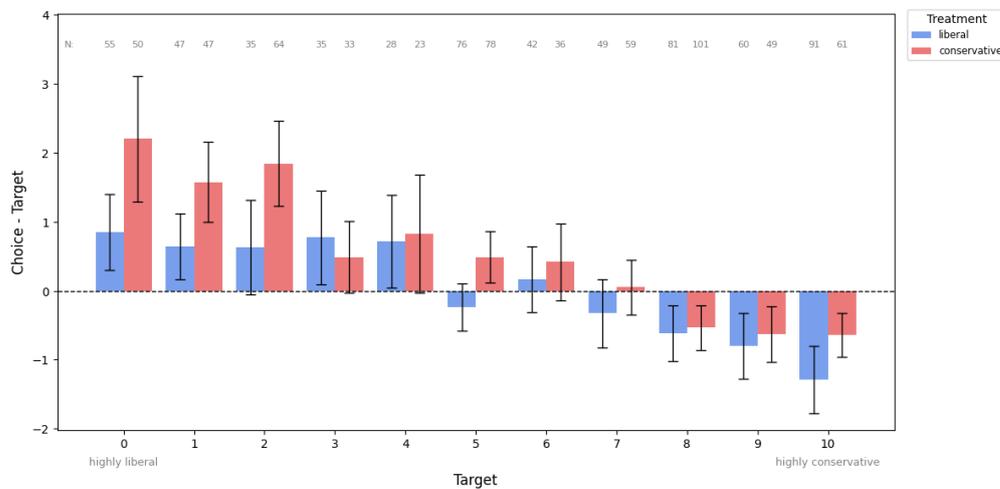
For completeness, Figure 3.A.13 plots the analogous figure for substitution intensity outcome. The data reveal a generally U-shaped relationship (i.e., parents with more extreme targets wanting to shape more intensely), but treatment differences are small and statistically insignificant across all target levels.

Overall, we conclude that our data provides clear evidence against the existence of a substitutability effect. If anything, our results point toward a complementarity effect, although this is statistically significant and sizable in magnitude only for the subsample with liberal targets.

### 3.5 Conclusion

This chapter provides novel empirical evidence on how parents think about the intergenerational transmission of cultural traits. Drawing on a large-scale, pre-registered survey of 1,200 US parents (50% male and female), we collect descriptive and experimental data to better understand the beliefs, preferences, and strategies that underlie cultural transmission within families.

38. For conservatives, we find that  $p \approx 0.35$  with coefficient  $\approx 0.06$  SD.



**Figure 3.10.** Substitutability: Choice Deviation From Target, by Target and Treatment

*Note:* This figure shows the average deviation between the parent's chosen political position in the transmission to their child and the parent's stated target (i.e.,  $SubstChoice$  minus  $SubstTarget$ ) on the y-axis. Positive values indicate that the chosen position is more conservative than the target, and negative values indicate a more liberal choice. Results are plotted separately for each possible target value and disaggregated by treatment condition (liberal vs. conservative peer/school environment). Bars represent group means with 95% confidence intervals. The gray numbers at the top indicate the number of respondents in each group. The consistently higher values in the *conservative* condition relative to the *liberal* condition weigh against the substitutability hypothesis.

In the first part of the study, we present detailed evidence on how parents perceive the role of different transmitters, the timing of transmission, their preferences for shaping various traits, their confidence in knowing what the optimal levels of these traits are, their goals for transmission, and the methods they use. Several patterns emerge. First, parents overwhelmingly view themselves as the primary transmitters of traits. Second, they express a strong desire to shape traits such as patience, altruism, and work ethic – traits that have been shown to matter for long-term economic success. Third, they believe that this shaping occurs primarily in early childhood, often before the age of twelve.

The second part of the study tests two assumptions central to leading models of cultural transmission (Bisin and Verdier, 2001, 2011): imperfect empathy and substitutability. We measure imperfect empathy as parents choosing a job for their child that aligns with their own preferences, even when this conflicts with the child's stated preference, and find that approximately 60% of parents exhibit this behavior. This share is remarkably stable across variation in covariates and provides empirical support for the imperfect-empathy assumption widely used in theoretical work. In contrast, we find no support for the substitutability assumption: Parents do not significantly increase their transmission effort when their children are exposed to political values from schools and peers that the parents oppose. Furthermore, if

anything, the findings suggest that parents may even reduce the extremity of their transmission in such environments. However, this tendency is mostly driven by liberal parents.

Taken together, this chapter contributes to the literature in two key ways. First, it offers a large-scale, empirical account of how parents think about the transmission of economically relevant attitudes and character traits. Second, it empirically assesses two central assumptions in the corresponding theoretical literature in the economics of culture.

Future work could extend this research in several directions. First, it could examine heterogeneity in beliefs and transmission strategies across cultural contexts or socio-economic groups. This could be pursued using the rich set of covariates collected in this study or through new data collection in other countries. Second, the robustness of the theoretical tests of Part 2 could be tested in alternative domains. That is, beyond job choice in the case of imperfect empathy, or beyond political attitudes in the case of substitutability.

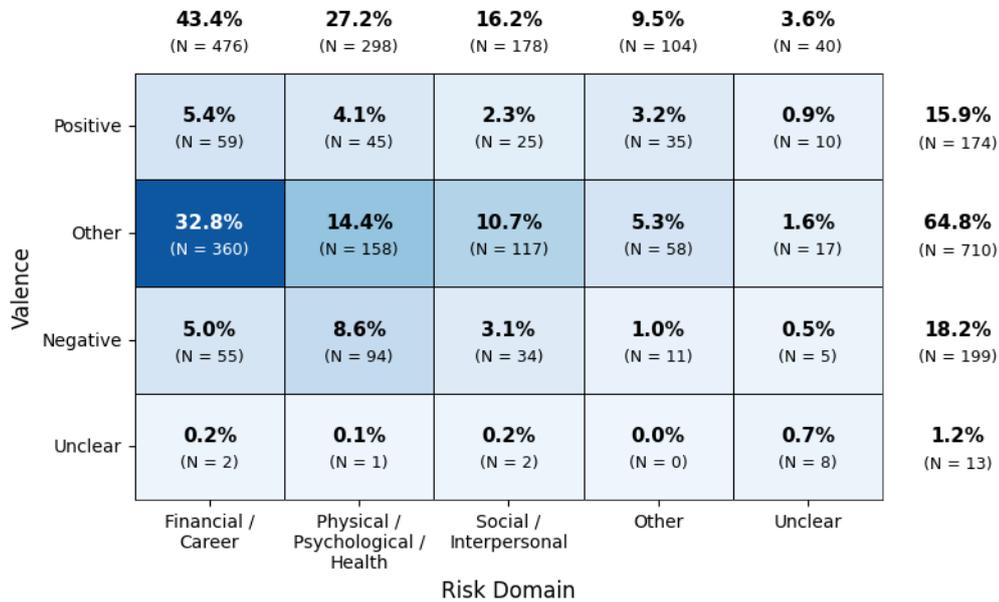
Overall, this chapter serves as a first step toward a better understanding of how parents think about the (non-genetic) transmission of economically relevant attitudes and character traits.

## Appendix 3.A Supplementary Tables and Figures

**Table 3.A.1.** Sample Summary

<b>Binary Demogr. (% Yes)</b>	<b>Sample</b>	<b>Benchmark</b>			
Parent Male	49.8	50			
Child Male	59.9	50			
Parent–Child Same Gender	54.3	50			
Income Above 75k	64.4	57.3			
College or Above	85.8	64.2			
Born in US	92.5	84.0			
Urban/City	47.7	30 / 29.3–64.1			
In Relationship with Other Parent	79.4	–			
Private School (Child)	38.0	9–10			
<b>Birth Year</b>	<b>Min</b>	<b>P25</b>	<b>Mean</b>	<b>P75</b>	<b>Max</b>
Birth Year Parent	1938	1974	1983.5	1994	2007
Birth Year Child	1965	2003	2009.4	2019	2025
<b>Simplified Ethnicity (%)</b>	<b>Sample</b>	<b>Benchmark</b>			
White (Non-Hispanic)	63.6	68.8			
Black	26.7	10.0			
Hispanic and Other	9.8	21.2			
<b>Region (%)</b>	<b>Sample</b>	<b>Benchmark</b>			
South	44.8	37.1			
Northeast	19.3	18.1			
West	18.6	23.8			
Midwest	17.3	21.0			
<b>Political Orientation (%)</b>	<b>Sample</b>	<b>Benchmark</b>			
Conservative	48.8	37.0			
Liberal	31.3	25.0			
Moderate	19.8	34.0			
<b>Parenting Style (%)</b>	<b>Sample</b>	<b>Benchmark</b>			
Authoritative	64.3	52.0			
Permissive	24.9	18.0			
Authoritarian	9.8	26.0			
Neglectful / Other	1.0	12.0			

*Notes:* This table summarizes the composition of the sample and compares it to national benchmarks where available. Benchmarks for ethnicity, income, education, nativity, region, and the Urban/City interval (29.3–64.1) are from IPUMS (<https://www.ipums.org/>). For Urban/City, IPUMS includes many responses for which it is unclear whether they live in city or suburban areas; the 30% benchmark comes from Pew Research Center (<https://www.pewresearch.org/social-trends/2018/05/22/demographic-and-economic-trends-in-urban-suburban-and-rural-communities/>). Political orientation data are from Gallup (<https://news.gallup.com/poll/655190/political-parties-historically-polarized-ideologically.aspx>). Parenting style benchmarks are from YouGov (<https://today.yougov.com/society/articles/45908-what-americans-think-about-parenting-styles-poll>), noting that their benchmark allows multiple responses whereas our survey did not. Private school enrollment benchmarks are from the National Center for Education Statistics (<https://nces.ed.gov/programs/coe/indicator/cgc>). Birth year statistics reflect the age distribution of the sample. All links were last accessed on September 25, 2025.



