The Effect of Social Comparison on Debt Taking: Experimental Evidence*

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February 2023

Abstract

A number of studies show a link between social comparison and high levels of household debt. However, the exact underlying mechanisms are not yet well understood. In this paper, we disentangle two mechanisms in a lab experiment to study the effects of social image concerns and peer information on debt-financed consumption choices. We find that having to announce their consumption decisions publicly does not make participants more likely to take on debt but does make them more likely to buy lower quality. Participants with information about other participants' choices seem to conform more to these choices, taking on slightly more debt as well as buying lower quality.

Keywords: Household Finance; Lab experiment; Social Comparison; Peer Effects
JEL: D14; G51; D91

*We thank Anik Ashraf, Fenella Carpena, Dirk Engelmann, Jana Friedrichsen, Jana Hamdan, Rawley Heimer, Lea Heursen, Raji Jayaraman, Lukas Menkhoff, Christoph Merkle, Felix von Meyerinck, Renke Schmacker, Helke Seitz, Roel van Veldhuizen, Georg Weizsäcker, Leeat Yariv, and seminar participants in Berlin, Oslo, and Vienna for helpful comments. The paper also benefited from discussions with conference participants at BFWG 2021, Experimental Finance Conference 2021, IMEBESS 2019, ESA Global Meeting 2020, the Royal Economic Society Conference 2021, and VfS Annual Conference 2021. Views presented are those of the authors and not necessarily those of the Oesterreichische Nationalbank or the Eurosystem. Financial support from the Collaborative Research Center TRR 190 is gratefully acknowledged. The experiment is registered in the AEA RCT Registry, ID: AEARCTR-0003597.

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1 Introduction

In Germany, 6.9 million households were regarded as over-indebted (Creditreform Wirtschaftsforschung, 2019) in 2019, while 7.5% of German households had negative assets in 2017 (Deutsche Bundesbank, 2019).[1] Within the European Union, Germany even constitutes a lower bar. In 2016, a nationally representative survey for the EU found that 14% of the interviewed households were in arrears at least once in the previous year (Eurofound, 2020). Household over-indebtedness is a wide ranging problem and can pose a serious risk to household well-being and the economy as a whole (IMF, 2017). Taking too much debt cannot be rationalized by conventional economic theory, except if an unpredictable shock happens to the household. Hence, there are many open questions about the determinants of debt taking (Zinman, 2015).

One promising but still very small line of research studies the effects of social comparison or peer effects on debt levels. Georgarakos et al. (2014) find that individuals who believe their social circle to have a higher income than themselves are more likely to hold debt. Agarwal et al. (2019) show, using lottery winners in Canada as exogenous variation, that unequal incomes in neighborhoods can lead to bankruptcy and financial distress. These studies find evidence for the effect of social comparison or peer effects on debt taking. More importantly, they highlight how inequality can lead to what may be perceived as wasteful spending and, eventually, to sub-optimal financial outcomes that can even spill over on financial markets. However, what remains unclear are the mechanisms behind these observed peer effects. In this paper, we shed light on these mechanisms by performing a lab experiment in which we are able to disentangle different channels that link peer effects and debt taking. Understanding the mechanism behind peer effects can help in the design of possible interventions to reduce debt taking.

In general, Bursztyn and Jensen (2017) distinguish between two kinds of mechanisms behind peer effects: *social image concerns*, which are concerns about private information about oneself that is revealed to others, and *peer information*, which is the information about others that is revealed to oneself. As disentangling these two effects is almost impossible using observational data, we take this question to the lab. In our experiment, we use three different treatments to disentangle these two potential drivers behind peer effects observed in debt taking. By disentangling these effects, we not only contribute to the above mentioned literature on peer effects on debt taking, but also test if these mechanisms that are individually identified to impact consumption (Clingingsmith and Sheremeta, 2017; Grohmann and Sakha, 2019), also hold once consumption can be financed through debt. At the same time, by running an experiment, we are able to circumvent identification

[1] The Schuldneratlas regards people as overindebted based on late payments and pre-collection letters, whereas the Bundesbank looks at assets and liabilities collected during household surveys.

problems associated with peer effects (Manski, 1993).

Our experiment builds on established experimental designs to tease out social signaling. Participants buy one pen out of a set of different quality pens with money they have previously earned in an IQ quiz. The amount earned depends on how participants, almost exclusively students in our setting, perform in comparison to the others in the session. All the money earned in the quiz that is not spent on a pen is lost. Thus, assuming cognitive ability to be a socially desirable trait amongst our participants - as has been shown in several experiments using such populations (e.g., Clingingsmith and Sheremeta, 2017; Friedrichsen et al., 2018) - the consumption decision can be indicative of being a "lower" or a "higher" type within the peer group that is created through the experimental session. However, participants can take out a loan against future experimental earnings to buy a higher quality pen.

The three treatments vary in the way participants make and communicate a consumption decision: In the control treatment, the decision which pen the individual participant buys is kept private. In contrast, in the public treatment, each participant must announce their decision publicly. The decision of which pen to buy is made in private before the public announcement, but participants know about the announcement before they decide. In the information treatment, the consumption decisions of participants who made their decision previously are shown on screen before the individual makes their own decision. In this way, participants are informed about the decision of others without identities being revealed. Being able to take out a loan to buy a higher quality pen enables participants to shroud their type from other participants. They can later repay this loan by exerting sufficient effort in a real-effort task (the slider task by Gill and Prowse (2012)) or by using their participation fee.

To estimate not only between treatment effects but also the possible shift in preferences within participants, we elicit a non-manipulated consumption preference prior to the experiment. We conducted an online survey with future participants and compare decisions in the survey to decisions in the experiment. Furthermore, we elicit a comprehensive list of character traits and interact these traits with our treatments to investigate who is susceptible to debt taking as a result of social comparison.

We have two main hypotheses regarding the outcome of our experiment that we base on the existing literature on social comparison. First, loan take-up in the public treatment is higher than in the control treatment since "lower" types try to hide their type by buying a more expensive pen as a way to signal high IQ. This hypothesis is pre-registered. Second, in the information treatment, we expect that there will be deviation compared to choices in the control group, as participants are likely to follow those that chose before them. Hence, we hypothesize that there will be conformity in the information treatment. This

deviates from the pre-registered version of our second hypothesis. Although we discuss conformity in our original development of the hypothesis in Online Appendix I.I, in the pre-registration, we were focused on loan take-up only. We did not account for the fact that conformity can also imply adjustment from "above". Therefore, we adjusted the hypothesis in the paper.

A key assumption of this experiment is that participants do come to the lab to maximize their earnings. Deviation from this initial intention may be perceived as non-optimal with hindsight. Through this, together with framing, we believe that we can recreate some of the emotions involved in going into debt in the lab setting. Related to this, we generally expect persons who take a loan to work harder in the slider task to make up for money borrowed earlier during the experiment. In addition, we have a number of minor hypotheses regarding character traits that are listed in Online Appendix I.II.

Our results do not support the first hypothesis outlined above. Participants in the public treatment do shift their choices, but they are not more likely to take a loan nor do they take out higher loans than those in the control treatment. Instead, they are more likely to buy a pen of lower quality than they could actually afford. The effect is larger for higher performing participants. In the information treatment, we find evidence for a taste for conformity. Lower performing participants, in this treatment, take out more and slightly higher loans while higher performing participants buy a lower quality pen than participants in the control group. This effect is driven by people that choose after a larger number of participants has already chosen. It seems that participants in the information treatment converge to some average quality pen, although the adjustment from above is much larger than from below. Further, unlike hypothesized, the amount of loan taken is negatively related to performance in the slider task for all treatments, even when controlling for general ability and motivation. This means participants do not want to make up for the lost earnings with extra work.

These results are supported when comparing pre-experiment preference to actual choice in the experiment: looking at the effects of our treatments within subjects, we can see that participants in the public treatment buy lower quality pens, whilst participants in the information treatment adjust their choices to match those who have chosen before them, which is not the case for participants in the control treatment.

We move on to discuss reasons why our results in the public treatment are unlike hypothesized: Two likely explanations are that (i) participants do not want to be perceived as smarter than their peers, as it might not be socially desirable; and/or (ii) they do not want to be publicly blamed for making other participants worse off as their "success" in the experiment is directly linked to the "failure" of others. We, therefore, believe that we found evidence of social image concern in individuals. However this seems to be of

a different type than originally hypothesized. Subsequently, we conclude that the both forms of peer effects that we examine in this study do matter. However they do not always take the expected form. Ultimately, the long-established idea of signalling social image by consuming more could not be shown. Overall, this study suggests that social comparison is important, but complex, in how it influences debt decisions. Future research should examine the different forms that peer effects can take, also depending on different samples and situations.

Our study is linked to three strands of the literature. First, to the literature of peer effects in consumption and other financial decisions. Several studies find that social comparison influences consumption. In the field, Kuhn et al. (2011) find that the likelihood of buying a new car increases if someone in the neighborhood has recently bought a new car (see also Grinblatt et al., 2008). Even poor, rural villagers make consumption decisions that are in line with the decisions of those that they observe (Grohmann and Sakha, 2019). Being informed about neighbors energy consumption causes people to reduce their own energy consumption, but only in the long run (Kažukauskas et al., 2021). Cosaert (2018) concludes that conspicuous consumption at least partially explains a preference for goods that convey utility because they are costly, so-called diamond goods. Methodologically, the experiment by Clingingsmith and Sheremeta (2017) is closely related to ours. They show in the lab that participants increase consumption of a "luxurious" good if income is linked to a desirable trait and the decision has to be made publicly. Our experimental design resembles theirs. However, we explicitly look at taking debt to consume and have an additional treatment to test for peer information effects.

Peer effects also affect a number of other financial decisions. Duflo and Saez (2003) show that retirement savings decisions are influenced by peers. Interestingly, Beshears et al. (2015) show that low-savings individuals save less when prompted about peers higher savings. Dur et al. (2021) find that although a simple social norm nudge might increase intentions to save it does not automatically translate to an actual change in the savings rate. However, Breza and Chandrasekhar (2019) find that peer monitors increase savings. Stock market participation has been shown to be affected by peer's stock market participation (Hong et al., 2004; Kaustia and Knüpfer, 2012). Heimer (2016) attributes part of the disposition effect to social interaction. In the lab, Gächter et al. (2012) show that participants are more likely to put in effort if they observe others doing so in response to high wages. Furthermore, people are less likely to take up additional assistance when this has to be done publicly to compensate for a smaller payout due to low cognitive ability (Friedrichsen et al., 2018).

The second, much smaller line of literature tries to disentangle effects that underlie social comparison. Many of the aforementioned studies implicitly explain higher "visible"

consumption merely as a result of conspicuous consumption. In reality, it is hard to disentangle if persons want to convey status with their visible consumption or they just "learned" from the visible consumption of others, and they want to conform. Bursztyn et al. (2018) find evidence for the demand of pure status in credit card ownership in a setting where the information channel is unlikely. Moreover, Bursztyn et al. (2014) show in a field experiment designed to disentangle the effect of "social learning" and "social utility" on investment decisions that both channels have an effect. However, the focus there is clearly on savings and investments not on consumption and debt taking. There is further substantial literature on how social -peer- information affects pro-social behavior and charitable giving (e.g. Frey and Meier, 2004; Shang and Croson, 2009; Mittone and Ploner, 2011; Smith et al., 2015) but much less so on (debt-financed) consumption.

Third, we add to the literature concerning debt taking and peer effects. To the best of our knowledge, there are only a few studies looking explicitly at this relationship. Bertrand and Morse (2016) describe how the presence of higher top-income households is probably causally related to higher credit and bankruptcy in low-income households. Kalda (2019) shows that financial distress leads peers of those experiencing distress to decrease their debt to income ratio. Guiso et al. (2013) argue that observing peers strategically defaulting on their mortgage leads to greater willingness to also strategically default. They attribute this to learning about financial distress through observation. As mentioned before, Georgarakos et al. (2014) find a link between beliefs regarding the affluence of the own social circle and debt taking. Agarwal et al. (2019) show that having a lottery winner in the neighborhood leads to increased rates of bankruptcy for others in the area. The former studies present correlations and none of these studies can explicitly show through which channels peer effects work.

The paper proceeds as follows: Section 2 explains the experimental design and the data we collect. Section 3 reports our main results and Section 4 presents robustness checks. Section 5 discusses our findings in more detail while Section 6 concludes.

2 Experimental Design

Our experiment is designed to mimic consumption decisions in social situations when credit is available. The treatments vary in how the consumption decisions are made. There are three main stages to our experiment. A schematic description of the different stages is shown in Figure 1.

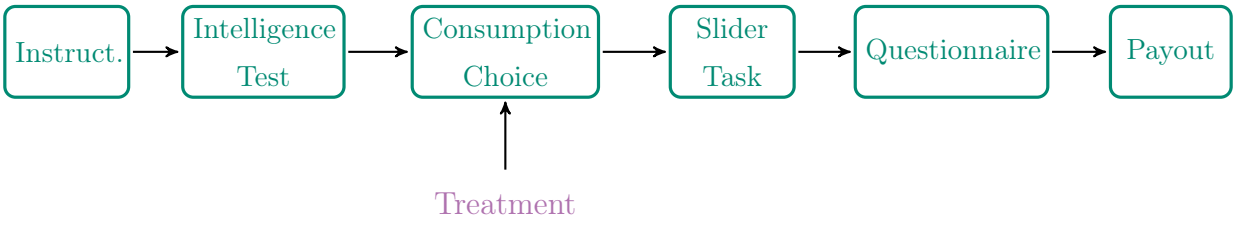


Figure 1: Experimental Flow

Once in the lab, participants first read the instructions and enter an individually constructed ID on the computer. Instructions are given in written form and on screen (see Online Appendix III.I). Then, the participants have to answer comprehension questions on the screen regarding the procedure of the experiment and payoff possibilities (see Online Appendix III.II). If there are participants who have made mistakes in this part, the experiment only continues after one of the experimenters explains the right answer to these persons.

2.1 Intelligence Test

Following the instructions and comprehension questions, participants take an IQ-style test, for which they are paid according to their performance in relation to others in the session. All questions are taken off a website endorsed by *Mensa* and designed by a member of Mensa. Participants are made aware that these type of questions are used to measure intelligence. The questions are not designed to give a reliable IQ measure but present small intelligence riddles with varying levels of difficulty. We include questions for several levels of difficulty as defined by the website. Our test consists of twelve questions: four questions on completing number sequences and eight questions on completing sequences of pictures with geometric forms (see Online Appendix III.III). Participants have seven minutes to answer as many questions as possible. An intelligence test is chosen by us to induce social image concerns because we believe it is intrinsically desirable for our student sample to perform well on it. Hence, we believe that for a student sample being seen as intelligent is socially desirable. Indeed, IQ-style or general knowledge tests have been used in other lab experiment to generate social status for students (Clingingsmith and Sheremeta, 2017) and even the same subject pool (Friedrichsen et al., 2018). The top performing quartile in each session is paid 3€, the third is paid 2€, the second is paid 1€ and the bottom quartile is paid 50 Cents.

The test has an adequate level of difficulty. Out of the twelve questions the best candidates answered nine questions correctly and the worst none. The average candidate gave 4.43 correct answers and, with a standard deviation of 2.14, there is a good spread in the number of questions answered correctly.

2.2 Consumption Choice

The quiz is followed by a "shopping round," in which participants can buy a pen with the money they just earned. The pen is available in 5 different qualities and we use a star rating to convey the difference in quality: the five-star pen costs 4€, the four-star 3€, the three-star 2€, the two-star 1€, and the one-star pen costs 50 Cents. Thus, there is a pen quality for each earnings level and one pen whose price exceeds maximum quiz earnings. A picture of the pens and their labels is placed by each computer at the beginning of the session (see Online Appendix III.IV). The lab prices present the actual list prices of the pens that are all from the same brand. The labeling makes it clear that the more expensive pens are supposed to be more desirable than the cheaper ones. To buy a pen that costs more than what participants earned during the IQ-test, participants can take a loan of up to 3.50€. Debt is repaid using other earnings during the same session. It is not possible for participants to leave the lab with actual debt, i.e. according to the lab rules they are not allowed to leave financially worse off. However, we assume that participants come to the lab to earn as much money as possible.

Indeed, at the end of the experiment, we asked participants, regarding the second cheapest and second most expensive pen, if they would prefer to have that pen or the amount of money that pen is sold at. While for the choice between 1€ and the two-star pen still 14% of all participants say they prefer the pen, for the choice between 3€ and the four-star pen not even 8% say they prefer the pen. We take this as evidence that participants do come to the lab to earn money, rather than to purchase pens. This, together with framing creates a situation that is similar to taking debt outside the lab, at least on an emotional level. All the earnings from the quiz that are not spent on a pen are lost.

The way the choice of a pen is communicated and what the participants know about the choice of others varies between treatments. Instructions on how to make pen choices and how to communicate these are shown to participants on the screen in front of them (for more details, see Subsection 2.3).

One of the reasons why we choose pens is that we assume that preferences are rather unidimensional: at baseline, the price is the most decisive factor in the preference relation for the five pens. This might not be the case for products like chocolate. Simply speaking, for pens the price is more important than personal taste for color or material. Hence, there should be no other reason to buy a lower quality pen except that it is cheaper. Indeed, when examining the data of the pre-experiment survey (detailed description below) this assumption seems to be valid, especially compared to other goods, as seen in Online Appendix Figure II.I. In total, we collected 323 answers in our online survey and approximately 50% choose the cheapest pen. For all the other goods surveyed, no more

than 24% ever choose the cheapest option. For example, for folders, which belong to the same group of goods as pens (stationery), only 16% choose the cheapest.

However, this means even in a standard economic framework without peer effects, incentives to buy a lower quality pen than one they can afford are relatively small or non-existent in our experiment. This is the case because all the earnings from the IQ-quiz that are not spent on a pen are lost. Hence, participants would leave money on the table. However, as outlined before, in the absence of peer effects, there is also no particular incentive to buy a more expensive pen. These pens are widely available and are offered at retail prices. Thus, pens are less likely to confound our treatment than other products, as the preference is more easily malleable.

2.3 Treatments and Hypotheses

We assume that the most revealing signal for being in a particular earnings/performance group is to buy the pen whose price exactly corresponds to this group. That is because both pen prices and the earnings structure for the intelligence test are common knowledge:[2]

$$Pr_{-i}(\sigma_i = T|a_i = T) > Pr_{-i}(\sigma_i = T|a_i \neq T), \tag{1}$$

where σ_i is the type of individual i, $T \in \{1, 2, 3, 4\}$ is the type space, which in our case are the four possible performance groups, and $a_i \in \{1, 2, 3, 4, 5\}$ is the action space, thus the five possible pens an individual can buy. $Pr_{-i}(\sigma_i|a_i)$ represents the probability that the other participants think individual i belongs to a certain group given a certain action.

Furthermore, we assume that buying the most expensive pen makes it most likely for the other participants that i belongs to the highest (the fourth) performance group:

$$Pr_{-i}(\sigma_i = 4|a_i = 5) > Pr_{-i}(\sigma_i \neq 4|a_i = 5) \tag{2}$$

With these assumptions in mind and based on the extant literature, we explain the treatments and their consequences in the following.

Private Treatment: Control

The private treatment is the control treatment. In the shopping round, participants simultaneously decide on their individual computer screens which pen they want to buy. The decision is kept private, and they continue to the slider task without any further

[2] Our notation is largely taken of the social image model of Bursztyn and Jensen (2017). Their model is based on Bénabou and Tirole (2006). A more general model of social influence that has similar implications but can incorporate conformity is developed in Fershtman and Segal (2018)

intervention. The instructions on the screen make no mention of other people, but simply ask participants to pick a pen that they would like to buy. In this treatment, social image concerns and peer information do not occur as there is no possibility for participants to infer which pen the others buy (which means $Pr_{-i}(\sigma_i|a_i)$ is not defined). Hence, it is not possible to infer other participants' types from their choices. The pens are handed over individually in another room next to the lab, and participants leave after payout.

Public Treatment: Social Image Concerns

In the public treatment, participants again first make the decision simultaneously. However, after everyone made their decision they have to stand up one after another and have to publicly announce which pen they have chosen. This is described to participants on the screen before they make their choice. The order in which participants stand up is random. Participation numbers are called out loud and participants have to stand up next to their cubicle. They are informed beforehand that announcing the consumption decision is part of the procedure and are shown by the experimenter how this should be done. Given our previous assumptions, buying a low quality pen is a strong signal for low performance in the IQ-quiz. Hence, further assuming that being seen as intelligent is a desirable trait, social image concerns can potentially occur, as $Pr_{-i}(\sigma_i|a_i)$ is well-defined. This is true especially for those individuals who end up in the lower performance groups. A number of papers that perform similar experiments in regards to social image concerns using students, especially looking at consumption choices, have shown that making decisions public changes choices in order to convey a more socially desirable image (e.g., Clingingsmith and Sheremeta, 2017; Friedrichsen et al., 2018). Thus, we hypothesize the following:

H1: Participants in the public treatment are more likely to take a loan to buy a higher quality pen in order to signal higher intelligence.

Information Treatment: Peer Information

The third treatment is an information treatment, where each respective participant in each session makes their decision sequentially, in random order, instead of simultaneously. Therefore, we can show participants in a small table on their screen how many pens of each kind have been bought previously in the same session (see Online Appendix III.VI). Everyone is sitting in a cubicle with high walls and instructions are solely given on the computer without any interruptions from the experimenters. In this way, participants are informed about what their session peers decided while no identities are revealed. This means that $Pr_{-i}(\sigma_i|a_i)$ is not defined because i cannot be identified, but that i has several

a_{-i} that she can consider when making her own decision. Therefore, peer information can occur, but social image concerns are very unlikely to play a role. As conformity, we define the intrinsic preference to align consumption decisions to those of others without others even learning about this (see Goeree and Yariv, 2015). Several papers have found evidence for conformity in similar settings, where participants can observe each other (Fatas et al., 2018; Grohmann and Sakha, 2019). Our second hypothesis is as follows: [3]

H2: Participants in the information treatment follow the decision of those who have already made their decision. Thus, we expect to find evidence for conformity in this setting.

2.4 Slider Task

After the consumption choice, participants perform the slider task developed by Gill and Prowse (2012). The slider task is a computerized real-effort task where participants have to move a predefined number of "sliders" to a predefined position with their cursors (see Online Appendix III.V). In our experiment, they have four minutes to move up to 48 sliders to the value "50." Effort is measured by counting the number of correctly adjusted sliders. Moving the sliders is rather cumbersome and non-entertaining. Furthermore, we implement a sharply decreasing marginal return to effort: the first eight correctly adjusted sliders pay 25 cents each, the next eight earn 15 cents each, the following eight get 10 cents each, the next eight earn 5 cents each, the following eight 3 cents, and the final eight 2 cents. The slider task gives participants who previously took a loan the chance to earn additional money to repay that loan. After the slider task, final earnings from the experiment are calculated. If participants decide to take up a loan and do not exert enough effort in the slider task to repay it, the money is taken off the participation fee.

Participants are explained the exact task (including a trial where they can learn to move the slider), the time allocated, and the payoff structure right before the slider task. The average number of sliders set correctly is 23.83 (std. dev. 10.26), and the maximum is 48 out of 48. This is in line with performances in other experiments that involve slider tasks, such as Gill and Prowse (2019).

2.5 Pre-Experiment Survey

In their invitation email to the experiment, participants are asked to complete an online survey that was created with *Google Forms*. Invitation emails are sent out one week before the sessions take place, and participants are reminded to fill out the survey 1-2

[3] Hypothesis two was reformulated compared to the original pre-registered hypothesis to take account of the possibility that adjustment also happens from above.

days beforehand. In the survey, they have to provide an individual ID so that we can later link these data to the data collected in the experiment.[4]

In the online survey, participants are asked for their preferred product out of a group of five homogeneous goods. They have to indicate their favorite type of chocolate, cola, folder, lip balm, and pen. We show them a picture and the list price of each product (see Online Appendix III.VII). The five pens are the same pens that they later can buy in the experiment. Thus, a pre-treatment preference for pens is elicited that we use for a within-subject analysis. By asking for a variety of homogeneous goods, we can reduce priming as participants are less likely to remember their choice. We further get an indication on whether the price is a decisive factor when choosing a pen and whether this is different for the other products. Additionally, we include questions on the importance of price, brands/image, and the opinion of others when buying small, everyday products like the products in the survey. These questions are measured on a Likert-scale from one to seven.

2.6 Individual Characteristics

We not only want to analyze the possible channels through which peer effects might increase debt taking but also who responds to which channel. Various studies look at differences in socially contingent consumption rather along socio-economic lines (like income, region, "race", etc.). We want to compliment the literature by investigating what kind of personal attitudes and characteristics make persons more or less susceptible to social image concerns and responsive to peer information. We concentrate on five distinct personality concepts, namely cognitive reflection, locus of control, global self-esteem, self-monitoring, and the Big Five personality traits. Each of these is measured with well-established methods from the literature. *Cognitive Reflection* measures a specific type of intelligence: the tendency to reflect on problems rather than following a wrong intuition when looking for an answer. We use the three questions originally introduced by Frederick (2005). *Locus of Control* presents the perceived control over the own life. Here, we use the scale used in the German Socio-Economic Panel (Wagner et al., 2007), which itself is based on Rotter (1966). The "Rosenberg Self-esteem Scale" (see Rosenberg, 1979; Ferring and Filipp, 1996; von Collani and Herzberg, 2003) is employed to assess *Global Self-Esteem* (GSE). In contrast to specific self-esteem, GSE is an overall feeling of self-worth that is not attached to a particular situation. *Self-Monitoring* describes the willingness and/or ability of individuals to adapt their behavior to different social situations and is measured with the revised self-monitoring scale by Snyder (1974) (see Snyder and Gangestad, 1986;

[4] The ID is composed of the third letter of the first name + the last two numbers of the zip code + the last letter of the last name in capitals + the birthday for each individual participant. In this way, we can merge the online survey with the experimental data whilst participants remain anonymous and no sensitive data is collected by the researcher.

Graf, 2004). *The Big Five* are measured using the short version of the big five inventory "BFI-S" (John and Srivastava, 1999; Gerlitz and Schupp, 2005). For detailed hypotheses, as well as results on the relationship between these personality traits and susceptibility to social comparison, see Online Appendix I.I.

Additionally, we collect socio-economic variables like sex and age as well as data on lab experience, financial literacy, and risk preference. The financial literacy scale is based on Lusardi and Mitchell (2008) and complemented by self-designed questions. Risk preference is measured with the question on general risk taking by Dohmen et al. (2011).

2.7 Procedure and Participants

Our experiment took place at Technische Universitaet Berlin in November 2018. Including three pilot sessions, 27 experimental sessions were run. Treatments were randomized at the session level, and each session lasted between 42 and 58 minutes.[5] The sessions themselves were randomized across weekdays and daytime. On average, participants earned 14.33€, including a show-up fee of 5€ and a participation fee of 3.50€. In total, 305 students from various disciplines participated. All sessions had at least nine participants, and most consisted of twelve participants. The experiment is programmed using z-Tree (Fischbacher, 2007), and participants are recruited from the subject pool of the Technical University laboratory via ORSEE (Greiner, 2015). The experiment is registered in the AEA RCT Registry, RCT ID: AEARCTR-0003597.[6]

In Table 1, we present the descriptive statistics for the whole sample and the control group, as well as the differences between the control group and the two treatments, respectively. For the main analysis, we exclude the 35 observations from our three pilot sessions, as we changed the experimental procedure substantially after the pilot. As can be seen, our treatments are gender-balanced, with the average participant around 23 years old, having studied for three semesters, a monthly income of ca. 690€, and already participated in at least one other experiment in the lab. There is no difference in intelligence as proxied by absolute performance in the IQ quiz and cognitive reflection between treatments. At first glance, the number of imbalances seems to be particularly high in our experiment in comparison to other studies. However, given the sample size and the large number of variables we are looking at, this is not surprising. Furthermore, an F-test on joint orthogonality of all variables on the treatment cannot be rejected (p-value=0.14). We still control for the imbalanced variables in our specifications whenever possible.

[5] There is a significant correlation between duration and treatments with mean duration of 46, 49, and 53 minutes, respectively. The difference can be explained by varieties in the procedure.

[6] See https://www.socialscienceregistry.org/trials/3597. The sample size is slightly smaller than pre-registered due to no-shows.