**Figure 3.A.1.** Risk Understanding by Valence and Domain (Normalized by Number of Responses)

Note: This figure complements Figure 3.1 by reporting the same data normalized by the number of responses rather than respondents. Each cell shows the percentage of all domain mentions falling into that valence-domain combination. *Valence* was single-choice, while *Risk Domain* allowed for multiple selections (unless *Unclear* was chosen). Because participants could select multiple domains, the denominator here is the total number of domain responses (rather than individuals), and each row sums to 100%. A research assistant coded participants' responses how they interpreted 'risk attitude' in the context of the survey.

**Table 3.A.2.** DeFacto Transmission Average Values (Risk Subsample)

Trait	Parents	Other Relatives	Friends/ Peer Group	Schools/ Teachers	Soc Media/ Influencers	N
Risk (All)	0.54 [0.32, 0.76]	0.16 [-0.01, 0.33]	1.90 [1.74, 2.06]	0.40 [0.23, 0.57]	1.63 [1.49, 1.77]	681
Risk (Subsample)	0.77 [0.51, 1.03]	0.23 [0.03, 0.43]	2.01 [1.83, 2.20]	0.61 [0.40, 0.81]	1.74 [1.58, 1.91]	476

Notes: This table complements Table 3.3 and reports the results for the trait risk attitude, including both the full sample and the subsample of respondents who mentioned financial/career risks in the risk-understanding question. As in the baseline table, beliefs are elicited on a scale from 0 ('not at all') to 4 ('extremely'), along with a direction variable where positive values indicate a belief that the transmitter makes the child more risk-seeking. Displayed are means, with 95% confidence intervals shown in gray.

**Table 3.A.3.** DeFacto Transmission Average Values (All By Political Groups)

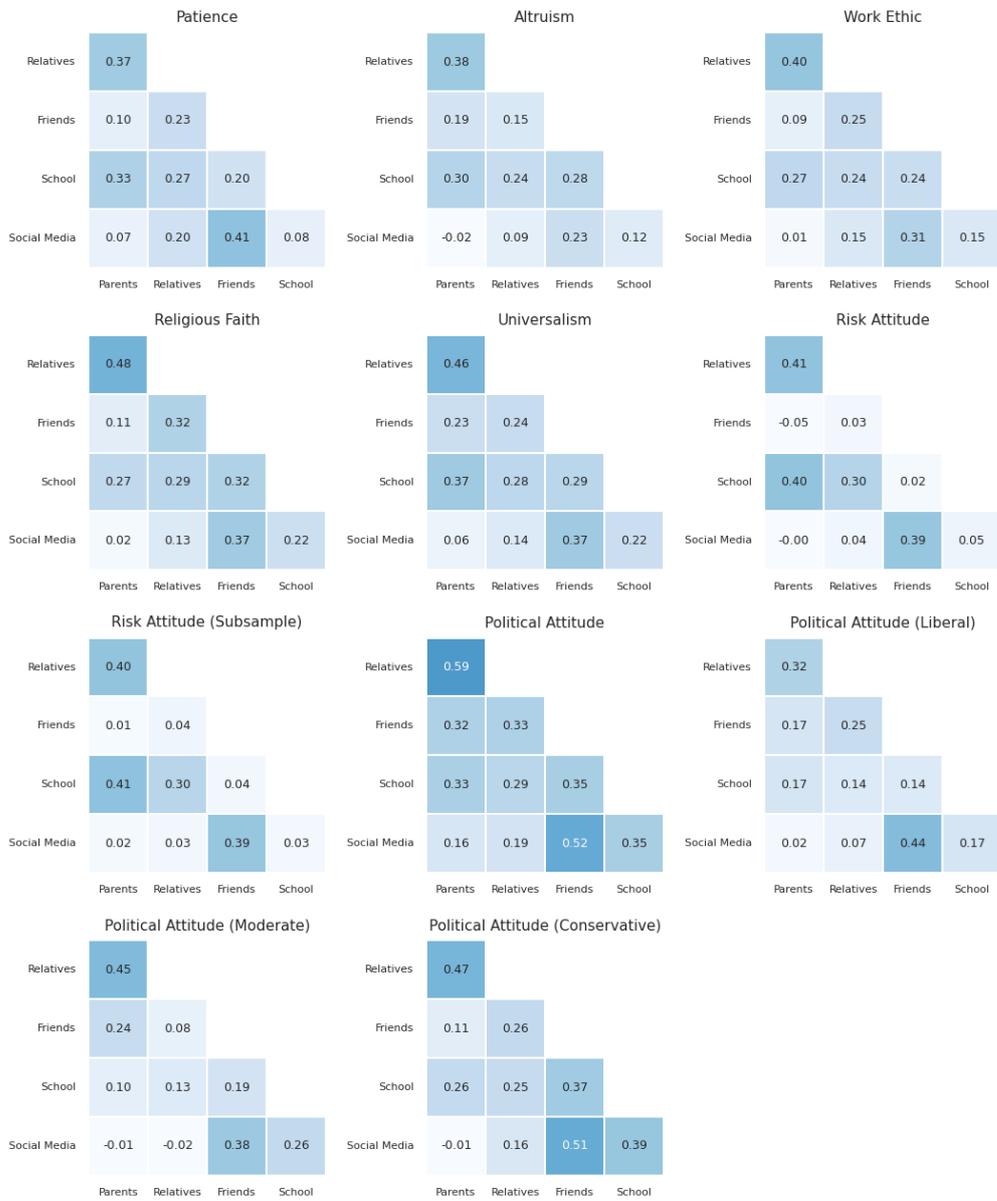
Trait	Parents	Other Relatives	Friends/ Peer Group	Schools/ Teachers	Soc Media/ Influencers	N
<b>Liberal</b>						
Pat	2.86 [2.63, 3.09]	1.68 [1.46, 1.89]	0.64 [0.30, 0.98]	2.33 [2.11, 2.55]	-0.04 [-0.30, 0.22]	208
Alt	3.01 [2.82, 3.20]	1.55 [1.30, 1.80]	1.28 [0.98, 1.58]	1.90 [1.65, 2.14]	0.45 [0.20, 0.70]	211
Work	3.15 [2.99, 3.32]	1.79 [1.56, 2.01]	1.09 [0.76, 1.42]	2.48 [2.27, 2.69]	0.37 [0.10, 0.64]	214
Rel	2.10 [1.72, 2.48]	1.80 [1.55, 2.06]	0.32 [-0.03, 0.63]	1.00 [0.74, 1.26]	0.02 [-0.23, 0.27]	205
Univ	2.63 [2.40, 2.87]	1.53 [1.29, 1.77]	1.83 [1.57, 2.08]	2.00 [1.78, 2.23]	1.05 [0.78, 1.32]	232
Risk	0.22 [-0.18, 0.61]	-0.01 [-0.30, 0.28]	2.14 [1.87, 2.40]	0.17 [-0.13, 0.47]	1.59 [1.36, 1.82]	218
<b>Moderate</b>						
Pat	2.60 [2.27, 2.92]	1.20 [0.92, 1.47]	0.18 [-0.21, 0.56]	2.01 [1.75, 2.28]	-0.18 [-0.48, 0.11]	136
Alt	2.62 [2.33, 2.92]	1.55 [1.29, 1.80]	0.82 [0.45, 1.19]	1.76 [1.47, 2.04]	0.29 [-0.01, 0.58]	143
Work	3.01 [2.77, 3.26]	1.58 [1.29, 1.87]	0.52 [0.12, 0.92]	2.05 [1.73, 2.37]	0.07 [-0.24, 0.38]	136
Rel	2.03 [1.58, 2.48]	1.48 [1.16, 1.80]	0.35 [0.01, 0.70]	0.65 [0.34, 0.95]	-0.20 [-0.47, 0.07]	141
Univ	1.93 [1.51, 2.35]	0.98 [0.62, 1.35]	1.11 [0.71, 1.51]	1.50 [1.16, 1.84]	1.02 [0.68, 1.36]	130
Risk	-0.03 [-0.53, 0.47]	-0.36 [-0.69, -0.02]	1.86 [1.51, 2.21]	0.02 [-0.31, 0.36]	1.53 [1.25, 1.82]	129
<b>Conservative</b>						
Pat	2.90 [2.73, 3.07]	1.70 [1.52, 1.87]	0.54 [0.27, 0.81]	2.27 [2.08, 2.46]	0.06 [-0.17, 0.28]	343
Alt	2.90 [2.72, 3.08]	1.65 [1.43, 1.86]	1.00 [0.73, 1.27]	1.85 [1.64, 2.06]	0.35 [0.12, 0.58]	328
Work	3.07 [2.92, 3.21]	1.67 [1.45, 1.89]	1.09 [0.81, 1.36]	2.37 [2.18, 2.57]	0.08 [-0.17, 0.32]	329
Rel	3.16 [3.00, 3.31]	2.19 [2.00, 2.37]	1.09 [0.84, 1.36]	1.18 [0.95, 1.41]	-0.16 [-0.38, 0.06]	327
Univ	1.92 [1.66, 2.18]	1.24 [1.00, 1.49]	1.36 [1.12, 1.60]	1.81 [1.57, 2.05]	1.02 [0.78, 1.27]	328
Risk	0.98 [0.67, 1.29]	0.48 [0.23, 0.73]	1.77 [1.53, 2.01]	0.70 [0.44, 0.95]	1.69 [1.47, 1.90]	341

Notes: This table complements Table 3.3 and reports the results separately by subjects' self-reported political leaning. Political leaning was measured on an 11-point scale from 0 ('highly liberal') to 10 ('highly conservative') and categories were defined as follows: 0–3 = Liberal, 4–6 = Moderate, 7–10 = Conservative. As in the baseline table, beliefs are elicited on a scale from 0 ('not at all') to 4 ('extremely') and multiplied by a binary direction variable (-1 for decrease and +1 for increase; for risk attitude, the increase referred to 'more risk-seeking'). Displayed are means, with 95% confidence intervals shown in gray.

**Table 3.A.4.** DeFacto Transmission Average Values (Only Magnitude, Disregard Direction)

Trait	Parents	Other Relatives	Friends/ Peer Group	Schools/ Teachers	Soc Media/ Influencers	N
Pat	3.21 [3.15, 3.27]	2.09 [2.02, 2.16]	2.33 [2.26, 2.41]	2.63 [2.56, 2.71]	1.59 [1.50, 1.68]	687
Alt	3.18 [3.11, 3.25]	2.22 [2.14, 2.30]	2.40 [2.33, 2.47]	2.42 [2.35, 2.50]	1.64 [1.55, 1.72]	682
Work	3.28 [3.22, 3.33]	2.29 [2.21, 2.36]	2.42 [2.35, 2.50]	2.75 [2.67, 2.82]	1.70 [1.61, 1.79]	679
Rel	3.33 [3.26, 3.40]	2.44 [2.36, 2.52]	2.12 [2.04, 2.21]	1.85 [1.75, 1.94]	1.52 [1.43, 1.61]	697
Univ	2.97 [2.90, 3.04]	2.22 [2.15, 2.30]	2.56 [2.49, 2.63]	2.44 [2.36, 2.51]	2.07 [1.98, 2.16]	690
Risk	2.80 [2.73, 2.88]	1.98 [1.90, 2.05]	2.66 [2.59, 2.74]	2.03 [1.95, 2.11]	2.17 [2.08, 2.26]	681
Pol (L)	2.92 [2.77, 3.06]	2.01 [1.87, 2.15]	2.54 [2.40, 2.67]	1.98 [1.84, 2.13]	2.18 [2.01, 2.35]	216
Pol (M)	2.59 [2.41, 2.77]	1.74 [1.57, 1.90]	2.22 [2.04, 2.40]	1.75 [1.55, 1.96]	1.93 [1.71, 2.14]	137
Pol (C)	2.77 [2.66, 2.88]	2.18 [2.07, 2.29]	2.49 [2.38, 2.61]	2.20 [2.08, 2.33]	2.33 [2.20, 2.45]	331

Notes: This table complements Table 3.3 and reports the results ignoring the direction of the de facto belief. That is, it focuses only on the perceived magnitude of influence – how strongly parents believe a given transmitter shapes the trait – regardless of whether the influence is seen as increasing or decreasing the trait. Beliefs were elicited on a 0 ('not at all') to 4 ('extremely') scale. Displayed are means, with 95% confidence intervals shown in gray. Results for political attitude are again reported separately for self-identified liberals, moderates, and conservatives, based on an 11-point political leaning scale (0 'highly liberal' to 10 'highly conservative'), with categories defined as follows: 0–3 = Liberal, 4–6 = Moderate, 7–10 = Conservative.



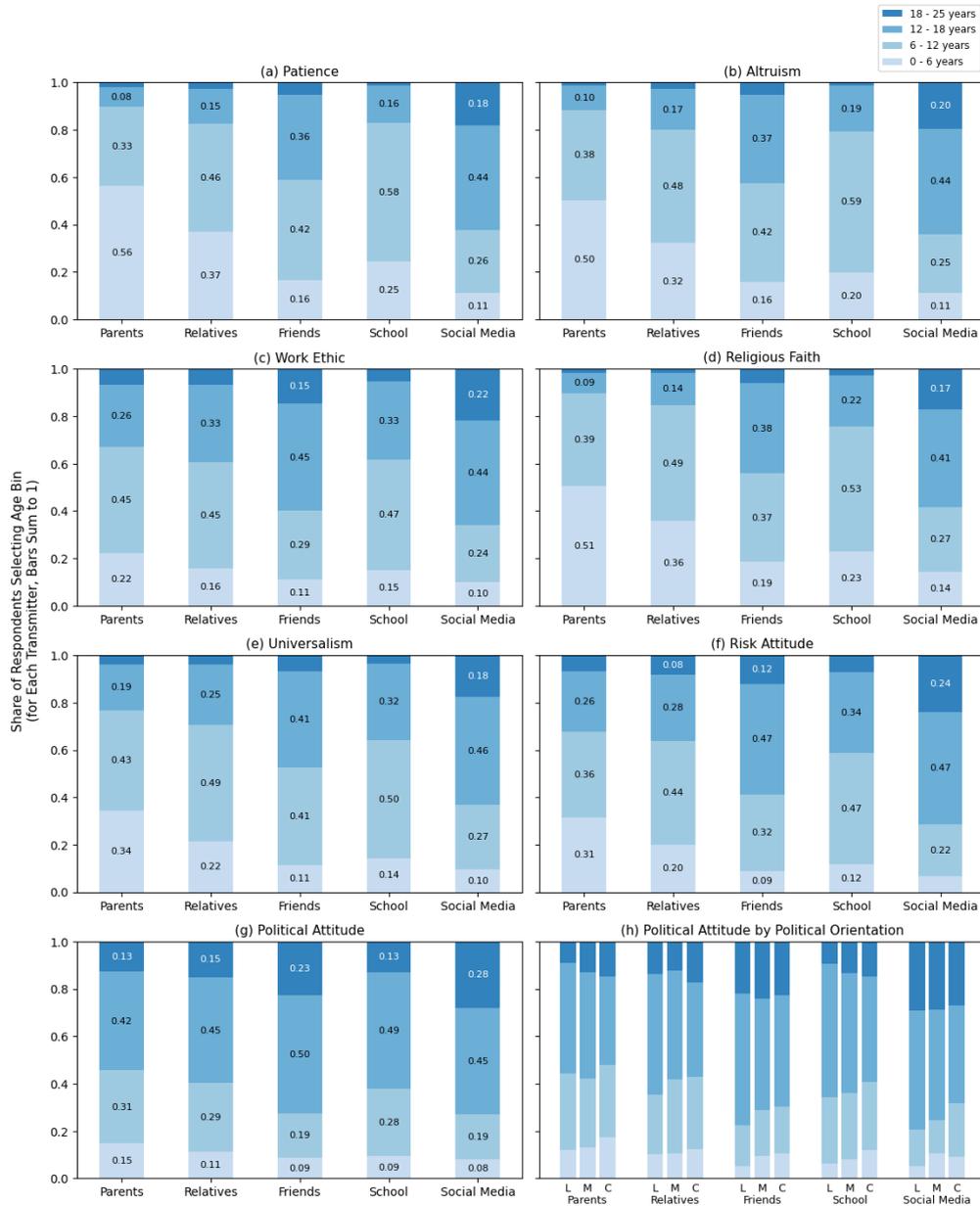
**Figure 3.A.2.** Correlation of DeFacto Signed Beliefs Across Transmitters

Note: This figure displays pairwise Spearman rank correlations of participants’ signed beliefs about the strength and direction with which each transmitter de facto shapes individual traits, holding the trait constant. Each panel represents one trait, with diagonal and upper-triangle values omitted for clarity. Political attitude correlations are shown both pooled for all respondents and separately by political subgroup (liberal, moderate, conservative).



**Figure 3.A.3.** Correlation of DeFacto Signed Beliefs Across Traits

*Note:* This figure displays pairwise Spearman rank correlations of participants' signed beliefs about the strength and direction with which each trait is de facto shaped by a given transmitter, holding the transmitter constant. Each panel represents one transmitter, with diagonal and upper-triangular values omitted for clarity. Political attitude is omitted as including it, separating by liberal, moderate, and conservative subgroups, would substantially increase the number of comparisons.



**Figure 3.A.4.** Transmission Time (Unweighted)

*Note:* This figure complements Figure 3.2 and shows the ages at which participants believe each trait is predominantly shaped, without applying any weighting for perceived importance. This question was answered for each transmitter they believed shaped the trait at least slightly (i.e., not ‘not at all’). For each trait, a stacked bar represents the percentage distribution of responses across the four age bins: 0–6, 6–12, 12–18, and 18–25 years. As each bar reflects raw response frequencies, the segments sum to 100%.

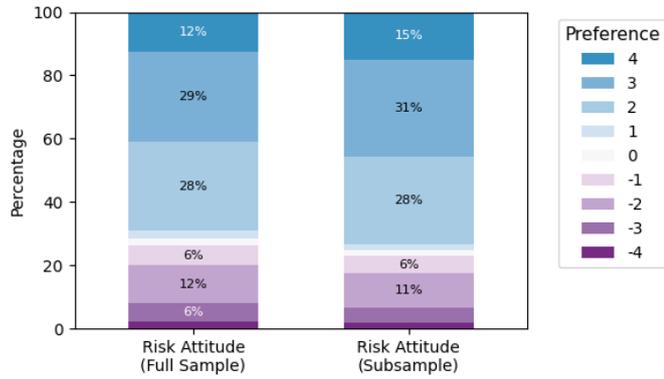


Figure 3.A.5. Preference Risk Subset

Note: This figure complements Figure 3.3 and shows how strongly participants want to shape their child's risk attitude, conditional on believing they de facto shape it at least slightly. The two bars display results for the full sample and for the subsample of participants who mentioned financial/career risks in the risk-understanding question. Responses were given on a 5-point Likert scale (from 'not at all' to 'extremely') along with a binary direction: negative values indicate a preference for making their child more risk-averse, positive values for more risk-seeking. Percentages within each bar reflect the distribution of signed preferences.