Table 1: Descriptive Statistics across Treatments

	Full Sample (1)	Control (2)	Control-Public (3)	Control-Info (4)
Male	0.48	0.48	−0.00	0.01
Age	22.86	22.57	−0.58	−0.29
Education	3.36	3.27	−0.20**	−0.06
Students	0.97	0.99	0.05*	0.02
Semester	3.68	3.50	−0.62	0.09
Student Job	0.28	0.27	−0.03	0.01
Mthl. Income	688.36	713.84	18.35	60.98
Risk Preference	5.34	5.81	0.85**	0.64*
Lab Experience	1.89	1.95	0.07	0.12
Know Someone	0.31	0.18	−0.22**	−0.17*
Persons in Session	11.33	11.67	0.46***	0.58***
Correct Control Questions	4.74	4.76	−0.01	0.08
Correct Quiz Questions	4.43	4.55	−0.00	0.37
Financial Literacy	4.59	4.70	0.27	0.07
Cognitive Reflection	1.91	2.01	0.19	0.12
Conscientiousness	−0.01	−0.12	−0.14	−0.20
Neuroticism	0.03	−0.06	−0.22	−0.07
Extraversion	−0.02	0.08	0.28*	0.04
Openness	−0.02	0.08	0.18	0.13
Agreeableness	−0.01	−0.13	−0.26*	−0.10
Self-Esteem	−0.01	0.19	0.37**	0.23*
Locus of Control	0.00	0.13	0.29*	0.10
Self-Monitoring	−0.02	−0.02	0.02	−0.03
Observations	270	93	182	181

Columns (1) and (2) report mean values for the whole sample and the control group. Column (3) and (4) report the difference in mean values between control and public treatment or information treatment group respectively. Variables: *Male* is a dummy that equals 1 if the participant is male and zero otherwise; *Age* is the age of the participant in years; *Education* measures the educational level on a scale from 1 "Lower secondary education" to 4 "PhD"; *Students* is a dummy that equals 1 if the participant is a student and zero otherwise; *Semester* is the current study semester of a participant who is a student; *Student Job* is a dummy that equals 1 if a participant who is a student has a sideline; *Mth. Income* is the monthly income of the participant in Euro; *Lab Experience* measures how often a participant has participated in lab experiments prior to our experiment; *Know Someone* indicates if the participant either does not know or knows another person in the session or is not sure about this; *Persons in Session* is the number of participants in each session; *Correct Control Questions* is the number of correctly answered comprehension questions we gave participants before the first stage of the experiment; *Correct Quiz Questions* is the number of correctly answered IQ questions in the first stage of the experiment; *Risk Preference, Financial Literacy, Cognitive Reflection, Conscientiousness, Neuroticism, Extraversion, Openness, Agreeableness, Self-Esteem, Locus of Control, and Self-Monitoring* are personality traits measured as explained in Subsection 2.6.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; p-values are obtained using twosided t-tests.

Participants in the public treatment have a higher level of education but are slightly less likely to be students than participants in the control treatment. They are less risk-seeking and less extroverted but more agreeable and have a larger internal locus of control. We find differences between the control and information treatments for risk-seeking and self-esteem but not in the education domain. Finally, although there were significantly fewer participants per session in the public and information treatment (which is, however, exogenous to the participants), participants in this treatment are more likely to know another person in their session. Since this study analyzes peer effects, endogeneity in the peer group size could seriously jeopardize identification. However, given that participants are not aware beforehand in which treatment they will end up and that we randomized the order of treatments between daytime and weekdays, we do not have reason to believe that real-life peers were more likely to sort into one or another treatment.

3 Results

3.1 Descriptives

We start our analysis by looking at debt-taking by treatment. Overall, around 20% of the participants actually take a loan (*Loan Dummy*), and the average loan amount conditional on take-up is about 1.30€. This means that, on average, participants take up a loan to buy a pen that is one quality level higher than the one they can afford with quiz earnings. This shows that loan take-up is relatively rare. However, as can be seen in Table 2, these numbers differ across treatments.

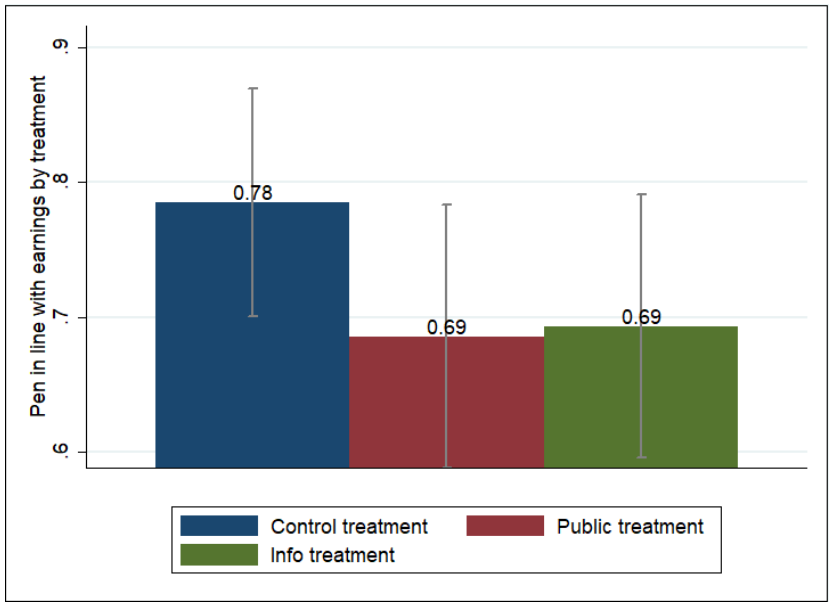
Figure 2 shows the proportion of participants that bought a pen in line with their earnings level. What we can see from this is that most participants bought the pen that corresponds to their earnings level. However, from this graph we can also clearly see that participants in the control treatment are more likely to chose pens in line with their earnings than in the other two treatments (78% vs 69%). Confidence intervals are large. P-values from onesided t-tests are 0.065 for control vs public treatment and 0.081 for control vs information treatment. Still, we take this as first evidence that the procedural way in which decisions are made leads many participants to deviate from their earnings level.

Table 2: Summary Statistics Outcome Variables

	Observations	Mean	Stand. Dev.	Minimum	Maximum
Control Treatment					
Loan Amount	93	0.22	0.62	0.00	3.50
Loan Dummy	93	0.17	0.38	0.00	1.00
Cond. Loan Amount	16	1.28	0.95	0.50	3.50
Public Treatment					
Loan Amount	89	0.23	0.53	0.00	2.50
Loan Dummy	89	0.21	0.41	0.00	1.00
Cond. Loan Amount	19	1.08	0.63	0.50	2.50
Info Treatment					
Loan Amount	88	0.31	0.73	0.00	3.50
Loan Dummy	88	0.20	0.41	0.00	1.00
Cond. Loan Amount	18	1.50	0.92	0.50	3.50

Loan Amount is the credit amount (in Euro) taken to buy a more expensive pen than one could afford (intensive margin), *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise (extensive margin), and *Cond. Loan Amount* is the credit amount (in Euro) only for those who take out a loan at all (Loan Dummy equals 1).

Figure 2: Pen Price and Earnings Match by Treatment



Average value of a dummy that equals one if the participant buys a pen whose price matches her earnings from the IQ quiz and zero otherwise (buying a more expensive or cheaper pen), by treatment group. 95% confidence intervals reported.

3.2 Loan Take-Up

We continue by examining the effects of our treatments in detail. In Table 3, the effects of the treatments on loan take-up are estimated.[7] For both treatments, there are no significant effects on whether participants took a loan nor on the amount of loan taken, as seen in Columns (1) and (2). When controlling for imbalances, as seen in Column (3), effect sizes are much smaller for the loan amount in the public treatment, even negative, which suggests that participants in the public treatment actually take a smaller loan than those in the control group. Column (4) shows that there is also no effect on the loan dummy when controlling for imbalances.

Table 3: Effects of Treatments on Loan Take-Up

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Public Treatment	0.010	0.041	–0.017	0.006
	(0.118)	(0.088)	(0.137)	(0.095)
Info Treatment	0.086	0.033	0.073	0.003
	(0.126)	(0.078)	(0.114)	(0.073)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	-0.004	-0.005	0.030	0.034
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. Control treatment is the reference category. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Given the results on loan amounts for the control group and our sample size, we would be able to detect moderate effect sizes (Cohen's d = 0.37, assuming a power=80% and α=5%). This is almost exactly the same minimum detectable effect size we calculated in our pre-analysis plan and slightly larger than the effect found in comparable studies (for example, Friedrichsen and Engelmann, 2018, find an effect of 0.3). However, the actual effect size of the public treatment is extremely small (Cohen's d = -0.017). The confidence intervals lie almost symmetrically around the null and never reach 0.3 in the

[7] In all our regressions, we estimate standard errors that are bootstrapped and clustered at session level. However, given the small number of clusters, we also calculate p-values using wild cluster bootstrap following the advice of Cameron et al. (2008). Our results of interest are robust to this specification.

positive direction. Hence, we are relatively confident that participants in our study do not take a larger loan due to social image concerns.[8] Effect sizes for the loan amount are larger in the information treatment, but standard errors are considerably higher. However, in the information treatment effects are path-dependent within each session. We do not account for this in Table 3 but aim to do so in Table 4.[9] In Columns (1) and (2), we control for the place in the order in which participants decide in the information treatment.

Table 4: Effects of Treatments on Loan Take-Up, Info Treatment Correction

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Info Treatment	–0.108	–0.173	–0.016	0.070
	(0.206)	(0.118)	(0.468)	(0.373)
Order	–0.017	–0.010*		
	(0.014)	(0.006)		
Order X Info	0.028	0.028**		
	(0.025)	(0.014)		
Mean Prev. Pens			0.023	0.051
			(0.113)	(0.117)
Mean X Info			0.034	–0.015
			(0.170)	(0.140)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	0.022	0.032	0.018	0.031
Correction	Order	Order	Mean Pen	Mean Pen
Observations	248	248	226	226

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. *Order* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. *Mean Prev. Pens* measures the mean quality of pens that have been bought before a participants in the information treatment decides. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

It is likely that those who have to decide later are adjust their behavior more as

[8] There is the possibility that participants hide their low performance, but we still do not find an effect: they simply lie when announcing the decision publicly. Controlling for this possibility by cross-checking each announcement with the data, we do not find a single person who lied in the public treatment.

[9] The analysis in which we control for paths dependency was not pre-registered for any outcome variable and can therefore be regarded as exploratory.

they receive more information regarding other participants choices. We hence include an interaction term between the information treatment and the position in the order of decisions made.[10] We find a significant order effect on the probability to take a loan at all but, no significant effect on the amount taken. This indicates that participants who make decisions later in the order are more likely to take a loan. The effect size is small, which might be the case because early decision makers can either set a high or a low benchmark. If early decision-makers choose low quality pens, there might be no reason for followers to take a loan, as the price of the pen chosen by participants is covered by their earnings. In order to test this, in Columns (3) and (4), we control for the average pen bought up to the point when the respective participant has to decide and the interaction between the average pen and position in the order. Looking at the average pen, we find positive but insignificant effects. When examining the interaction term, we find an insignificant, albeit positive, effect on loan take-up and a negative effect on the likelihood. In general, there seems to be an effect of the info treatment if controlling for path dependency, but effects seem to be too small to reach significance.[11]

To sum up, when looking at between-subject effects, results are not as hypothesized. The public treatment seems to have no effect on loan take up. The information treatment seems to have an influence on loan take up, but only for participants that choose later in the order of participants. As described above, we measured a large number of personal characteristics to examine whether some people are more susceptible to social comparison than others. In the absence of average effects these results are also rather muted. Results on how different personal characteristics interact with peer effects are in Online Appendix I.II. In the next subsection, we examine within-subject results by comparing choices during the experiment to choices in the pre-experiment survey to gain further insights.

3.3 Deviation from Pre-Experiment Choice

It is possible that people simply buy the pen that most aligns with their preferences and that preferences weigh stronger than the effect of the treatment. In this subsection, we, therefore, compare pen choices in the pre-experiment survey to pen choices during the experiment. Hence, we can examine whether our treatments led participants to choose to buy a different pen from the one they claimed to use in everyday life.

This is not a test between stated and revealed preference, as in the online survey, we already ask explicitly for usage and not preference. More importantly, we expect a difference between the two pen choices, even for the control treatment because, in the

[10] Since in the other two treatments there is no order that matters for the decision, we use the subject number instead.

[11] Both corrections have advantages and disadvantages. We prefer the order approach as it allows us to keep all observations, which is not the case if using the mean approach.

experiment, participants are induced to buy the pen that is in line with their earnings. Evidence of this can be seen in Figure 2 above. In this sense, we are most interested in whether the treatments changed the choice of the pen above and beyond the change already induced by the experimental setting. As argued in Subsection 2.3, participants have a large incentive to buy the pen that corresponds to their earnings level, especially if the price is the most important criterion for the choice of pens. Indeed, only about 30% of participants in all treatments chose a pen in line with their reported pen in the pre-experiment survey.

Unfortunately, despite having more survey responses than participants, not all our participants answered the online survey or they used different IDs such that we cannot merge their responses with the experimental data. We are able to match 219 cases that are evenly distributed between treatment groups (for each treatment, we have about 80% who answered the online survey). Furthermore, there are no significant personal differences between those for whom we have valid answers and for those who we do not (see Online Appendix Table II.I).[12] Looking at plain numbers already gives an indication of how participants deviate from the pre-experiment choice. 51% of the control group and 54% of the information treatment group chooses a higher quality pen in the experiment than in the survey. This compares to 43% in the public treatment. Looking at buying a lower quality pen, the mirror pattern of this emerges. 27% in the public treatment buy a lower quality pen, compared to 18% and 17% in the control and information treatment respectively.

In Table 5, we regress the different pen choices on treatments. As expected, there are no significant effects on pre-experiment choices (see Column (1)). However, there are also no significant effects on choices in the experiment (see Column (2)). Interestingly, there is a change in signs for both treatment groups, which means that there is a considerable difference between the two coefficients. We test the difference between coefficients in Column (3). This difference is marginally significant in the public treatment but only if we do not control for imbalances. Nevertheless, there is some indication in these regressions that participants in the public treatment not only take a smaller loan but choose a cheaper pen than they chose during the pre-experiment survey. Similar to Table 4, Table 6 shows the results for the info treatment but controls for order effects. Again, we report differences in coefficients in Column (3). Here, we find highly significant effects. Participants in the information treatment who decide later in the order buy a more expensive pen that is further away from their pre-experimental choice.

[12] Running our main regressions from Table 3 and the order correction for the information treatment as in Table 4 only on the sub-sample of participants who answered the pre-experiment survey, we find qualitatively similar results: there are no significant treatment effects except the significant interaction between information treatment and the order in which participants decide.