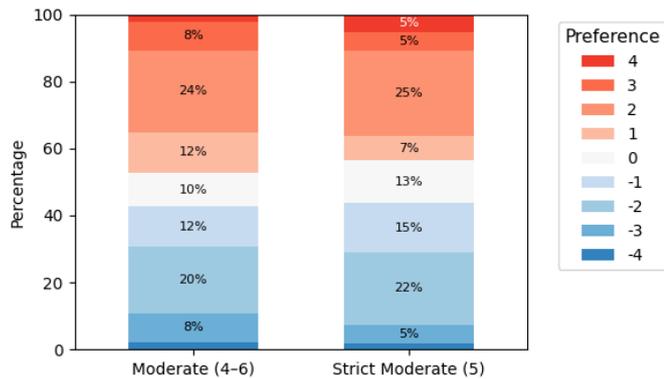
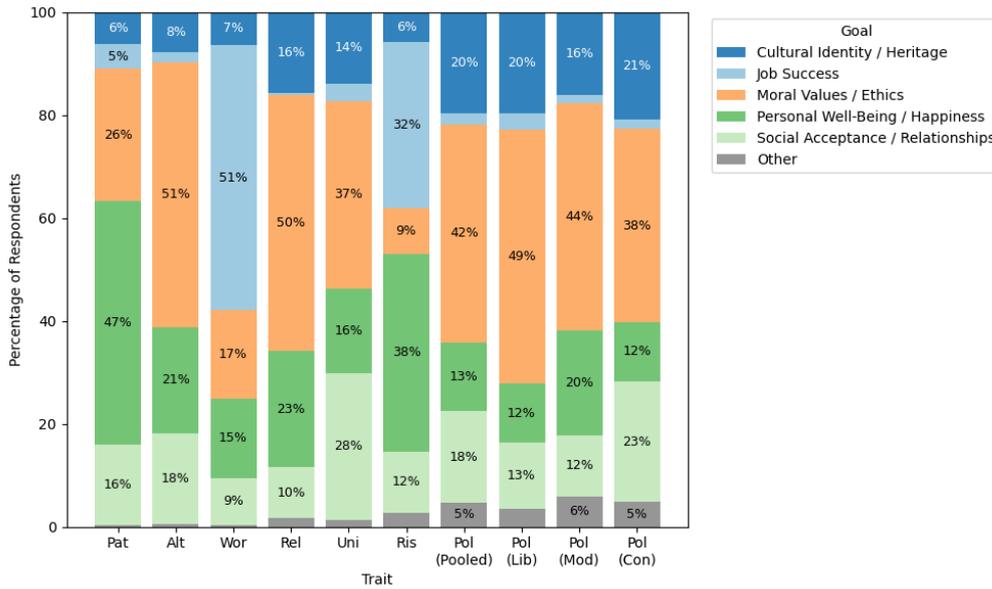


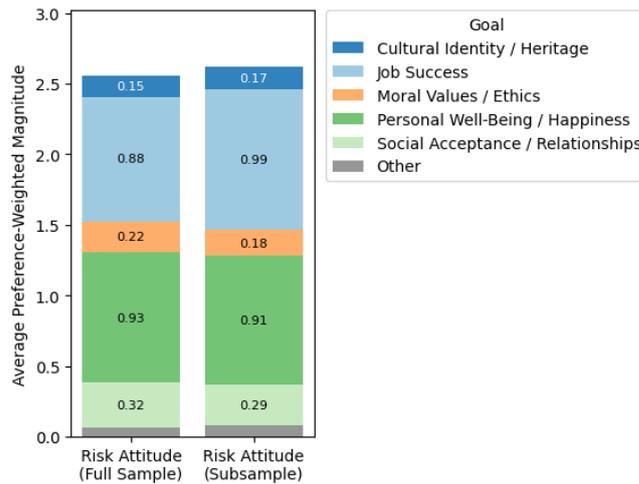
Figure 3.A.6. Preference Political (Strict Moderates)

Note: This figure complements Figure 3.4 and shows transmission preferences for political attitude among self-identified moderates. Responses were given on a 5-point Likert scale (from 'not at all' to 'extremely') along with a binary direction: negative values indicate a preference for more liberal attitudes, positive values for more conservative ones. Results are shown separately for the standard moderate category (values 4-6 on the 0-10 political self-placement scale) and for a stricter definition (value 5 only). Percentages within each bar reflect the distribution of signed preferences.



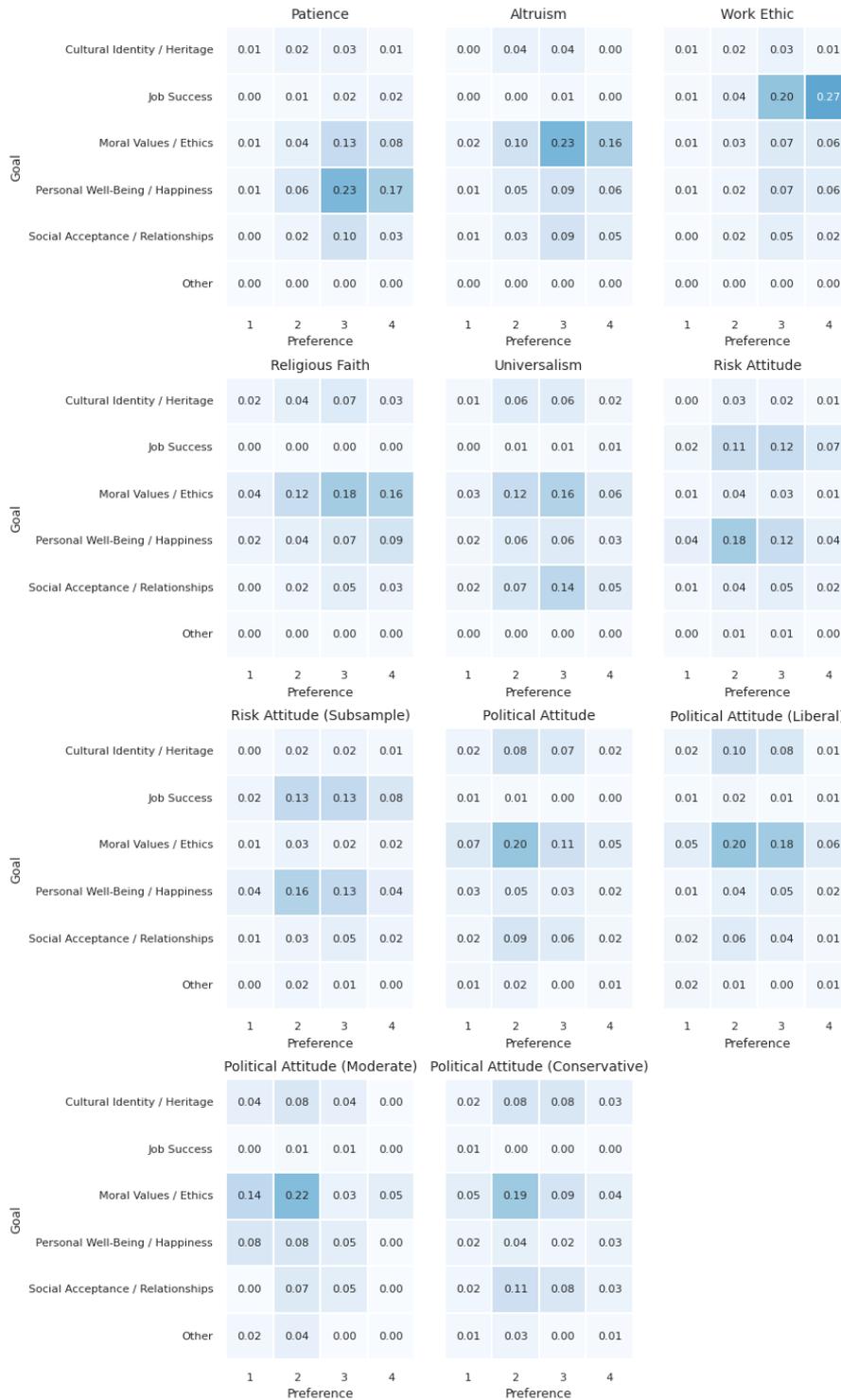
**Figure 3.A.7.** Transmission Goal Relative Frequencies

Note: This figure complements Figure 3.6 and shows the distribution of participants' stated goals for shaping each trait. Participants selected one goal for each trait they wanted to shape at least slightly. Bars reflect the relative frequency of each goal category within trait. In contrast to Figure 3.6, responses are unweighted, i.e., each bar summing to 100%. For political attitude, results are shown for the full sample pooled and separately by political orientation (liberal, moderate, conservative).



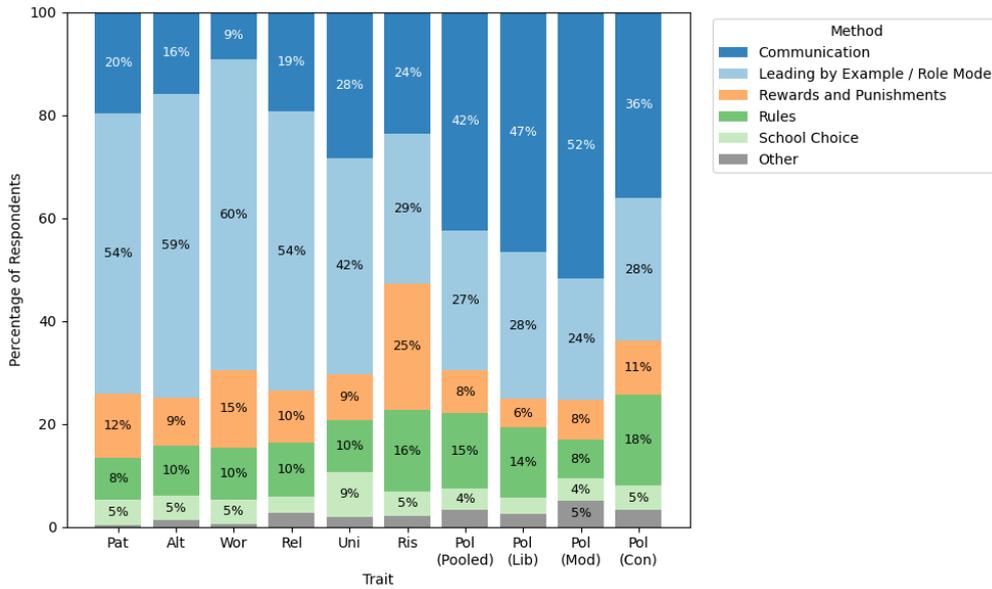
**Figure 3.A.8.** Transmission Goal (Risk Subsample)

Note: This figure complements Figure 3.6 and shows the average preference-weighted distribution of transmission goals for risk attitude, separately for the full sample and the subsample of participants who mentioned financial/career risks in the risk-understanding question.