Table 5: Pre-Experiment Choice and Adjustment

	Pen Before	Pen After	Difference
	(1)	(2)	(3)
Public Treatment	0.098	–0.061	–0.237
	(0.226)	(0.231)	(0.246)
Info Treatment	–0.227	0.022	0.262
	(0.254)	(0.179)	(0.289)
Mean Control Group	2.00	2.69	0.71
Adj. R-Squared	0.036	-0.014	0.004
Observations	201	248	201

Results from OLS regressions. *Pen Before* is the pen out of five pens that participants have chosen as their preferred pen for everyday use in the pre-experimental online survey, *Pen After* is the pen out of five pens that participants buy in the shopping stage of the experiment, and *Difference* is the difference between *Pen Before* and *Pen After*, which are both measured as categorical variables taking values from one to five. Control treatment is the reference category. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Pre-Experiment Choice and Adjustment, Info Treatment Correction

	Pen Before	Pen After	Difference
	(1)	(2)	(3)
Info Treatment	–0.101	–0.550*	–0.567
	(0.473)	(0.326)	(0.520)
Order	0.035	–0.064***	–0.102***
	(0.045)	(0.023)	(0.036)
Order X Info	–0.022	0.092***	0.134**
	(0.055)	(0.035)	(0.054)
Mean Control Group	2.00	2.69	0.71
Adj. R-Squared	0.028	0.004	0.021
Correction	Order	Order	Order
Observations	201	248	201

Results from OLS regressions. *Pen Before* is the pen out of five pens that participants have chosen as their preferred pen for everyday use in the pre-experimental online survey, *Pen After* is the pen out of five pens that participants buy in the shopping stage of the experiment, and *Difference* is the difference between *Pen Before* and *Pen After*, which are both measured as categorical variables taking values from one to five. *Order* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.4 Leaving Money on the Table

So far, we have seen that participants in the information treatment are more likely to take a loan if they have to make their choice later in the order. At the same time, we find that, in the public treatment, there is no significant effect on loan take up. But we find some indication that the choice in the experiment compared to the choice in the pre-experiment survey is a cheaper pen. We here investigate this further.[13]

We examine whether participants leave money on the table by buying a cheaper pen than the one they could afford according to their earnings. In Table 7, we test if people leave money on the table and how much they leave. Results are very clear; participants in the public treatment buy significantly lower quality pens and are significantly more likely to do this than those in the control treatment, as shown in Columns (1) and (2). As we can see when we introduce interaction terms between performance and the treatment in

[13] This part of the analysis was not pre-registered. It is exploratory and performed to further explore the non-hypothesized results above.

Columns (3) and (4), this effect is driven by high performers, which means we have an asymmetry: high performers in the public treatment are adjusting downwards but low performers do not adjust upwards.

Table 7: Buying a Lower Quality than Affordable

	Lost Amount (1)	Lost Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Public Treatment	0.115***	0.076**	–0.066	–0.033
	(0.043)	(0.030)	(0.070)	(0.044)
Info Treatment	0.068	0.067	–0.018	0.005
	(0.057)	(0.055)	(0.043)	(0.050)
Performance			0.002	0.002
			(0.003)	(0.003)
Performance X Public			0.026*	0.016**
			(0.014)	(0.008)
Performance X Info			0.013	0.009
			(0.009)	(0.008)
Mean Control Group	0.038	0.043	0.038	0.043
Adj. R-Squared	-0.003	-0.006	0.018	0.001
Observations	248	248	248	248

Results from OLS regressions. *Lost Amount* is the amount (in Euro) by which the pen a participant buys is cheaper than the one she could afford, *Lost Dummy* equals 1 if the participant buys a cheaper pen and zero otherwise. *Performance* is the rank of the participant obtained by counting how many questions in the IQ-test are answered correctly in comparison to all the other participants in each session. Control treatment is the reference category. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As in the previous subsections, we repeat these calculation for the info treatment by controlling for order effects. Results are shown in Table 8. We can see here that people in the information treatment are also more likely to leave money on the table than people in the control group. However, this effect reduces slightly for those that make their decision later in the order. Thus, in contrast to the public treatment, there is adjustment from below as well as above, although adjustment from above shows larger and more significant effects.

Table 8: Buying a Lower Quality than Affordable, Info Treatment Correction

	Lost Amount (1)	Lost Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Info Treatment	0.187**	0.183**	0.203**	0.233**
	(0.078)	(0.085)	(0.091)	(0.110)
Order	0.012*	0.012*	0.014*	0.013*
	(0.006)	(0.006)	(0.008)	(0.007)
Order X Info	–0.020**	–0.020*	–0.021**	–0.026**
	(0.009)	(0.010)	(0.010)	(0.012)
Mean Control Group	0.038	0.043	0.038	0.043
Adj. R-Squared	0.003	0.005	-0.001	0.003
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Results from OLS regressions. *Lost Amount* is the amount (in Euro) by which the pen a participant buys is cheaper than the one she could afford, *Lost Dummy* equals 1 if the participant buys a cheaper pen and zero otherwise. *Order* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To sum up, unlike hypothesized, we find that participants in the public treatment are significantly more likely to leave money on the table rather than going into debt to buy a higher quality pen. Participants in the information treatment, on the other hand, adjust their purchases according to those that chose before them. This is as we hypothesized and is in line with previous literature.

3.5 Results on Effort Provision

As described above, the choice of pen is followed by the slider task. It is our expectation that participants who took a loan in the consumption stage will try to make up for their loss in income by exerting additional effort in the slider task. We base this expectation on the belief that participants come to the lab in order to make money and that they will perceive the amount taken out as a loan to be a loss that they want to make up for. In Table 9, however, we find exactly the opposite: the amount of loan taken is significantly negatively related to effort. The treatments themselves seem to have no additional effect on the effect exerted in the slider task. In Column (2), we can see that there is a positive relationship between performing well in the IQ-quiz and performing well in the slider task.

Table 9: Effort and Loan Take-Up

	Effort Slider Task (1)	Effort Slider Task (2)	Effort Slider Task (3)
Public Treatment	1.064	0.952	1.931
	(1.429)	(1.421)	(1.524)
Info Treatment	2.131	2.053	2.145
	(1.515)	(1.573)	(1.716)
Loan Amount	–3.397***	–3.254***	–2.023*
	(0.840)	(0.879)	(1.215)
Performance		0.348*	0.359*
		(0.190)	(0.193)
Loan Amount X Public			–4.023**
			(1.588)
Loan Amount X Info			–0.748
			(2.378)
Mean Control Group	22.61	22.61	22.61
Adj. R-Squared	0.018	0.026	0.027
Observations	248	248	248

Results from OLS regressions. *Effort Slider Task* is the number of correctly adjusted sliders in the slider task. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford and *Performance* is the rank of the participant obtained by counting how many questions in the IQ-test are answered correctly in comparison to all the other participants in each session. Control treatment is the reference category. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

One can only speculate about the reasons behind this. It is, for example, possible that some people have a high general ability. Alternatively, low performers may have been demotivated by their low performance and, as such, put little effort into the slider task. In Column (3), we see that the effect of having taken a loan on putting no effort into the slider task is largest in the public treatment. From the table above it is not clear, why exactly we find these effects. However, these results combined indicate that having taken a loan in the consumption round may have demotivating effects later in the experiment. Given the low loan take-up, the payoff from the slider task is too generous. Only two persons did not manage to work enough to repay their loans, all the others mostly obtained a surplus from the slider task. This makes it hard to draw meaningful conclusions, since participants did not have to work more to repay their debts. Still, evidence from the slider task might help to refute one explanation why some people might have left money on the

table. It could be the case that high performing participants want to behave pro-socially by buying a cheaper pen, leaving more money for the experimenters to conduct more research.[14] Such participants could also exert less effort in the slider task to leave money in the experimental budget. However, we do not find a significant relationship between leaving money on the table and performance in the slider task. We discuss, in our opinion, more likely reasons for leaving money on the table in Section 5.

4 Robustness

In this section, we outline a number of changes to the econometric specifications that we use in the main body of this paper. These changes show that our main results are robust.

Controlling for the Pre-Experiment Choice There are slight, albeit not significant, imbalances across treatments in the pen participants chose in the online survey. Therefore, we control for this pre-experimental choice in Online Appendix Tables II.II and II.III and test if our main results are robust to this inclusion. Although our sample size is smaller, as not all participants answered the online survey, results regarding loan take-up in the two treatments stay the same. There is no significant positive effect of the public treatment on taking a loan. In this specification, coefficients are larger in size but all of them are negative. For the information treatment, we again find a significant and positive interaction between treatment and order of deciding. The effect is furthermore of a similar size than before. Reassuringly, the more expensive the chosen pen in the online survey is, the larger is the loan amount in the lab. This indicates that participants did not give joke answers in the online survey, which is not incentivized, but reported truthfully.

Deviation from Pre-Experiment Choice - Dummy Given that participants deviate in both directions from their pre-experimental choice, we test if, in total, the treatments make it less or more likely to buy the pen that one actually prefers. In Online Appendix Table II.IV, we find a small negative effect on deviating from the preferred pen for both treatments, which is, however, not significant. If we control for order effects, the treatments seem to increase the likelihood of buying the preferred pen for first movers, but this effect fades out with the place in the order. Again, these effects are not significant as standard errors are extremely large. In general, the table supports our previous results as found in Tables 5 and 6.

Using a Different Order in Control and Public Treatment To correct for path depen-

[14] We thank two anonymous referees for pointing this out.

dence in the information treatment, we control for the order in which participants decide. However, since participants decide simultaneously in the control and public treatment, we have to use an artificial order for their choices. For our main results, we use the most straightforward order our data provides, which are the individual subject numbers that z-Tree is assigning to participants within each session. As a robustness check, we use a different ordering that is based on actual orders in the information treatment. For each potential number of total participants in the session, which are 9, 10, 11, or 12, we randomly draw one information treatment session and implement its ordering in the other two treatments. Results are presented in Online Appendix Table II.V. The interaction term between loan take-up and information treatment is almost the same in size and significance as the term in Table 4. The coefficients for leaving money on the table are smaller and not significant anymore. However, they still point in the same direction as before in Table 8, and their size is still large.

Excluding Participants with Low Understanding of Signaling It is possible that some participants do not understand the premise of the experiment, i.e. they are unaware that they can signal high status by taking a loan in the public treatment. To check this, we rerun our main regressions under the following conditions: (i) excluding participants who answered the IQ question incorrectly that most participants got right, (ii) excluding participants who got no question on the cognitive reflection test right, and (iii) excluding participants with low self-monitoring scores. Results are shown in Table II.VI, Table II.VII, and Table II.VIII in the Online Appendix. Obviously, the sample gets considerably smaller and there are some concerns regarding the relationship between earnings, the necessity to signal, and IQ performance, which might render these exclusions unreliable. However, we do not find any qualitative difference from our main results. In addition, there are no significant relationships between IQ score and the comprehension questions, nor between self-monitoring and the comprehension questions. Hence, we believe that the results in Section 3 are not driven by poor understanding of signalling.

5 Discussion

We have two main findings regarding debt taking and consumption choices. First, participants buy worse quality pens than they can afford, thus, leaving money on the table when their choices are public to others. Second, we also find this effect in the information treatment. However here, participants adjust from both sides. So, even though the adjustment from above seems to be much larger than from below, we find evidence for conformity. This leads us to conclude that signaling intelligence to others is not a

primary concern for participants. We discuss four potential reasons for the observed findings in the public treatment. These are "standing-out-aversion," "alternative equilibrium," "smarty-pants-effect" and "blame aversion."

"Standing-Out-Aversion" Jones and Linardi (2014) formulate a simple model and find evidence for what they call wallflowers: Some people are averse to being seen as too selfish or as too generous. They do not want to stand out with their level of generosity. Therefore, they adjust their action to what they believe the average is doing. If we directly translate this model from reputational to social image concerns and apply it to our experimental design, we should see that loan take-up is the highest in the public treatment. Given that the payout and performance structure is common knowledge, we assume that participants expect that the average person buys a two-star or three-star pen. Thus, low-performing persons would have to take a loan to match the mean decision. This is not what we find. We do find that high-performing persons leave money on the table to buy a cheaper pen in the public treatment. However, this asymmetry does not support "standing out aversion" as an explanation. Further evidence against this explanation is that we find no differential effects for females and males (see Online Appendix Table II.IX). Jones and Linardi (2014) find females are especially likely to be wallflowers and, if anything, our coefficients point in the exact opposite direction.[15]

Alternative Equilibrium in Combination with Forms of Sophisticated Behavior It is possible that participants' thinking is more sophisticated than originally anticipated and that this may drive the results. In the public treatment, participants could expect that only low performing individuals purchase high quality pens. In this case, high performing individuals would be inclined to purchase low quality pens and, thus, to leave money on the table. At the same time, sophisticated low performing individuals would also choose low quality pens as this would signal high status rather than low status. We examine the data to see if there is evidence for this behavior. There is a clear and positive correlation of 0.71 between the relative rank from the IQ test and the quality of the pen purchased in the public treatment. We do not have an exact measure of participant's sophistication. However, to address the second point, we can use the comprehension questions. The mean pen quality for the lower half of performers in the public treatment is 1.78. When we look at the mean pen quality for individuals that got all comprehension questions right,

[15] Another consideration is that participants in our design are not exactly standing out when buying a low or high-quality pen as a quarter of participants is expected to do so given the payoff categories. Still, since we did not elicit beliefs about what participants think others will do, we do not know whether some persons might think that they would be the only one making extreme choices. However, in this case, even more participants in the public treatment should be willing to take a loan.

i.e. for those who are sophisticated, this is 1.75. Although only suggestive, this evidence makes it unlikely, that there is a systematic trend of buying downward and that there is an alternative equilibrium where only low performers that are unsophisticated buy high quality pens.

"Smarty-Pants-Effect" Our participants avoid signaling higher intelligence by not taking a loan and buying a cheaper pen than they can afford. McManus and Rao (2015) find similar results to ours in a very different experiment. They present three explanations for this avoidance, of which two might be present in our setting. The first might be what they call "smarty-pants-effect," which means that participants neither want to appear smarter than their peers nor to be perceived as arrogant. This same effect is more prominently known as the "acting white" effect (e.g. Austen-Smith and Fryer, 2005; Bursztyn et al., 2019). This is a possible explanation for our results. However, we believe that this is unlikely to be the case in a student population such as this one. Generally speaking, high intelligence tends to be valued. Friedrichsen et al. (2018) even find that low performance in a general knowledge quiz is associated with shame, using the same subject pool.

"Blame Aversion" An alternative explanation is what we call "blame aversion," which relates to social preferences. There is evidence that persons care about negative externalities of their own performance on others in cases where relative performance determines payout (e.g Bandiera et al., 2005). In our experiment, high-performing participants are the reason why low-performing participants can only afford a low quality pen. Thus, inequality is inevitable, and self-esteem damage is done. However, it might be the case that high performers do not want to publicly take the blame for others being worse off and, therefore, pretend to be a low performer. We believe blame aversion being the more likely explanation for our results in the public treatment than smarty-pants-effect. In any case, participants in the public treatment still exhibit social image concerns, just not the ones we anticipated.

6 Conclusion

Since the number of over-indebted households is rising worldwide, it becomes increasingly important to understand the drivers behind this process. This paper contributes to the emerging literature on household borrowing behavior. Research has shown that social comparison is one of the reasons leading to increased debt taking, which in turn leads to overindebtedness (Agarwal et al., 2019; Georgarakos et al., 2014). In this paper, we analyze two potential channels behind the effects of social comparison on debt taking.

It is our aim to disentangle these two channels that underlie social comparison. Therefore, we take our research question to the lab, as it is difficult to do this in other settings. We design two treatments through which we want to separately examine social image concerns and peer information. While the former relates to how an individual wants to be perceived by others, the latter relates to how an individual themselves perceives the decision of others. Few studies disentangle these two effects.

Our results on how social comparison might affect borrowing are not as we originally hypothesized. Social image concerns lead to underspending and hence leaving money on the table in our setup. This is the exact opposite of what we hypothesized based on the literature. We discuss two potential reasons: first, participants do not want to be perceived as more intelligent, which would be unexpected for a student population. Second, high performers do not want to be blamed for the failures of others. We hence, do believe that borrowing decisions are affected by the public treatment, but not in the way that we hypothesized. We acknowledge that these findings may be particular for our setting and might differ in other environments where, for example, status is not only defined by intelligence. Furthermore, no perfect correlation between success of one group and failure of another group exists in other settings.

Regarding peer information, we find hypothesized results. There is convincing evidence for an intrinsic inclination to conform, which leads to more debt taking by individuals with low performance and to leaving money on the table for those in the upper tail of the performance distribution. In our setting, adjustment from above is stronger than adjustment from below.

One clear conclusion from this study is that social comparison is important in decision making regarding debt. These results also highlight that social image concerns can take many forms. In order to contemplate possible policy interventions, more research needs to be conducted on how social image concerns are influenced by the setting and people involved. The virtual world opened up the possibility to convey social image to an unprecedentedly large amount of people. At the same time, online platforms constantly present their users the consumption choices of others. Still, how virtual, mostly unknown "peers" affect consumption is not yet well understood.

The possible biggest caveat of our study is that borrowing in the lab is highly artificial, since participants cannot leave the lab indebted. Still, we believe that our experimental design is different from standard spending decisions in the lab and that participants thought of the possible loan they could take as creating temporary debt. A further caveat could be the choice of good that can be bought in the lab: the upper end of the distribution adjusting more might be purely driven by the fact the decision only involves simple pens. It cannot be expected that the rich downward adjust their consumption when it comes

to products where quality differences matter much more. Given the extensive research on status consumption in the last 120 years, future research should concentrate more on the effect of social comparison on debt-financed consumption, similar to what is done in the domain of pro-social behavior.

Our findings highlight that not only are peer effects in household borrowing underresearched but also the intrinsic mechanisms underlying borrowing decisions leave a lot of room for further studies. Especially since the consequences of "sub-optimal" decisions can be grave. Conformity affects decisions on both sides of the distribution in our experiment as participants deviate from their intrinsic preference elicited before the experiment. In real life, conformity might disadvantageously hurt low income households. Especially in countries with high income inequality, like emerging markets, conforming to an average level of consumption might lead to severe financial distress. Research looking at how inequality in neighborhoods affects financial distress seem to confirm this concern.

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Online Appendix to accompany "The Effect of Social Comparison on Debt Taking: Experimental Evidence"

Contents:

I Susceptibility to Social Comparison

In the following subsections, we present all the hypotheses as stated in our pre-analysis plan, including those on which personality characteristics are more or less susceptible to social comparison effects. Subsequently, we discuss the results on the personality types.

I.I Hypotheses

To answer our main research question, we look at the difference in the amount and the probability of loans taken between those in the private treatment and those in the other two treatments. Thus, these variables focus on the differences between the amount that someone should have spent according to standard economic predictions and the amount that someone actually spends. Furthermore, we look at within-subjects differences in what participants reported to be their quality preference for the pen in our online survey and what they actually buy during the experiment. To assess which personalities are more susceptible to social comparison effects, we interact the personality traits with our treatments. Finally, we also analyze the amount of effort exerted in the slider task to investigate who is willing to work more in the future to actually avoid financial distress because of socially contingent consumption.

Question 1: *"Are people willing to pay out of their future income because of social image/status concerns?"*

We expect that at least some people are willing to do so. As previously explained, buying a low quality pen is a credible signal for being a "lower" cognitive ability type, as it can be directly linked to worse performance in the test of intelligence. Since we assume that cognitive ability is a desirable trait for our student sample, for some persons the additional benefit of being perceived as having higher cognitive ability is large enough to offset the potential costs of borrowing or of "working more" (see hypothesis 1a). In our experiment, participants can borrow money without interest, reducing potential costs of borrowing to general opportunity costs of spending more instead of keeping money. Some participants in the public treatment are, thus, willing to use their future income to buy a higher quality pen than they can afford in order to hide their true performance. Since social image concerns can only arise when individual decisions are made publicly, these concerns neither arise in the private nor the information treatment.

Hypothesis 1: *"Participants in the public treatment are more likely to take out a loan and take out a higher loan amount to buy a higher quality pen than participants in the*

private treatment."

Participants in the public treatment who take out a loan, because they want to convey a certain type, end up with less money after the shopping round than their control treatment counterparts who cannot engage in socially contingent consumption. Assuming only weak fatigue, the marginal rate of substitution of not exerting effort in the slider task for money should be larger for those subjects, as they have a debt on their accounts. Differently speaking, persons who take out a loan might be willing to work more because they want to settle their debts.[1] Determining if individuals with a loan exert more effort is interesting because, in real life, higher consumption could be financed by debt or by working more (e.g. Neumark and Postlewaite, 1998; Bowles and Park, 2005). Hence, some of our participants could already have internalized working more in the slider task to take out a higher loan.

Hypothesis 1a: *"Participants in the public treatment will exert more effort in the slider task than participants in the private treatment, because they took out a higher loan before."*

Question 2: *"Can the peer effect on visible consumption mostly linked to social image/status concerns partly be explained by peer information?"*

A different explanation why people adjust their consumption to peers is that they are intrinsically motivated or because they receive information about the usefulness/quality of a product. Intrinsic motivation could be a form of self-image concern, a desire to imitate or a desire to conform to others. Pure information about the quality is especially important if the individual is not familiar with the product. To analyze whether the effect of peer information is comparable to that of social image concerns, we designed the information treatment in such a way that only new information but no social image concerns can arise.[2] Our prediction is that peer information only has a small effect on the decisions in our setting. The pens we use are trivial goods and quality differences are comparatively small, which is why we expect the intrinsic and informational gain to be small. However, we acknowledge that this is not necessarily true for goods that are usually considered in field studies on conspicuous consumption, e.g. cars, travel destinations, restaurant

[1] An alternative explanation would be that these persons do not want to lose money they already have in their mental accounts. They do not like the feeling of creating a debt that eventually will be deducted from their participation fee, which is already part of their endowment.

[2] Given our experimental design, observations in the information treatment within a session are path dependent. We try to control for this issue in our analysis.

visits, and so forth. In this sense, our treatment for information effects lies at the lower bound. Finding significant results would possibly imply that a substantial share of visible consumption is actually not driven by conspicuous consumption.

Hypothesis 2: *"Participants in the information treatment will take out a higher loan than participants in the private treatment, but a smaller loan than participants in the public treatment."*

Hypothesis 2a: *"Participants in the information treatment will exert more effort in the slider task than participants in the private treatment, but less than participants in the public treatment."*

Question 3: *"Are there certain types of personality that correlate with larger socially contingent consumption?"*

Since cognitive reflection is related to standard IQ measures, we expect small effects in our setting. Participants with high cognitive reflection are expected to perform well in our intelligence task and, therefore, can buy high quality pens without needing to take out a loan. This reduces the difference between the control and the other treatments. Nevertheless, we hypothesize to find a negative relation between CR and susceptibility to social image concerns after controlling for performance. Royzman et al. (2014) find that moral values of reflective persons are more independent of existing social norms. We see this as indication of putting less value on what other people think about oneself.

Hypothesis 3a: *"Participants with higher cognitive reflection are less susceptible to social image concerns."*

We expect higher internal locus of control to decrease the reliance on social networks and perceived peer pressure, because it relates to the belief that individuals are responsible for their lives themselves.

Hypothesis 3b: *"Participants with rather internal locus of control are less susceptible to social image concerns."*

Self-esteem and power, the capability to control other people, are related concepts and power affects self-esteem (Wojciszke and Struzynska-Kujalowicz, 2007). Since research shows that feeling powerful decreases conspicuous consumption, we expect an analogous

effect for self-esteem.

Hypothesis 3c: *"Participants with higher self-esteem are less susceptible to social image concerns."*

High self-monitors adjust their self-presentation more than low self-monitors to signal a desired type. High self-monitors have a more precise estimate of the social desirability of an action and care more about being perceived as a higher type.

Hypothesis 3d: *"Participants with higher self-monitoring are more susceptible to social image concerns."*

Looking at the Big Five personality traits, we concentrate on the traits of extraversion, openness, and agreeableness. For the remaining two traits, we do not have a clear prediction. Extraversion is shown to be positively correlated to status consumption of low status individuals (Landis and Gladstone, 2017). Therefore, we expect it to be related to social image concerns. For openness and agreeableness, we only formulate hypotheses regarding their effect on responding to peer information. A high level of openness means to be open to new experiences, ideas, and variety seeking. Therefore, openness drives participants away from the mean decision of others, which is considered as not innovative and unexciting. Agreeableness is closely related to the desire for conformity and cooperation, which is why we predict it to be related to anchoring the own decision on others' decisions.

Hypothesis 3e: *"Participants with a higher level of extraversion are more susceptible to social image concerns."*

Hypothesis 3f: *"Participants with a higher level of openness will anchor their decision less to the average decision in the information treatment than those with a lower level."*

Hypothesis 3g: *"Participants with a higher level of agreeableness will anchor their decision closer to the average decision in the information treatment than those with a lower level."*

I.II Results on Personality Types

For all characteristics listed in Table I.I, we only derived hypotheses for the interaction with the public treatment and, therefore, do not report coefficients for the information

treatment. We first look at cognitive reflection (CR). Since we find a highly significant correlation between CR and actual performance in the IQ-quiz, we additionally control for performance. As expected, a better performance is significantly negatively correlated with taking a loan. Interestingly, for the control treatment, a higher CR is significantly positively related to loan take-up. However, we find a negative effect of cognitive reflection on loan take-up in the public treatment. The effect is rather small and only marginally significant on the extensive margin. However, if we do not control for possibly endogenous self-esteem, the effects are stronger and highly significant. In general, the interaction effect is robust to various specifications and more than offsets the positive effect of CR in the control. Because this study is slightly under-powered to estimate effects of this size, we are still cautious in interpreting the results. Still, it seems that individuals with higher cognitive reflection do adjust their decision because of social image concerns, but in opposite direction to the others. In this sense, we have to reject hypothesis 3a.

The results for the interaction between public treatment and locus of control (LOC) are shown in the second panel of Table I.I. Internal LOC is also correlated with performance but to a smaller extent. We do not find a significant interaction effect for the probability to take up a loan at all, although the coefficient points in the right direction.[3] Given the rather large standard errors and the imbalance of LOC between control and public, we view our results as inconclusive. Thus, we also cannot confirm hypothesis 3b.

For global self-esteem (GSE), we find an insignificant interaction term and a rather small effect size. As participants with higher GSE are overly represented in the public treatment or higher GSE might be induced by the treatment, we would expect larger effects in negative direction: The treatment could give those persons who performed well in the quiz a confidence boost, who can now announce this publicly (and vice versa). Actually, there is a mild correlation between quiz performance and GSE. However, this should increase the effect size in favor of our hypothesis, which is not the case. Thus, we reject hypothesis 3c.

[3] If we apply wild cluster bootstrap, we additionally find an overall significant positive effect of LOC at the 10% level.

Table I.I: Personality and Loan Take-Up

	Loan Amount	Loan Dummy
Cognitive Reflection		
CR	0.100***	0.094***
	(0.031)	(0.028)
CR X Public	–0.128	–0.116*
	(0.084)	(0.065)
Public Treatment	0.249	0.244
	(0.210)	(0.153)
Locus of Control		
LOC	0.056	0.054
	(0.082)	(0.036)
LOC X Public	–0.094	–0.101
	(0.104)	(0.064)
Public Treatment	–0.012	0.013
	(0.138)	(0.093)
Self-Esteem		
GSE	–0.100	–0.049
	(0.080)	(0.071)
GSE X Public	–0.047	–0.027
	(0.131)	(0.092)
Public Treatment	0.006	0.023
	(0.146)	(0.102)
Self-Monitoring		
SM	0.046	0.023
	(0.049)	(0.036)
SM X Public	0.017	0.020
	(0.072)	(0.069)
Public Treatment	–0.016	0.003
	(0.137)	(0.096)
Extraversion		
EV	–0.030	–0.001
	(0.055)	(0.048)
EV X Public	0.148	0.093
	(0.098)	(0.066)
Public Treatment	–0.010	0.013
	(0.134)	(0.098)
Observations	248	248

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. Control treatment is the reference category. Coefficients on information treatment not reported. Controlled for variables with significant differences between control and public treatment group: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. All regressions except for extraversion additionally controlled for performance in the IQ-Quiz. SE in parentheses, bootstrapped and clustered on session level.

* p < 0.10, ** p < 0.05, *** p < 0.01

Similar as for GSE, we do not find any effect for self-monitoring (SM). The effect size is fairly small and effects are not significant at all. Interestingly, SM is negatively correlated to quiz performance, which even should increase the potential effect. Based on these results, we also reject hypothesis 3d.

The last panel in the table presents the results on extraversion (EV). The effects go in hypothesized direction, but are never significant. Effect sizes, though, are of moderate size (Cohen's d $\sim$ 0.24) and p-values are "flirting with significance." As previously noted, our study is under-powered for this effect size and, hence, we are hesitant to reject hypothesis 3e but also cannot confirm it, which means results are inconclusive.