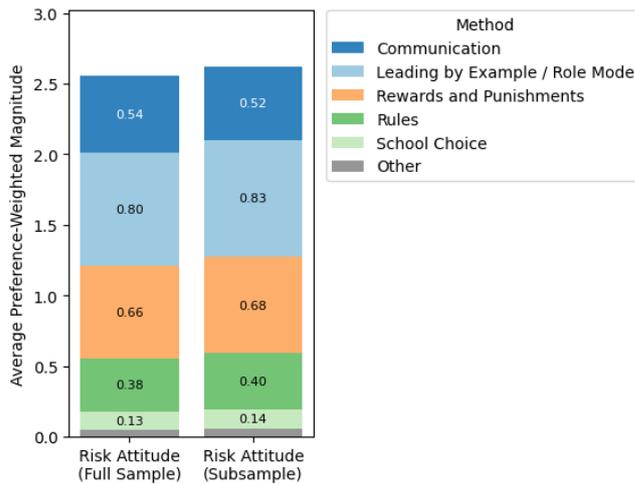
**Figure 3.A.9.** Goal-Preference Frequencies

Note: This figure displays the joint distribution of the transmission goal and the magnitude of the transmission preference for each trait. Each panel corresponds to a trait and shows the frequency with which each goal-preference combination was reported, conditional on participants wanting to shape the trait at least slightly. Rows correspond to goal categories; columns indicate the intensity of transmission preference (from 1 to 4). For political attitude, the results are shown for the full sample pooled and separately by political orientation (liberal, moderate, conservative).



**Figure 3.A.10.** Transmission Method Relative Frequencies

Note: This figure complements Figure 3.7 and shows the distribution of participants' stated methods for shaping each trait. Participants selected one method for each trait they wanted to shape at least slightly. Bars reflect the relative frequency of each goal category within trait. In contrast to Figure 3.7, responses are unweighted, with each bar summing to 100%. For political attitude, the results are shown for the full sample pooled and separately by political orientation (liberal, moderate, conservative).



**Figure 3.A.11.** Transmission Method (Risk Subsample)

Note: This figure complements Figure 3.7 and shows the average preference-weighted distribution of transmission method for risk attitude, separately for the full sample and the subsample of participants who mentioned financial/career risks in the risk-understanding question.

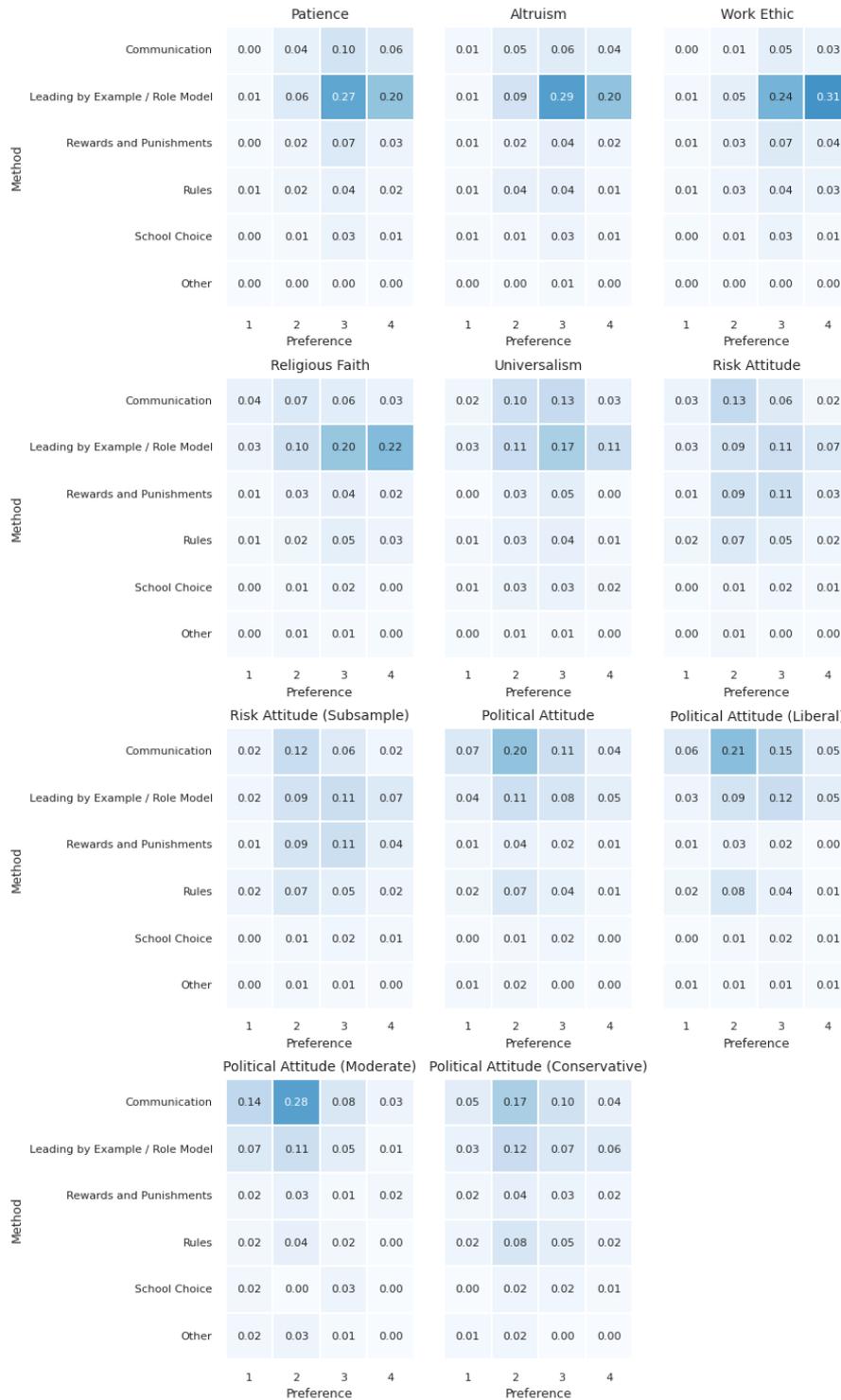


Figure 3.A.12. Method-Preference Frequencies

Note: This figure displays the joint distribution of the transmission method and the magnitude of the transmission preference for each trait. Each panel corresponds to a trait and shows the frequency with which each method-preference combination was reported, conditional on participants wanting to shape the trait at least slightly. Rows correspond to method categories; columns indicate the intensity of transmission preference (from 1 to 4). For political attitude, the results are shown for the full sample pooled and separately by political orientation (liberal, moderate, conservative).

**Table 3.A.5.** Imperfect Empathy: OLS and Logit Regression

	OLS	Logit
Intercept	0.4334 (0.0920)	-0.2660 (0.3890)
ImpEmpParentChoice[Job2]	-0.0326 (0.0295)	-0.1399 (0.1240)
ImpLifeCycleRelevanceDemeaned	-0.0054 (0.0070)	-0.0235 (0.0300)
ImpEmpWorldChangeRelevanceDemeaned	-0.0090 (0.0073)	-0.0388 (0.0320)
Ethnicity[White]	0.0121 (0.0353)	0.0534 (0.1470)
Ethnicity[Hispanic/Other]	-0.0121 (0.0578)	-0.0507 (0.2340)
PoliticalSelf[moderate]	0.0017 (0.0485)	0.0039 (0.2050)
PoliticalSelf[liberal]	-0.0050 (0.0590)	-0.0251 (0.2500)
ChildTarget[liberal]	0.0182 (0.0566)	0.0800 (0.2390)
ChildTarget[neutral]	-0.0440 (0.0543)	-0.1842 (0.2290)
ParentingStyle[Authoritative]	-0.0557 (0.0479)	-0.2487 (0.2180)
ParentingStyle[Neglectful]	0.1314 (0.1606)	0.6167 (0.8650)
ParentingStyle[Other]	0.0835 (0.2340)	0.4072 (1.1980)
ParentingStyle[Permissive]	-0.0584 (0.0531)	-0.2592 (0.2390)
GenderParent[Male]	-0.0247 (0.0303)	-0.1062 (0.1290)
GenderChild[Male]	-0.0262 (0.0297)	-0.1121 (0.1260)
ParentChildSameGender	0.0433 (0.0295)	0.1848 (0.1260)
BornInUSA	0.1355 (0.0584)	0.5633 (0.2390)
EducationAtLeastCollege	0.1074 (0.0456)	0.4548 (0.1910)
IncomeAbove75k	0.0199 (0.0318)	0.0844 (0.1350)
UrbanArea	-0.0408 (0.0308)	-0.1742 (0.1300)
ChildPrivateSchool	0.0327 (0.0326)	0.1400 (0.1370)

Table 3.A.5 – continued from previous page

	OLS	Logit
ParentBirthYearDemeaned	-0.0014 (0.0019)	-0.0059 (0.0080)
ChildBirthYearDemeaned	-0.0008 (0.0023)	-0.0034 (0.0090)
ParentalInvolvementGeneralDemeaned	-0.0058 (0.0123)	-0.0251 (0.0520)
ParentalInvolvementShapingDemeaned	-0.0036 (0.0126)	-0.0149 (0.0540)
ParentChildRelationDemeaned	0.0107 (0.0098)	0.0458 (0.0420)
LifeImportantFamilyDemeaned	-0.0027 (0.0370)	-0.0115 (0.1620)
LifeImportantFriendsDemeaned	-0.0098 (0.0221)	-0.0420 (0.0970)
LifeImportantLeisureDemeaned	0.0150 (0.0235)	0.0635 (0.1000)
LifeImportantPoliticsDemeaned	0.0006 (0.0188)	0.0042 (0.0820)
LifeImportantWorkDemeaned	-0.0323 (0.0222)	-0.1418 (0.0970)
LifeImportantReligionDemeaned	0.0280 (0.0159)	0.1203 (0.0680)
Observations	1189	1189
$R^2$ / Pseudo- $R^2$	0.034	0.025

Notes: This table reports the results from regressions (OLS and Logit) of the imperfect-empathy measure *paternalistic* on a broad set of covariates. The goal is to assess whether being classified as exhibiting imperfect empathy is systematically associated with observable characteristics. Only two covariates – having at least a college degree and being born in the US – are significantly associated with *paternalistic* at the  $p < 0.05$  level in both the OLS and Logit models. However, both covariates are strongly imbalanced in the sample, so we interpret these associations with caution. Standard errors are reported in parentheses (OLS uses robust HC1 standard errors). Coefficients for categorical variables are interpreted relative to the omitted reference categories.