Hypotheses 3f and 3g address the anchoring of decisions to others in the information treatment. To measure anchoring, we look at the difference between pre-experiment choice and actual choice, interacting the information treatment with the two personality traits. In Table I.II, we first investigate whether persons with a higher level of openness deviate less from their individual preference as they receive information about others. In contrast to the previous table, we now do not report coefficients on the public treatment but only on the information treatment. We find a mild correlation between openness and performance in the IQ-quiz and, therefore, again control for performance. First, we notice a strange differential effect for the pre-experimental choice. In general, a higher level of openness is related to choosing a more expensive pen in the online survey but the interaction has a large significantly negative effect. Eventually, persons with a higher level of openness who were assigned to the information treatment choose a cheaper pen in the survey. However, as treatment assignment is random, this is most likely an artifact of the small sample size. Nevertheless, we find a considerably large positive effect for the actual choice and the difference between the choice before and during the experiment. This means that we have to reject hypothesis 3f, as apparently it is exactly the opposite: persons with a higher level of openness deviate more from their pre-experiment preference.

Panel 2 in Table I.II, shows the effect of agreeableness on anchoring. Here, we do not find significant effects and the difference between pre-experimental and actual choice is small in size. Therefore, we also reject hypothesis 3g.

Table I.II: Personality and Adjustment

	Pen Before	Pen After	Difference
Openness			
OP	0.283**	0.034	–0.287**
	(0.117)	(0.059)	(0.113)
OP X Info	–0.371*	0.158	0.570**
	(0.210)	(0.109)	(0.252)
Info Treatment	–0.192	0.003	0.194
	(0.238)	(0.147)	(0.252)
Agreeableness			
AG	0.118	0.083	0.009
	(0.158)	(0.124)	(0.159)
AG X Info	–0.162	–0.154	–0.057
	(0.252)	(0.185)	(0.327)
Info Treatment	–0.235	0.017	0.255
	(0.257)	(0.195)	(0.300)
Observations	201	248	201

Results from OLS regressions. *Pen Before* is the pen out of five pens that participants have chosen as their preferred pen for everyday use in the pre-experimental online survey, *Pen After* is the pen out of five pens that participants buy in the shopping stage of the experiment, and *Difference* is the difference between *Pen Before* and *Pen After*, which are both measured as categorical variables taking values from one to five. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and information treatment group: education, being a student, risk preference, know someone, persons in sessions, and self-esteem. Regressions for openness additionally controlled for performance in the IQ-Quiz. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

II Additional Results

Figure II.I: Pre-Experimental Choices - Pens ('Kugelschreiber'), Lip-Balms ('Lippenpflegestift'), and Folders ('Mappen')

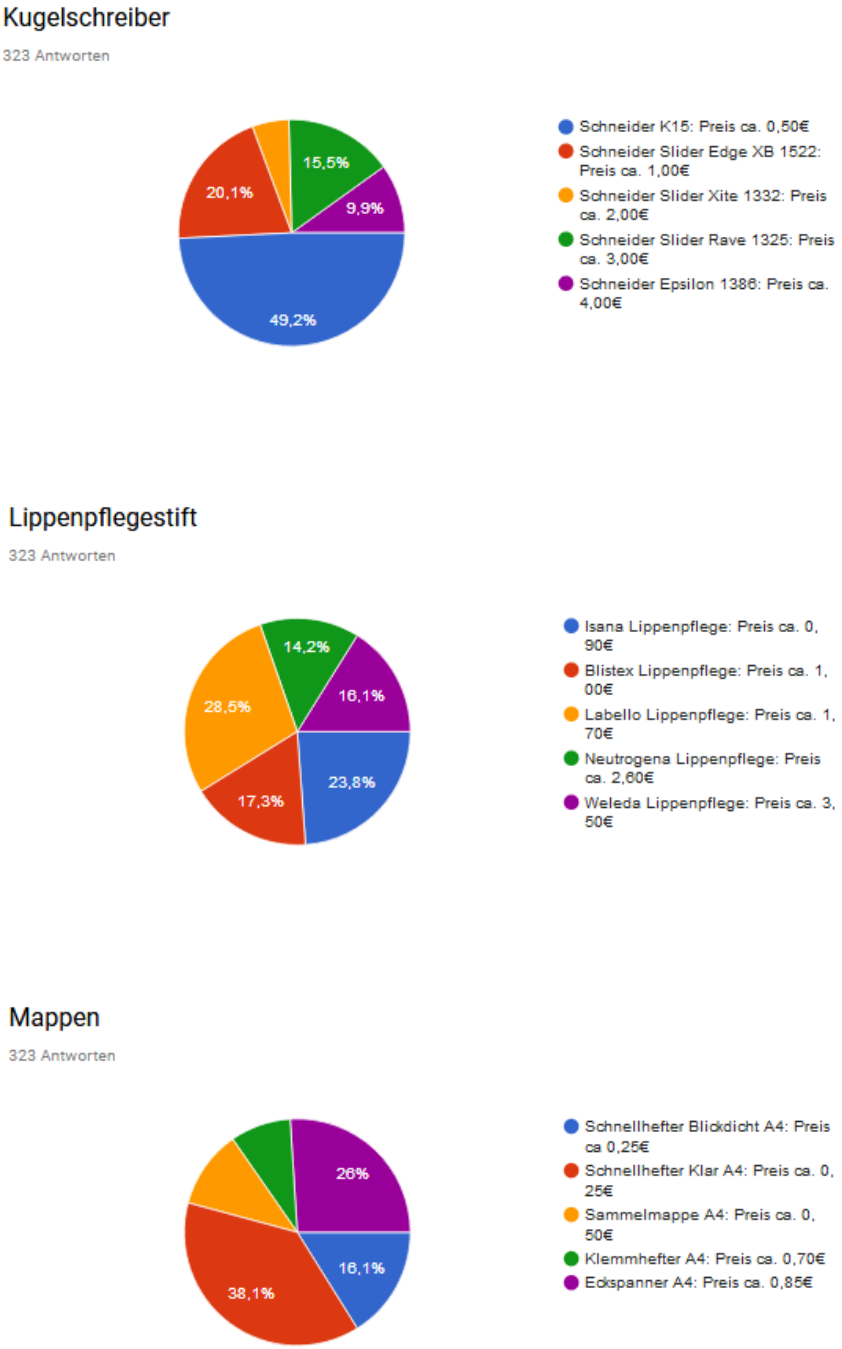


Table II.I: Descriptive Statistics across Survey Participation

	Full Sample (1)	Online Survey (2)	No Survey (3)	Difference (4)
Male	0.48	0.47	0.52	0.05
Age	22.86	22.87	22.80	−0.06
Education	3.36	3.36	3.35	−0.00
Students	0.97	0.96	0.98	0.02
Semester	3.68	3.51	4.39	0.88
Student Job	0.28	0.26	0.33	0.07
Mthl. Income	688.36	692.70	669.86	−22.84
Risk Preference	5.34	5.39	5.15	−0.24
Lab Experience	1.89	1.87	1.96	0.09
Know Someone	0.31	0.34	0.20	−0.14*
Persons in Session	11.33	11.26	11.61	0.35***
Correct Control Questions	4.74	4.76	4.67	−0.09
Correct Quiz Questions	4.43	4.43	4.43	0.00
Financial Literacy	4.59	4.60	4.53	−0.07
CR	1.91	1.91	1.90	−0.01
Conscientiousness	−0.01	−0.01	−0.01	0.00
Neuroticism	0.03	−0.01	0.20	0.21
Extraversion	−0.02	−0.03	0.01	0.04
OP	−0.02	−0.00	−0.11	−0.11
Agreeableness	−0.01	0.02	−0.16	−0.18
Self-Esteem	−0.01	0.03	−0.18	−0.21
Locus of Control	0.00	0.05	−0.21	−0.26
SM	−0.02	−0.04	0.09	0.14
Observations	270	219	51	270

Column (1) reports mean values for the whole sample, column (2) for those participants who completed the only survey, and column (3) for those who did not. Column (4) reports the difference in mean values between those who completed and those who did not completed the only survey. Variables: *Male* is a dummy that equals 1 if the participant is male and zero otherwise; *Age* is the age of the participant in years; *Education* measures the educational level on a scale from 1 "Lower secondary education" to 4 "PhD"; *Students* is a dummy that equals 1 if the participant is a student and zero otherwise; *Semester* is the current study semester of a participant who is a student; *Student Job* is a dummy that equals 1 if a participant who is a student has a sideline; *Mth. Income* is the monthly income of the participant in Euro; *Lab Experience* measures how often a participant has participated in lab experiments prior to our experiment; *Know Someone* indicates if the participant either does not know or knows another person in the session or is not sure about this; *Persons in Session* is the number of participants in each session; *Correct Control Questions* is the number of correctly answered comprehension questions we gave participants before the first stage of the experiment; *Correct Quiz Questions* is the number of correctly answered IQ questions in the first stage of the experiment; *Risk Preference, Financial Literacy, Cognitive Reflection, Conscientiousness, Neuroticism, Extraversion, Openness, Agreeableness, Self-Esteem, Locus of Control, and Self-Monitoring* are personality traits measured as explained in Subsection 2.6.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; p-values are obtained using twosided t-tests.

Table II.II: Effects on Loan Take-Up - Pre-Experiment Choice

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Public Treatment	–0.068	–0.006	–0.083	–0.021
	(0.125)	(0.088)	(0.146)	(0.094)
Info Treatment	0.078	0.030	0.036	–0.017
	(0.135)	(0.076)	(0.133)	(0.079)
Pen Before	0.084**	0.056***	0.064*	0.039
	(0.036)	(0.021)	(0.038)	(0.025)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	0.022	0.024	0.032	0.035
Controls	No	No	Yes	Yes
Observations	219	219	201	201

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. *Pen Before* is the pen out of five pens that participants have chosen as their preferred pen for everyday use in the pre-experimental online survey. Control treatment is the reference category. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.III: Effects on Loan Take-Up, Info Treatment Correction - Pre-Experiment Choice

	Loan Amount	Loan Dummy	Loan Amount	Loan Dummy
	(1)	(2)	(3)	(4)
Info Treatment	–0.151	–0.165	0.031	0.180
	(0.249)	(0.109)	(0.586)	(0.476)
Order	–0.016	–0.002		
	(0.022)	(0.010)		
Order X Info	0.030	0.024*		
	(0.027)	(0.013)		
Pen Before	0.067*	0.040	0.014	0.019
	(0.038)	(0.026)	(0.032)	(0.027)
Mean Prev. Pens			0.086	0.124
			(0.137)	(0.141)
Mean X Info			–0.005	–0.067
			(0.211)	(0.175)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	0.023	0.035	0.006	0.026
Correction	Order	Order	Mean Pen	Mean Pen
Observations	201	201	183	183

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. *Order* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. *Pen Before* is the pen out of five pens that participants have chosen as their preferred pen for everyday use in the pre-experimental online survey. *Mean Prev. Pens* measures the mean quality of pens that have been bought before a participants in the information treatment decides. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.IV: Deviation from Pre-Experiment Choice - Dummy

	Pre-Experiment = Experiment			
	(1)	(2)	(3)	(4)
Public Treatment	–0.012	–0.051	0.198	0.216
	(0.065)	(0.078)	(0.167)	(0.196)
Info Treatment	–0.020	–0.027	0.103	0.135
	(0.080)	(0.102)	(0.166)	(0.203)
Order			0.012	0.015
			(0.015)	(0.014)
Order X Info			–0.020	–0.025
			(0.023)	(0.025)
Mean Control Group	0.312	0.312	0.312	0.312
Adj. R-Squared	-0.009	-0.048	-0.012	-0.049
Controls	No	Yes	No	Yes
Correction	No	No	Order	Order
Observations	219	201	219	201

Results from OLS regressions. *Pre-Experiment = Experiment* is a dummy indicating if the pen a participant buys in the experimental session is the same as the one that was chosen as preferred pen in the pre-experimental online survey. *Order* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. Control treatment is the reference category. Coefficients on the interaction between order and public treatment not reported. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.V: Using a Different Ordering

	Loan Amount (1)	Loan Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Info Treatment	0.016	–0.161	0.094	0.138
	(0.193)	(0.113)	(0.086)	(0.104)
Order 2	0.004	–0.007	–0.004	–0.002
	(0.021)	(0.005)	(0.004)	(0.004)
Order 2 X Info	0.007	0.025*	–0.003	–0.011
	(0.029)	(0.013)	(0.008)	(0.010)
Mean Control Group	0.220	0.172	0.038	0.043
Adj. R-Squared	0.043	0.043	0.009	-0.001
Correction	Order 2	Order 2	Order 2	Order 2
Observations	248	248	248	248

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. *Lost Amount* is the amount (in Euro) by which the pen a participant buys is cheaper than the one she could afford, *Lost Dummy* equals 1 if the participant buys a cheaper pen and zero otherwise. *Order 2* is a variable that measures how many other participants in the session made their consumption choice before a participant in the information treatment decides. Control treatment is the reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.VI: Excluding Participants Who Get the Easiest IQ Question Wrong

	Loan Amount	Loan Dummy	Loan Amount	Loan Dummy
	(1)	(2)	(3)	(4)
Public Treatment	0.052	0.037	0.059	0.025
	(0.100)	(0.095)	(0.120)	(0.099)
Info Treatment	0.066	–0.012	0.080	–0.014
	(0.076)	(0.069)	(0.080)	(0.072)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	-0.007	-0.007	-0.007	0.021
Controls	No	No	Yes	Yes
Observations	207	207	193	193

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. Control treatment is the reference category. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.VII: Excluding Participants with Low Cognitive Reflection Score

	Loan Amount	Loan Dummy	Loan Amount	Loan Dummy
	(1)	(2)	(3)	(4)
Public Treatment	0.115	0.135	0.054	0.075
	(0.109)	(0.088)	(0.140)	(0.108)
Info Treatment	0.225	0.138	0.154	0.069
	(0.148)	(0.087)	(0.152)	(0.094)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	0.009	0.014	0.041	0.059
Controls	No	No	Yes	Yes
Observations	171	171	157	157

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. Control treatment is the reference category. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.VIII: Excluding Participants with Low Self-Monitoring Score

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Public Treatment	0.017	0.055	–0.011	0.020
	(0.137)	(0.090)	(0.167)	(0.099)
Info Treatment	0.070	0.042	0.065	0.018
	(0.146)	(0.079)	(0.144)	(0.079)
Mean Control Group	0.220	0.172	0.220	0.172
Adj. R-Squared	-0.008	-0.007	0.023	0.031
Controls	No	No	Yes	Yes
Observations	205	205	187	187

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. Control treatment is the reference category. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.IX: Decisions by Sex

	Loan Amount (1)	Loan Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Public Treatment	–0.009	0.034	0.097*	0.071
	(0.163)	(0.138)	(0.053)	(0.047)
Info Treatment	–0.009	–0.057	–0.002	0.010
	(0.161)	(0.124)	(0.048)	(0.051)
Male	0.066	–0.034	–0.060	–0.045
	(0.107)	(0.083)	(0.040)	(0.046)
Male X Public	–0.035	–0.055	0.039	0.010
	(0.186)	(0.141)	(0.110)	(0.079)
Mean Control Group	0.220	0.172	0.038	0.043
Adj. R-Squared	0.027	0.030	-0.006	-0.009
Observations	247	247	247	247

Results from OLS regressions. *Loan Amount* is the credit amount (in Euro) taken to buy a more expensive pen than one could afford, *Loan Dummy* equals 1 if the participant takes out a credit to buy a more expensive pen and zero otherwise. *Lost Amount* is the amount (in Euro) by which the pen a participant buys is cheaper than the one she could afford, *Lost Dummy* equals 1 if the participant buys a cheaper pen and zero otherwise. *Male* is a dummy that equals 1 if the participant is male and zero otherwise. Control treatment is the reference category. Controlled for variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II.X: Number of Modes for Pens Bought

	Number of Modes	Number of Modes
Public Treatment	–0.207	–0.191
	(0.561)	(0.611)
Info Treatment	–0.415	–0.641
	(0.437)	(0.442)
Mean Control Group	1.903	1.903
Adj. R-Squared	0.022	0.073
Controls	No	Yes
Observations	270	248

Results from OLS regressions. *Number of Modes* measures how many different kind of pens are bought most frequently in each session. Controls are variables with significant differences between control and treatment groups: education, being a student, risk preference, know someone, persons in sessions, extraversion, agreeableness, self-esteem, and locus of control. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

III Experimental Material

Material III.I: Instructions

Instructions

The experiment in which you are going to participate serves to analyze decision behavior.

For your presence, you will receive an amount of 5 Euro, independent of your decisions and of other events in the experiment. The participation fee is 3.50 Euro. In addition, you can earn money in the experiment that depends on your decisions and on the decisions of the other participants. For that reason, it is very important that you read these instructions thoroughly.

During the experiment it is not permitted to use electronic devices or to communicate with the other participants as long as you are not requested to do so. Please only use the programs and functions provided for this experiment. Please do not talk to the other participants. If you have a question, please raise your hand. We will come to answer your question in private. Please do not ask your question out loud in any circumstance. In case the question is relevant for all participants, we will repeat it and answer it for everyone. If you violate the rules, you will be excluded from the experiment and the payment.

At the beginning of the experiment, you will find short comprehension questions on the screen. Please answer these. If you answer one or more of these questions incorrectly, one of the experimenters will come to discuss open questions with you if necessary.

Structure of the experiment:

1. First option to obtain income - IQ test questions
2. Information about your performance in the first revenue round
3. Shopping round
4. Second option to obtain income – Slider
5. Questionnaire

What happens during the first revenue round?

You have to answer 12 questions during the first revenue round. These are questions that are also used to measure intelligence. The income in this round depends on your performance in relation to the other participants. The three participants with the best results get 3 Euro, the second three get 2 Euro, the third three get 1 Euro and the last three get 0.50 Euro. This means, you are in a direct comparison with the other participants. In case of a tie, the speed with which the questions were answered decides over the ranking.

You will learn how you performed in comparison to the other participants directly after the IQ test questions. You alone will see your personal rank.

What happens during the shopping round?

After the IQ test questions, you will have the possibility to buy a pen. You can decide between five different pens. All pens are of different quality and have different prices. If your earned income is not sufficient, you will have the opportunity to take out a loan to buy a pen of better

quality. All pens are clearly labelled and the quality of the pens is obvious. Income not spend will expire. The taken credit will be subtracted from your participation fee of 3.50 Euro. You will receive the pen at the end of the experiment together with your payment.

You will later see on the screen how you inform the experimenters about your decision.

What happens during the second revenue round?

In this round, you can earn additional income. Your income will depend solely on your own performance. You have to move sliders to a certain point. You will be paid for each slider that is moved to the right point. The income you will earn per slider will decrease with the amount of sliders you already set correctly: for the first set of eight correctly set sliders you earn more than for the second set of eight correctly set sliders, for the second set of eight correctly set sliders you earn more than for the third set of eight correctly set sliders etc.. You can keep the whole income you earned during this round.

This round follows a questionnaire. At the end of the experiment, you will receive your payment and the pen you bought in the next room one after another. Please wait outside the room until we call your name as only one person at the same time should be inside the room to receive the payment.

Schematic:

Total remuneration = Show-Up fee 5 Euro

+ Participation fee 3.50 Euro

+ Variable income 1 (IQ test: must be spent to purchase a pen or expires)

+ Variable income 2 (Slider task: money can be kept)

Material III.II: Comprehension Questions

Comprehension questions:

1. On what does your **income depend** in the **first revenue round**?
    a. Only on my own performance
    b. On my own performance in relation to other participants
    c. Only on the performance of the others

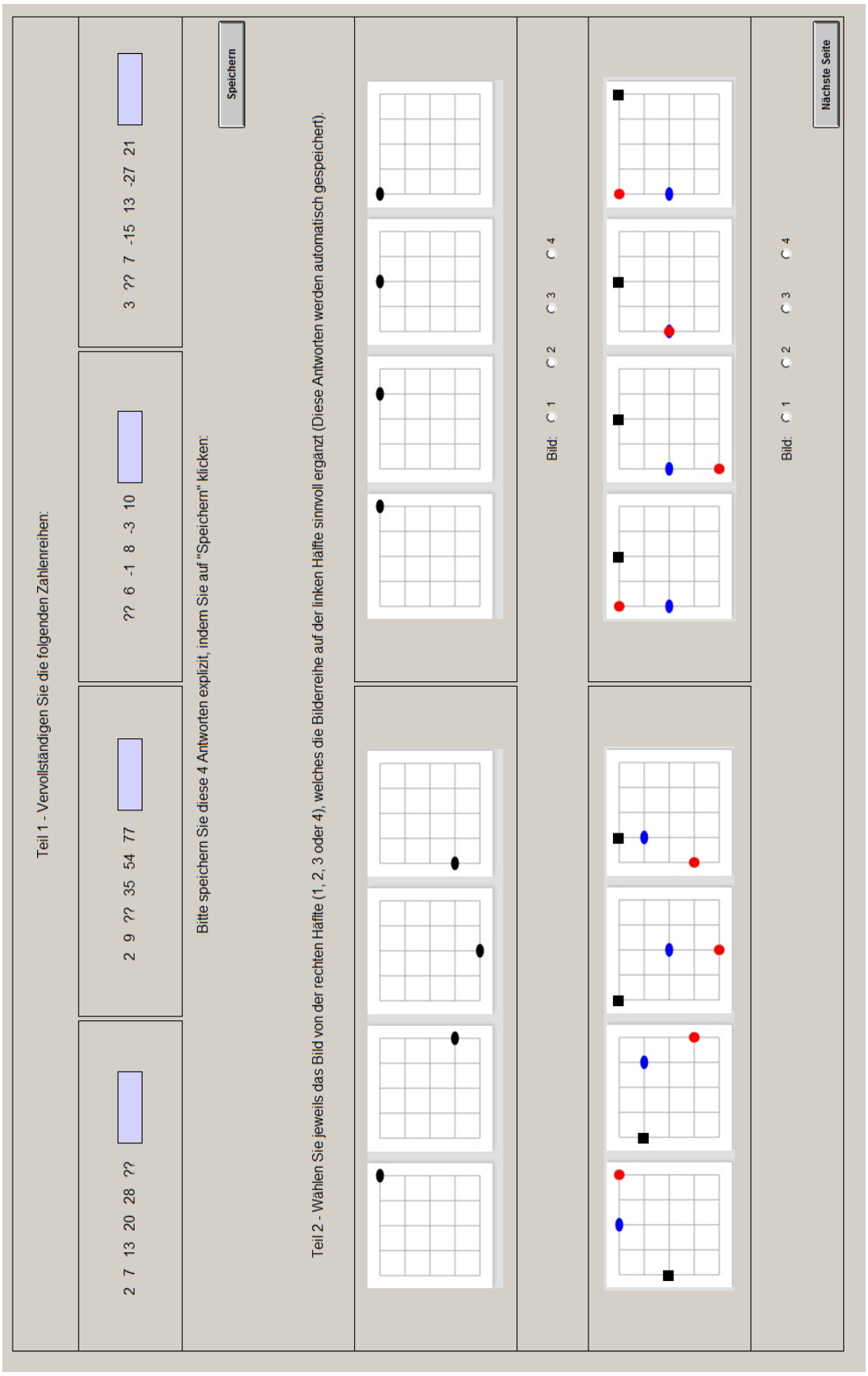
2. What happens to the **income** of the first round that **you do not spend**?
    a. I can keep it
    b. It expires
    c. The other participants get it

3. On what does your **income depend** in the **second revenue round**?
    a. Only on my own performance
    b. On my own performance in relation to other participants
    c. Only on the performance of the others

4. What are the options in case you want to **buy a better pen** than your **income pay for?**
    a. Take out a loan
    b. Nothing
    c. Take money from other participants

5. What happens if you **cannot pay back the credit** with the earned money?
    a. I can give back the pen
    b. I have to pay the money to the experimenters
    c. The money will be deducted from my participation revenue