**Table 3.A.6.** Substitutability Intensity (Political Subsamples)

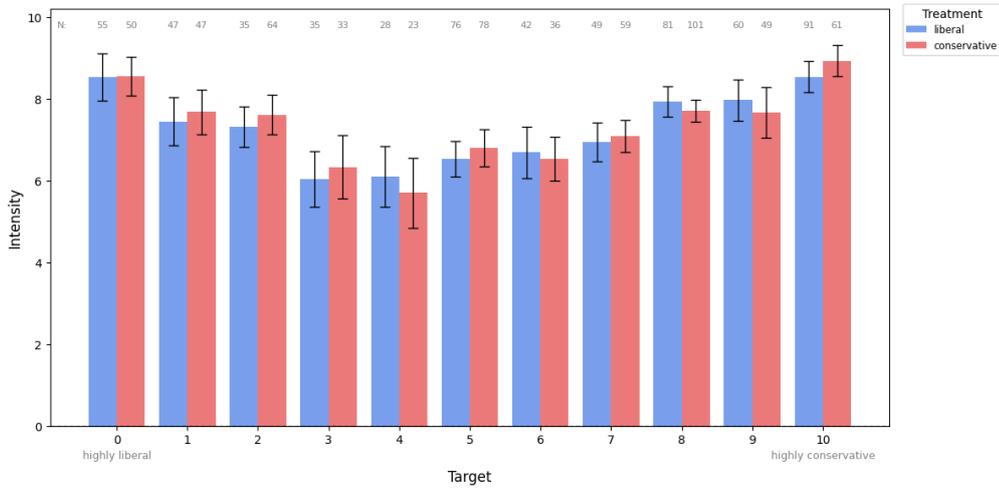
	Liberal Targets		Conservative Targets	
Misaligned	0.1731 (0.1947)	0.1637 (0.2103)	0.0054 (0.1399)	0.0289 (0.1412)
SubstTarget	-0.6647 (0.0742)	-0.5673 (0.0862)	0.5030 (0.0541)	0.4606 (0.0589)
Constant	8.3950 (0.1940)	8.4874 (0.8628)	3.5962 (0.4449)	3.9461 (0.6411)
Controls		✓		✓
Outcome SD	2.172	2.174	1.868	1.851
R <sup>2</sup>	0.169	0.235	0.127	0.207
p (Treatment)	0.374	0.423	0.969	0.837
Observations	417	412	629	626

Notes: This table reports the results of Table 3.5 separately for parents with liberal and conservative target values for their children (*SubstTarget*). That is, it reports OLS regression estimates with *SubstIntensity* (parental transmission effort) as outcome. *Misaligned* is binary and equal to 1 if and only if parents' target for the child's political leaning (*SubstTarget*) and the treatment (hypothetical leaning of child's school and friends) are misaligned. Robust standard errors (HC1) in parentheses. Columns (2) and (4) include a large number of controls (see Table 3.A.5 for the full list).

**Table 3.A.7.** Substitutability Extremeness (Political Subsamples)

	Liberal Targets		Conservative Targets	
Misaligned	-0.3903 (0.1220)	-0.4281 (0.1247)	-0.0901 (0.0977)	-0.0950 (0.0997)
SubstTarget	-0.6985 (0.0472)	-0.5909 (0.0555)	0.6655 (0.0383)	0.6077 (0.0486)
Constant	4.2965 (0.1142)	3.9193 (0.5816)	-2.4308 (0.3227)	-1.6205 (0.5343)
Controls		✓		✓
Outcome SD	1.571	1.572	1.515	1.516
R <sup>2</sup>	0.370	0.438	0.337	0.379
p (Treatment)	0.001	0.001	0.356	0.341
Observations	417	412	629	626

Notes: This table reports the results of Table 3.6 separately for parents with liberal and conservative target values for their children (*SubstTarget*). That is, it reports OLS regression estimates with *SubstExtremeness* (extremeness of parental transmission) as outcome. *Misaligned* is binary and equal to one if and only if parents' target for the child's political leaning (*SubstTarget*) and the treatment (hypothetical leaning of child's school and friends) are misaligned. Robust standard errors (HC1) in parentheses. Columns (2) and (4) include a large number of controls (see Table 3.A.5 for the full list).



**Figure 3.A.13.** Substitutability Intensity, by Target Preference and Treatment

*Note:* This figure plots average values of the stated effort with which parents want to shape their child's political orientation in the substitutability scenario (*SubstIntensity*) on the *y*-axis. Values are measured on a 0–10 scale (0 'no effort at all,' 10 'maximum effort'), grouped by the parent's target for the child's political ideology (*x*-axis), and shown separately for the liberal and conservative treatment conditions. Bars reflect group means with 95% confidence intervals. The gray numbers at the top indicate the number of respondents in each group. The figure reveals a broadly U-shaped pattern, with parents who hold more extreme targets expressing stronger shaping efforts. Treatment differences are small and not statistically significant at any target level.

## **Appendix 3.B Additional Information**

### **3.B.1 Instructions**

---

Start of Block: Intro

**Welcome!**

This study is designed for computer (PC or Mac) users only (desktop, laptop, etc.). If you are accessing this study on a smartphone, a tablet or any other non-computer devices, please switch to a computer and enter the study again, or return the submission on Prolific.

This study is about how **attitudes or character traits are shaped in children, excluding genetic inheritance.**

For example, this is about how **teachers or peer groups shape children's attitudes or character traits.**

Or about **how parents shape children's attitudes or character traits, but excluding genetic inheritance.**

That is, in the entire study we want you to think only about non-genetic shaping.

Page Break

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All your answers will be anonymized. This means that we have no way of linking your responses to your identity.

Importantly, there are often no right or wrong answers in this survey.

We are simply interested in your opinion.

**Therefore, it is important that you please answer honestly.**

---

End of Block: Intro

Start of Block: ProlificID

**What is your Prolific ID?**

Note that this response should auto-fill with the correct ID.

---

End of Block: ProlificID

Start of Block: NumberChildren

How many children do you have?

0	1	2	3	4	5	> 5
---	---	---	---	---	---	-----

*[If "0" was selected, survey ends.]*

End of Block: NumberChildren

---

Start of Block: DataConsent

### Data Protection and Informed Consent

*Please consent to the processing of your data and our privacy policy.*

To display the full privacy policy click here

*[if clicked:]*

#### Who is responsible for the study?

Responsible for the execution of the study and therefore for the processing of your data: Alexander Laubel  
Bonn University  
Regina-Pacis-Weg 3  
53113 Bonn  
Germany

#### What are the purposes of the study?

The purpose of this study is to improve our understanding of human behavior in economic contexts. We are interested in natural, unbiased behavior. Hence, no details on the background of this research project is given beforehand, which is in accordance with the standard in experimental economics. All necessary information will be provided in due time.

#### What happens to my data?

All participating employees and scientists work in accordance with the provisions of the Data Protection Regulation, the Federal Data Protection Act and the relevant State Data Protection Acts. Immediately after the collection, your data will be stored anonymized and then statistically analysed. No conclusions about your person can be drawn from these results. Your data will be stored by a technical service provider outside the EU. We have taken all necessary precautions and concluded all contracts which are necessary in order to comply with data protection. Even if the service provider is not located in the EU, your data is secure as we have concluded contracts with the service provider designated for this purpose in the EU. For this study, we collect "special categories of personal data", in particular data revealing migration background (racial or ethnic origin - term according to Art. 9 GDPR), political opinions, religious or philosophical beliefs or trade union membership, genetic data, health data or data concerning a person's sex life or sexual orientation, which we use only for research purposes and solely with your consent.

#### Declaration of Consent

I hereby consent to the processing of my personal data for this research project for the questions on human behavior. I can withdraw my consent at any time. I have taken note of all information concerning the usage of my data and on my rights in the privacy policy. Your data will be stored and analyzed in full compliance with the highest standards of the data protection laws of the European Union. In particular, no conclusions about your person will be drawn. You can withdraw your consent at any time.

I consent

I do not consent

*[If "I do not consent" was selected, survey ends.]*

End of Block: DataConsent

---

Start of Block: AttentionCheck1

Often, participants click through surveys like this one without reading the instructions carefully. As a consequence, many of the participants can't be used in research. To show us that you are reading the questions carefully, please give the following answers to the question below: "Very interested" and "I've never heard of it".

Based on the text you read above, what have you been asked to answer to the following question:

How interested are you in American Football?

Very interested

A bit interested

Not interested at all

I've never heard of it

End of Block: AttentionCheck

---

Start of Block: DeFactoTransmission

As mentioned before, this study is about how attitudes or character traits are shaped in children, excluding genetic inheritance.

Below, we present a list of attitudes and character traits that are of interest to us. For each one, we provide a brief explanation of what we mean by the term.

*[random subselection of four traits shown.]*

**Patience**

A person with high patience is generally willing to give up something today in order to benefit from that in the future.

**Altruism**

A person with high altruism is generally willing to share with others without expecting anything in return.

**Work Ethic**

A person with high work ethic values hard work and believes that individual effort is crucial for success.

**Political Attitude**

A person with strong political attitude has a clear and staunch set of political opinions (e.g., liberal or conservative).

**Religious Faith**

A person with high religious faith strongly adheres to religious beliefs that guide both worldview and daily life.

**Universalism**

A person with high universalism exhibits the same level of altruism and trust towards strangers as towards members of their own community (e.g., nationality, race, religion).

**Risk Attitude**

A person's risk attitude indicates whether a person is generally willing to take risks or tries to avoid taking risks.

Page Break

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Please state how much you think each attitude or character trait is shaped by the following agents or institutions: parents, other relatives, friends / peer group, schools / teachers, social media / influencers.

**In case you have multiple children, please answer the following questions for your first child.**

This helps us make the answers comparable across respondents.

**How strongly is each of the following attitudes or character traits shaped in your first child by these agents or institutions?**

**Please abstract from the direction of the effect, but indicate only how strong in magnitude it is.**

**For example, "slightly shaping" can mean both "slightly increasing" or "slightly decreasing" a trait.**

Please abstract from genetic shaping.