Material III.III: IQ-Quiz



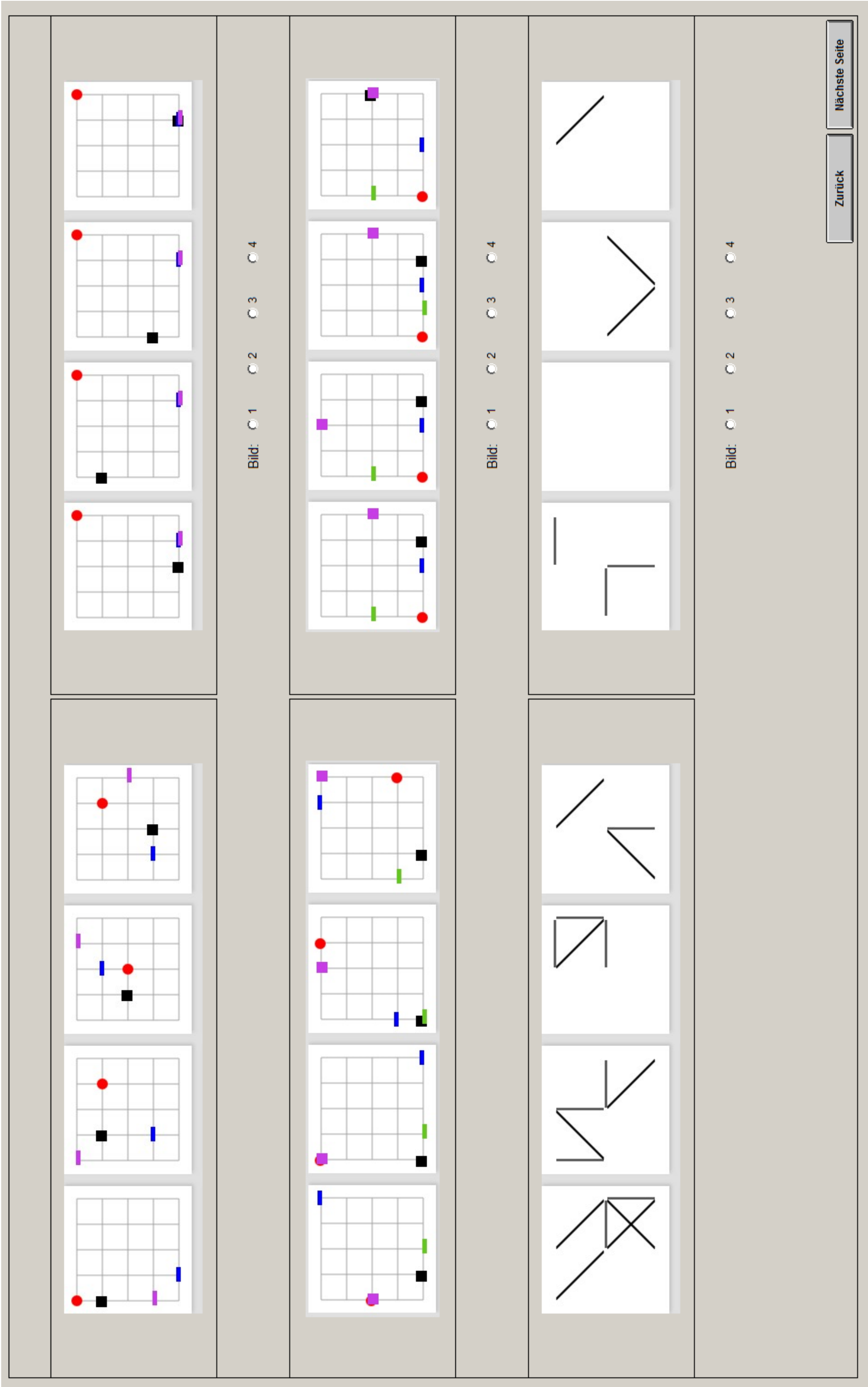


Bild: 1 2 3 4
Bild: 1 2 3 4
Bild: 1 2 3 4
Zurück
Nächste Seite

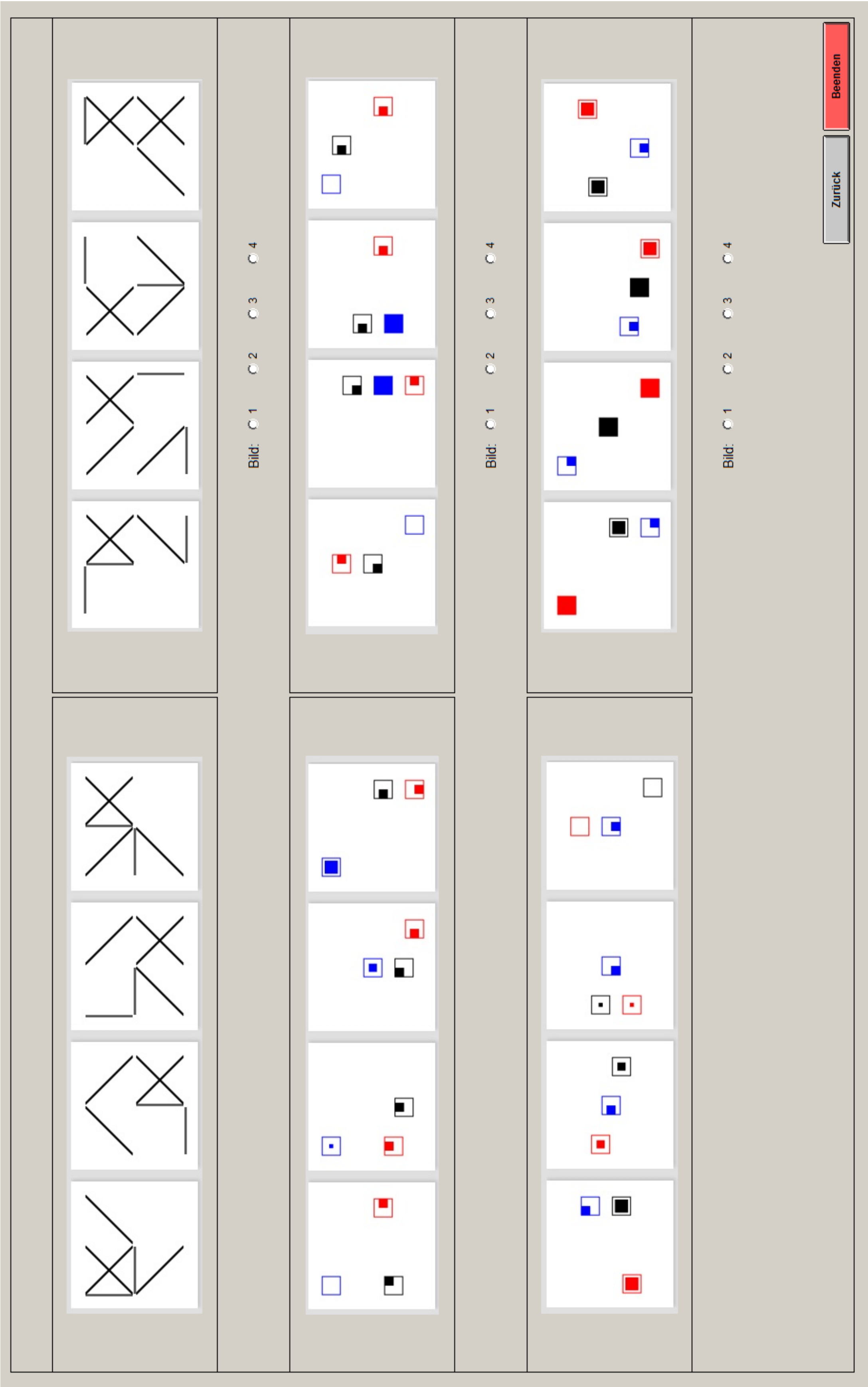
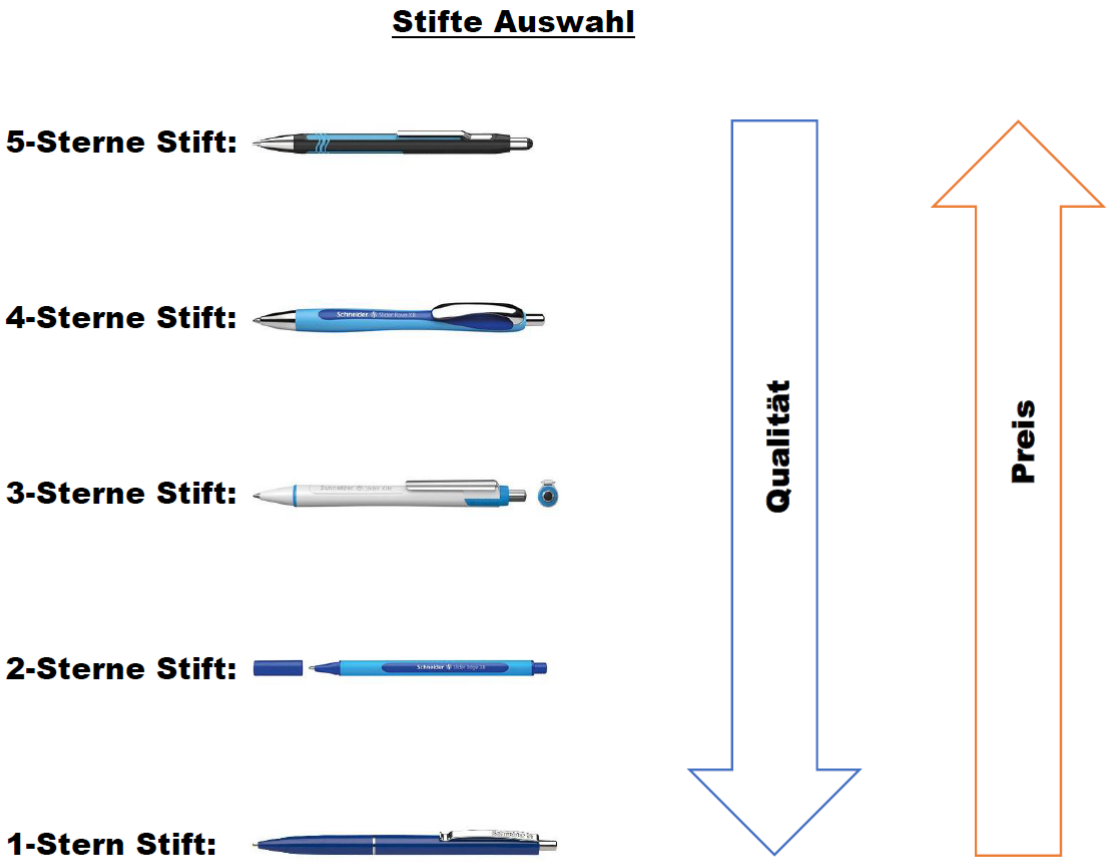
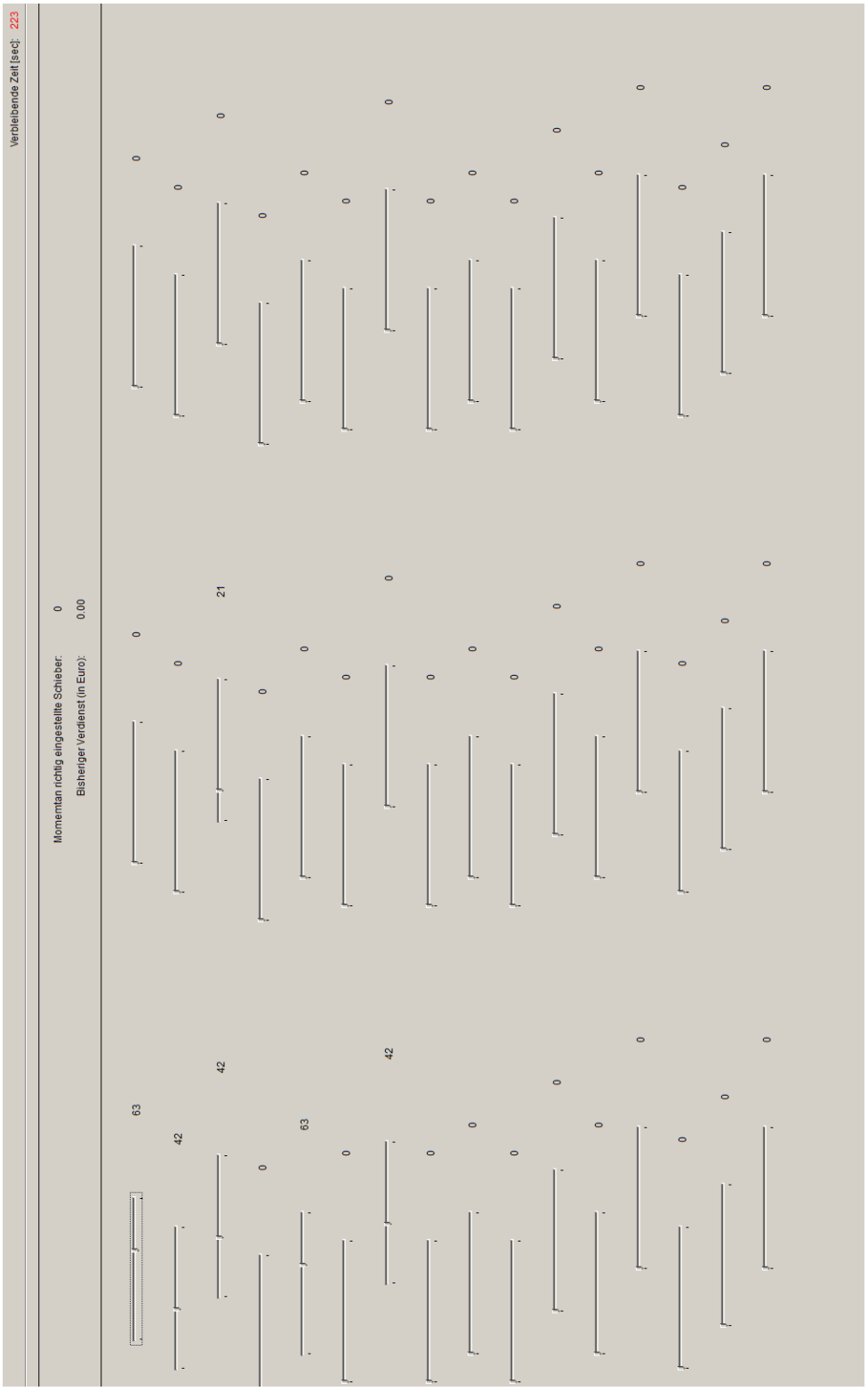


Bild: 1 2 3 4
Bild: 1 2 3 4
Bild: 1 2 3 4
Zurück
Beenden

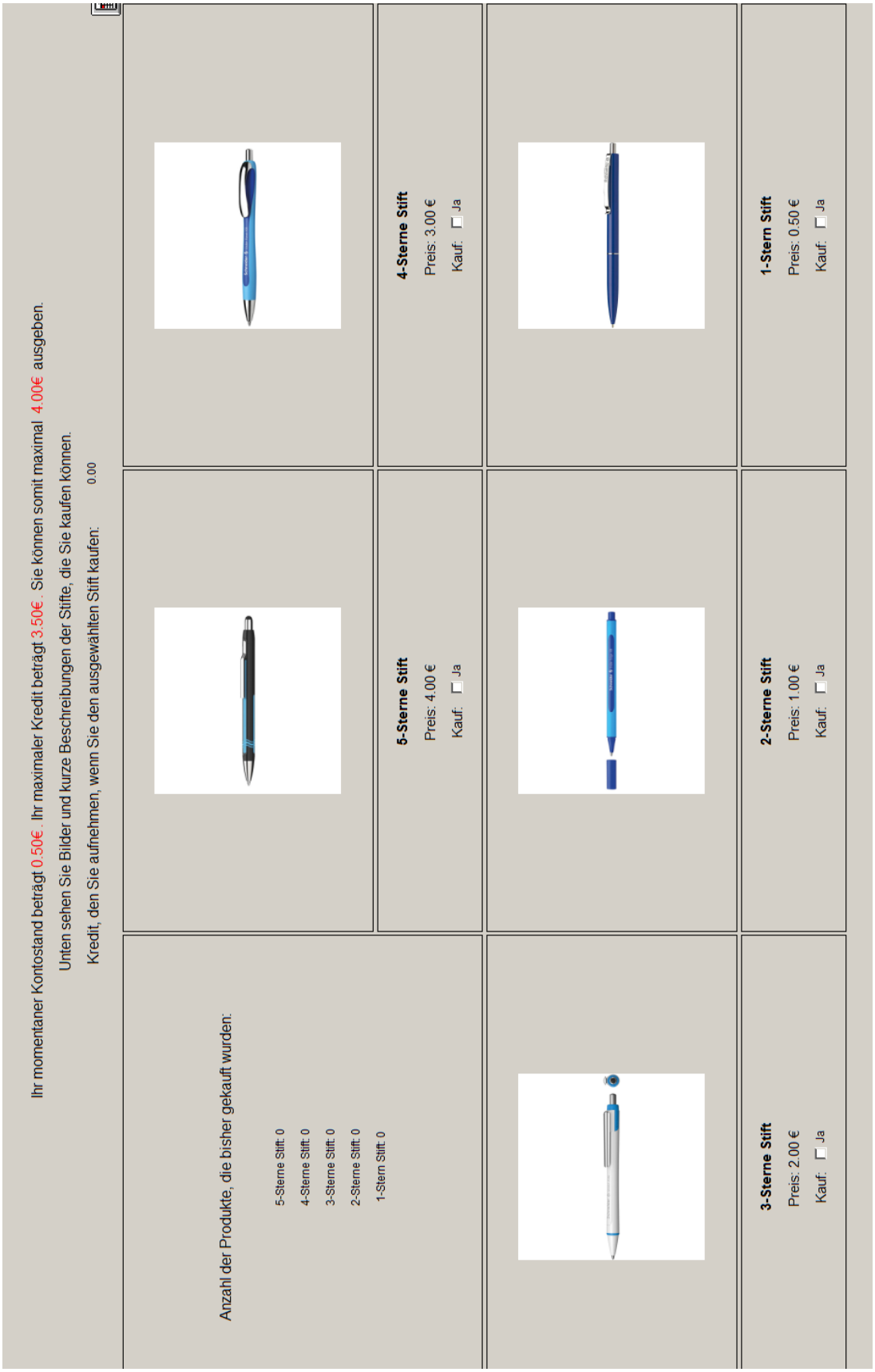
Material III.IV: Printed Paper with Pens



Material III.V: Slider Task



Material III.VI: Shopping Information Treatment



Material III.VII: Example Products Online Survey

Produktauswahl

Als nächstes bitten wir Sie aus jeder Produktkategorie das Produkt auszuwählen, das Sie am ehesten in Ihrem alltäglichen Leben kaufen und verwenden.

Schokolade *

Alpia Alpenmilch 100g: Preis ca. 0,80€

Milka Alpenmilch 100g: Preis ca. 1,00€

Ritter Sport Alpenmilch 100g: Preis ca. 1,10€

Alnatura Bio Vollmich 100g: Preis ca. 1,30€

Lindor Milch 100g: Preis ca. 2,00€

Kugelschreiber *

Schneider K15: Preis ca. 0,50€

Schneider Slider Edge XB 1522: Preis ca. 1,00€

Schneider Slider Xite 1332: Preis ca. 2,00€

Schneider Slider Rave 1325: Preis ca. 3,00€

Schneider Epsilon 1386: Preis ca. 4,00€