**Patience**

	not at all	slightly	moderately	highly	extremely
Parents	<input type="radio"/>				
Other Relatives	<input type="radio"/>				
Friends / Peer Group	<input type="radio"/>				
Schools / Teachers	<input type="radio"/>				
Social Media / Influencers	<input type="radio"/>				

*[exemplary for Patience, repeated for all four traits.]*

End of Block: DeFactoTransmission

---

Start of Block: AttentionCheck2

The beverage test you are about to take is very simple, when asked for your favorite beverage you must select "Lipton Ice Tea." This is an attention check.

Based on the text you read above, what is your favorite beverage?

Bud Light

Coca-Cola

Mountain Dew

Gatorade

Lipton Ice Tea

Ocean Spray Juice

*[If both attention checks failed, survey ends.]*

End of Block: AttentionCheck2

---

Start of Block: DeFactoDirection

For each agent or groups of agents that you think shape a certain attitude or character trait at least a little bit, we would like to ask the following:

**In what direction is each of the following attitudes or character traits shaped by these agents or institutions?**

Please abstract from genetic shaping.

### Patience

You said **parents** shape your first child's **patience** [answer]. In what direction do parents shape your first child's patience?

Impatient

Patient

*[exemplary for trait Patience and transmitter Parents; repeated for all four traits and all transmitters for which subjects answered at least 'slightly' for DeFactoTransmission. For the other traits the directions are Non-altruistic vs. Altruistic (Altruism); Low Work Ethic vs. High Work Ethic (Work Ethic); Liberal vs. Conservative (Political Attitude); Non-religious vs. Religious (Religious Faith); Non-universalist vs. Universalist (Universalism); Less willing to take risks vs. More willing to take risks (Risk Attitude). For Political Attitude an explanation was given:]*

### Political Attitude

Here, we would like to know in what direction you think the respective agent or groups of agents shape your first child: liberal vs. conservative.

By liberal, we mean valuing for example:

- Diversity
- Self-expression and individual choice
- Social security and redistribution

By conservative, we mean valuing for example:

- Tradition and established norms

- Discipline and respect for authority
- Personal responsibility for life outcomes

End of Block: DeFactoDirection

---

Start of Block: TransmissionTime

For each agent or groups of agents that you think shape a certain attitude or character trait at least a little bit, we would like to ask the following:

**At what age of your first child is each of the following attitudes or character traits shaped by these agents or institutions?**

If you think multiple apply, please select the answer you think applies most.

Please abstract from genetic shaping.

**Patience**

**You said** parents **shape your first child's** patience **[answer]**. At what age of your first child do you think this happens predominantly?

0 - 6 years

6 - 12 years

12 - 18 years

18 - 25 years

*[exemplary for trait Patience and transmitter Parents; repeated for all four traits and all transmitters for which subjects answered at least 'slightly' for DeFactoTransmission.]*

End of Block: TransmissionTime

---

Start of Block: TransmissionPreference

Now we would like you to **think about what attitudes or character traits you personally want to shape in your first child.**

In the end, it is a personal preference what you want to shape and we are interested in your opinion.

There is no right or wrong.

**For each attitude or character trait, please state how much you want to shape it in a non-genetic way.**

**Please abstract from the direction of the effect, but indicate only how strong in magnitude it is.**

**For example, "slightly shaping" can mean both "slightly increasing" or "slightly decreasing" a trait.**

**Patience**

not at all     slightly     moderately     highly     extremely

*[if choice other than 'not at all' was selected, additional question dynamically popped-up:]*

*In what direction do you want to shape your first child's patience?*

Impatient     Patient

*[exemplary for trait Patience; repeated for all four traits (but only for transmitter Parents), if answered at least 'slightly' for DeFactoTransmission.]*

End of Block: TransmissionPreference

---

Start of Block: TransmissionOptimalLevel

For each attitude or character trait, we would like to additionally know the following:

**How sure are you about the optimal level your first child should have of each attitude or character trait?**

In other words, **how confident are you that you know the "right" amount your first child should ideally possess?**

**Patience**

not at all     slightly     moderately     highly     extremely

*[exemplary for trait Patience; repeated for all four traits.]*

End of Block: TransmissionOptimalLevel

---

Start of Block: TransmissionGoal

For each attitude or character trait that you would like to shape at least a little bit, we would like to additionally know the following:

**Why do you want to shape a certain attitude or character trait of your first child?**

If you think multiple reasons apply, please select the answer you think applies most.

Again, there is no right or wrong. We are simply interested in your personal opinion.

**Patience**

Job Success     Personal Well-Being / Happiness     Moral Values / Ethics     Cultural Identity / Heritage     Social Acceptance / Relationships     Other

*[exemplary for trait Patience; repeated for all four traits and all transmitters for which both DeFactoTransmission and TransmissionPreference was at least 'slightly.']*

End of Block: TransmissionGoal

---

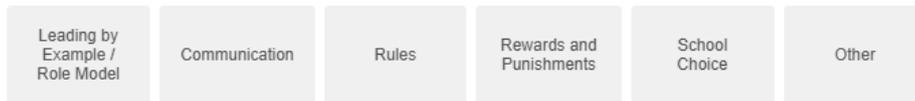
Start of Block: TransmissionMethod

For each attitude or character trait that you would like to shape at least a little bit, we would like to additionally know the following:

**How do you want to shape a certain attitude or character trait of your first child?**

Again, there is no right or wrong. We are simply interested in your personal opinion.

**Patience**



*[exemplary for trait Patience; repeated for all four traits and all transmitters for which both DeFactoTransmission and TransmissionPreference was at least 'slightly.']*

End of Block: TransmissionMethod

---

Start of Block: RiskUnderstanding

*[only if Risk Attitude is among the selected traits, and only for Risk Attitude.]*

Previously, you answered questions about **Risk Attitude**. For example, you answered how much believe it is shaped by certain persons or how much you want to shape it yourself.

When you were answering these questions, what kinds of risks were you thinking about?

Please write down a few key words.

*[60 characters maximum.]*

End of Block: RiskUnderstanding

---

Start of Block: ImperfectEmpathy

Now, we will shift the focus of the questions slightly.

We will provide an example of an important life decision: job choice.

You will then be asked several questions.

Please read the example and the questions carefully, as sometimes only a few words will change.

One of the biggest decisions in life is what kind of job to take.

Below, you find two descriptions of typical jobs.

**Job 1**

This job offers a stable routine, with predictable hours. There is minimal travel involved, so you can spend evenings and weekends with family and friends. The work environment is steady and supportive, with room for career growth but less pressure to frequently prove oneself. This role prioritizes work-life balance, allowing for a structured and steady lifestyle. The pay is lower compared to more demanding roles, but it comes with stability and consistency.

**Job 2**

This job is fast-paced and offers high potential for professional advancement, with flexible but demanding hours that sometimes require working evenings or weekends. There is significant travel involved, which can lead to valuable experiences but also requires time away from home. This role prioritizes career achievement, with an emphasis on adapting to new challenges and environments. The pay is higher, reflecting the demands and opportunities for growth in this role.

**Looking back now, which job do you wish you had chosen as your first job?**

Job 1	Job 2
-------	-------

Page Break

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You said that, looking back now, **you wish you had chosen Job 1 [Job 2] as your first job.**

**Assume your first child would want to choose Job 2 [Job 1] as their first job.**

Even if you do not think your first child would actually choose Job 2 [Job 1], please assume this is the case for the purpose of the following question.

**Which job would you choose for your first child as their first job?**

Job 1	Job 2
-------	-------

**Why would you choose this job for your first child?**

Job Success	Personal Well-Being / Happiness	Moral Values / Ethics	Cultural Identity / Heritage	Social Acceptance / Relationships	Other
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Page Break

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Now, we want you to think about the following: **Maybe people change their preference regarding their first job over the course of their life.**

**That is, someone might choose one job when they are younger, but later in life, looking back, they might wish they had chosen a different one.**

**How relevant do you think that such a change is in the context of your previous answers about your and your first child's job preference?**

Please answer on a scale from 0 to 10, where 0 means "not relevant at all" and 10 means "extremely relevant."

0 1 2 3 4 5 6 7 8 9 10

Not relevant at all                  Extremely relevant

Now, we want you to think about the following: **Maybe the world has changed between when you chose your first job and when your first child is choosing their first job.**

**That is, maybe job opportunities, societal values, or labor market conditions have shifted in ways that affect what kinds of jobs are desirable.**

**How relevant do you think that such a change is in the context of your previous answers about your and your first child's job preference?**

Please answer on a scale from 0 to 10, where 0 means "not relevant at all" and 10 means "extremely relevant."



End of Block: ImperfectEmpathy

---

Start of Block: Substitutability

Now we are interested in the following scenario:

**Liberal [Conservative (random treatment variation)] Values in School and Peer-Group**

Imagine that your child is attending a school where most teachers emphasize liberal [conservative] values.

This means, valuing for example:

- Diversity [Tradition and established norms]
- Self-expression and individual choice [Discipline and respect for authority]
- Social security and redistribution [Personal responsibility for life outcomes]

Additionally, your child's peer group largely reflects these values, with their friends and classmates discussing similar ideas and practices.

**Assuming this scenario to be true**, we are interested in the extent to which you would emphasize liberal vs. conservative values when interacting with your child.

**Which values would you try to emphasize in the interaction with your child if the scenario was true?**

There is no right or wrong. We are simply interested in your personal opinion.

Please answer on a scale from 0 to 10, where 0 means "highly liberal" and 10 means "highly conservative."



**Assuming this scenario to be true, how much effort would you put in emphasizing these values?**

Please answer on a scale from 0 to 10, where 0 means "no effort at all" and 10 means "maximum effort."



End of Block: Substitutability

---

Start of Block: Socio-Demographics

What is your year of birth?

What is your gender?

Male

Female

Other

Page Break

---

In which state do you currently reside?

What is your employment status?

Full-time employed

Part-time employed

Self-employed

Student

Retired

Unemployed (searching for a job)

Inactive (not searching for a job)

Page Break

---

Were you born in the United States?

Yes

No

Which category best describes you?

American Indian or Alaska Native (Eg: Navajo nation, Blackfeet tribe, Mayan, Aztec, Native Village or Barrow Inupiat Traditional Government, Nome Eskimo Community, etc)

Asian (Eg: Chinese, Filipino, Asian Indian, Vietnamese, Korean, Japanese, etc)

Black or African American (Eg: African American, Jamaican, Haitian, Nigerian, Ethiopian, Somalian, etc)

Hispanic, Latino or Spanish origin (Eg: Mexican or Mexican American, Puerto Rican, Cuban, Salvadoran, Dominican, Colombian, etc)

Middle Eastern or North African (Eg: Lebanese, Iranian, Egyptian, Syrian, Moroccan, Algerian, etc)

Native Hawaiian or Other Pacific Islander (Eg: Native Hawaiian, Samoan, Chamorro, Tongan, Fijian, etc)

White (Eg: German, Irish, English, Italian, Polish, French, etc)

Some other race, ethnicity or origin

Page Break

---

What is the highest level of education you have completed?

No schooling completed

Primary school

Lower secondary school

Vocational degree

High school

College degree

Master's degree or above

Page Break

---

Are you still in a relationship / marriage with the other parent of your first child?

Yes

No

[if "No" selected:]

Are you in a new relationship / marriage?

Yes	No
-----	----

Page Break

---

[If “Yes” answered to “Are you in a new relationship / marriage?”:]

Between the other parent and your current partner, who has had more influence on shaping your first child’s attitudes and character traits?

Other Parent	New Partner
--------------	-------------

Page Break

---

How would you **describe your personality**?

Please select **up to three** traits that you think fit best.

Curious and eager to learn
Organized and responsible
Energetic and outgoing
Kind and considerate
Emotionally sensitive
Independent and strong-willed
Quiet and reserved
Easygoing and adaptable
Determined and persistent
Playful and imaginative
Careful and risk-averse
Spontaneous and adventurous
Prefers structure and routines
Socially cautious but warm once comfortable

How would you describe the **personality of the other parent** of your first child?

Please select **up to three** traits that you think fit best.

[If “New Partner” answered for “Between the other parent and your current partner, who has had more influence on shaping your first child’s attitudes and character traits?”:]

- Curious and eager to learn
- Organized and responsible
- Energetic and outgoing
- Kind and considerate
- Emotionally sensitive
- Independent and strong-willed
- Quiet and reserved
- Easygoing and adaptable
- Determined and persistent
- Playful and imaginative
- Careful and risk-averse
- Spontaneous and adventurous
- Prefers structure and routines
- Socially cautious but warm once comfortable

Page Break \_\_\_\_\_

How would you describe the quality of the relationship between you and your first child in general?

Exceptionally bad    ○ ○ ○ ○ ○ ○ ○ ○ ○ ○    Exceptionally good

Page Break \_\_\_\_\_

How would you characterize the parenting style **you use(d)** with your first child?

Authoritative (supportive, sets clear boundaries, encourages open communication)

Authoritarian (strict, high expectations, little room for negotiation)

Permissive (lenient, giving lots of freedom and independence)

Neglectful (emotionally distant, uninvolved, little guidance)

Other

How would you characterize the parenting style **the other parent use(d) [your new partner uses / used]**<sup>1</sup> with your first child?

Authoritative (supportive, sets clear boundaries, encourages open communication)

Authoritarian (strict, high expectations, little room for negotiation)

Permissive (lenient, giving lots of freedom and independence)

Neglectful (emotionally distant, uninvolved, little guidance)

Other

How would you say you and the other parent [your new partner]<sup>1</sup> divide(d) parenting responsibilities of your first child?

Equally

Only I am responsible ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ Only the other parent [new partner] responsible

How would you say you and your new partner divide(d) parenting responsibilities of your first child **specifically in shaping their attitudes and character traits**?

Equally

Only I am responsible ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ Only the other parent [new partner] responsible

Page Break

---

How many siblings do **you** have?

0 1 2 3 4 5 > 5

<sup>1</sup> If “New Partner” answered for “Between the other parent and your current partner, who has had more influence on shaping your first child’s attitudes and character traits?”].

How would you characterize the parenting style **your father** used with **you**?

- Authoritative (supportive, sets clear boundaries, encourages open communication)
- Authoritarian (strict, high expectations, little room for negotiation)
- Permissive (lenient, giving lots of freedom and independence)
- Neglectful (emotionally distant, uninvolved, little guidance)
- Other

How would you characterize the parenting style **your mother** used with **you**?

- Authoritative (supportive, sets clear boundaries, encourages open communication)
- Authoritarian (strict, high expectations, little room for negotiation)
- Permissive (lenient, giving lots of freedom and independence)
- Neglectful (emotionally distant, uninvolved, little guidance)
- Other

Page Break 

---

What kind of school does your first child attend (or did they attend / do you plan them to attend)?

If multiple apply, please select the one you think applies best or mostly.

- Public school
- Private school
- Charter or Magnet School
- Homeschool
- Special Education
- Religious School
- Other

Page Break 

---

**How liberal or conservative do you want your first child to be?**

By liberal, we mean valuing for example:

- Diversity
- Self-expression and individual choice
- Social security and redistribution

By conservative, we mean valuing for example:

- Tradition and established norms
- Discipline and respect for authority
- Personal responsibility for life outcomes

There is no right or wrong. We are simply interested in your personal opinion.

0 means "highly liberal" and 10 means "highly conservative."



Page Break

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**How would you describe the political orientation of your first child's school / teachers and of your first child's friends / peer group?**

By liberal, we mean valuing for example:

- Diversity
- Self-expression and individual choice
- Social security and redistribution

By conservative, we mean valuing for example:

- Tradition and established norms
- Discipline and respect for authority
- Personal responsibility for life outcomes

0 means "highly liberal" and 10 means "highly conservative."

**School / Teachers**



**Friends / Peer Group**



Page Break

---

What is the birth year and gender of your first child?

Birth year of first child:

Gender of first child:

Male	Female	Other
------	--------	-------

How would you describe the personality of your first child?

Please select **up to three** that you think fit best.

Curious and eager to learn
Organized and responsible
Energetic and outgoing
Kind and considerate
Emotionally sensitive
Independent and strong-willed
Quiet and reserved
Easygoing and adaptable
Determined and persistent
Playful and imaginative
Careful and risk-averse
Spontaneous and adventurous
Prefers structure and routines
Socially cautious but warm once comfortable

Page Break 

---

For your other children, please also provide their gender.

Gender of second child:

Male	Female	Other
------	--------	-------

*[repeated for at most five children]*

Page Break 

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For each of the following, please indicate how important it is in your life.

	not at all important	not very important	rather important	very important
Family	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Friends	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Leisure time	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Politics	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Work	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Religion	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Page Break 

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What's your approximate **annual household income (before withholding taxes)**?

- Less than \$10,000
- \$10,000 to \$14,999
- \$15,000 to \$24,999
- \$25,000 to \$34,999
- \$35,000 to \$49,999
- \$50,000 to \$74,999
- \$75,000 to \$99,999
- \$100,000 to \$149,999
- \$150,000 to \$199,999
- \$200,000 or more

How would you describe the area where you live?

Urban / City

Suburban

Town

Village

Countryside

Page Break

---

How would you describe **your political views**?

0 means "highly liberal" and 10 means "highly conservative."

0 1 2 3 4 5 6 7 8 9 10

Highly liberal            Highly conservative

How would you describe the **political views of the other parent of your first child**?

0 means "highly liberal" and 10 means "highly conservative."

0 1 2 3 4 5 6 7 8 9 10

Highly liberal            Highly conservative

*[If applicable:]*

How would you describe the **political views of your new partner**?

0 means "highly liberal" and 10 means "highly conservative."

0 1 2 3 4 5 6 7 8 9 10

Highly liberal            Highly conservative

End of Block: Socio-Demographics

---

### 3.B.2 Coding Instructions

[The block below was looped for each response.]

---

Start of Block: Loop

**Response**

[response to be coded]

**Coding:**

1) Valence (only 1 choice):

Positive	Negative	Other	Unclear
----------	----------	-------	---------

2) Domain (multiple choices possible; select all that apply):

Physical / Psychological / Health
Financial / Career
Social / Interpersonal
Other
Unclear

Note: If "Unclear" is selected, no other choice can be selected.

**Coding Instructions:**

The responses you are classifying are from parents, who were asked what kinds of risks they were thinking about when answering questions about their children's "risk attitude" in a survey.

We aim to better understand how parents interpreted the concept of "risk" along the following two dimensions:

**1) Valence Dimension (One category per response)**

The **Valence** dimension captures whether the parent views the child's risk-taking as desirable (something to encourage) or undesirable (something to avoid).

**Categories:**

Ask: Does the parent seem to want to encourage or prevent this kind of risk-taking?

- **Positive** if the response relates to, e.g., development, benefits, monetary gain, or learning
- **Negative** if the response relates to, e.g., danger, harm (substance abuse, unhealthy behavior) or moral concern
- **Other** if the response is ambiguous or neither positive nor negative
- **Unclear** if the response is nonsensical / lacks meaning

## 2) Domain Dimension (Multiple labels possible)

The **Domain** dimension captures the **type** or **context** of the risk the parent is referring to. A single response may be tagged with **multiple domains**.

### Categories:

- **Physical / Psychological / Health:** mentioning, e.g., bodily harm, emotional / psychological harm, safety, or substance use
- **Financial / Career:** mentioning, e.g., career, investment, job, growth, material benefits, monetary gain, opportunities, chances
- **Social / Interpersonal:** mentioning, e.g., relationships, peer pressure, or expression of oneself
- **Other:** any domains not covered by the three categories above
- **Unclear:** unclear or nonsensical responses

### Rules of Thumb:

- Assign **all relevant** domain labels.
- Use **Other** if a domain is mentioned that is not among the three ones explicitly mentioned above
- Use **Unclear** if the answer lacks meaning or is unclear

End of Block: Loop

---

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