
The Effect of Social Comparison on Debt Taking: Experimental Evidence

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Discussion Paper No. 227

December 19, 2019

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17th December 2019

Abstract

A number of studies show that there is a link between social comparison and high levels of household debt. However, the exact mechanisms behind this link are not yet well understood. In this paper, we perform a lab experiment designed to study the effects of social image concerns and peer information on consumption choices that can be financed through debt taking. We find that having to announce one's consumption decision publicly leads to leaving money on the table, which is the opposite of what we expected. Being informed about other participants' choices leads to conformity in choices between participants.

Keywords: Household Finance; Lab experiment; Social Comparison; Peer Effects

JEL: D14; D12; D91

*We thank Anik Ashraf, Dirk Engelmann, Jana Friedrichsen, Lea Heursen, Raji Jayaraman, Lukas Menkhoff, Renke Schmacker, Helke Seitz, Roel van Veldhuizen, Georg Weizsäcker, and seminar participants in Berlin for helpful comments. The paper also profited from discussions with conference participants at IMEBESS 2019, Utrecht. Financial support from the Collaborative Research Center TRR 190 is gratefully acknowledged.

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1 Introduction

The number of over-indebted households is increasing worldwide (IMF, 2017). In Germany, for example, 6.9 million households were regarded as over-indebted (Creditreform Wirtschaftsforschung, 2017) and 7.5% of German households had negative assets in 2017 (Deutsche Bundesbank, 2019). These high levels of debt cannot be rationalized by conventional economic theory and, in this regard, households' borrowing behavior is vastly understudied. There are many open questions about the determinants of debt taking (Zinman, 2015). Despite this household debt is a wide ranging problem, as over-indebtedness can pose a serious risk to household well-being and the economy as a whole (IMF, 2017).

One promising, but still very small, line of research studies the effects of social comparison and peer effects on debt levels. Georgarakos et al. (2014) find that individuals who believe their social circle to have higher income than themselves are more likely to hold debt. Agarwal et al. (2018) show, using lottery winners in Canada as exogenous variation, that unequal incomes in neighborhoods can lead to financial distress. These studies find evidence for the effect of social comparison on debt. However, what remains unclear are the mechanisms behind these effects. In this paper, we aim to shed light onto these mechanisms by performing a lab experiment in which we are able to disentangle two different channels.

We use three different treatments to disentangle the following two possible drivers: *social image concerns*, concerns about private information about oneself that is revealed to others, and *peer information*, the information about others that is revealed to oneself. Bursztyn and Jensen (2017) clearly distinguish between these two kinds of peer effects. By separating these two mechanisms, we also contribute to literature on peer effects in general.

The three treatments vary the way participants make and communicate a consumption decision: participants are supposed to 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 perform in comparison to the others in the session. All the money not spent is lost. Thus, assuming cognitive ability to be a socially desirable trait, the consumption decision can be indicative of being a "lower" or a "higher" type. 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 is shown to the participants on screen before they make their own decision. This way, participants are informed about the decision of others without identities being revealed.

In all treatments, participants have the possibility to take out a loan to buy a higher quality pen. 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 by conducting an online survey and compare it to the decision 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 social comparison.

We have two main expectations regarding the outcome of our experiment. 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. Second, take-up is also expected to be higher in the information treatment than in the control, because of a preference for conformity or because new information is received by learning about other participants choices. However, we expect this effect to be smaller than the social image effect in the public treatment, because it could potentially also move in the opposite direction. Related to these hypotheses, we generally expect persons who take a loan to work harder in the slider task.

Our results are very surprising. Participants in the public treatment do not take more loans than those in the control treatment, but are more likely to buy a pen of lower quality than they could actually afford. Two likely explanations for this finding are that (i) they 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. Thus, they still exhibit social image concerns but of a different kind than we hypothesized. We expected social image concerns to coincide with social status concerns in our experiment but clearly have to refute this expectation. In the information treatment, we find strong evidence for a taste for conformity. Lower performing participants do take out slightly more loans while higher performing participants buy a lower quality pen than participants in the control group. This means that participants in the information treatment converge to some average quality pen although adjustment from above is much larger than from below.

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 more 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. Surprisingly, 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.

Beyond helping to broaden out knowledge about why private debt might have increased sharply worldwide, our study is linked to at least two other strands of the literature. First, to the literature on peer effects on consumption decisions in general and through which channels these effects actually might be induced. Second, on the discussion started by [Veblen \(1899\)](#) over 100 years ago on who is actually engaging in conspicuous consumption and is even willing to take out a loan to finance it.

Several studies find that social comparison at least influences consumption decisions. 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](#)). Rural villagers make consumption decisions that are more in line with the decisions of those that they observe ([Grohmann and Sakha, 2019](#)). In the lab, 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](#)). Methodologically, the experiment by [Clingsmith 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 out a debt to consume and we have an additional treatment to test for peer information effects.

As previously noted, concerning debt taking and peer effects, we find only a few studies looking explicitly at this relationship. [Bertrand and Morse \(2016\)](#), describe, in their appendix, how the presence of higher top-income households is probably causally related to higher credit and bankruptcy in low-income households. As mentioned before, [Georgarakos et al. \(2014\)](#) find a link between beliefs on the affluence of the own social circle and debt taking. [Agarwal et al. \(2018\)](#) show that having a lottery winner in the neighborhood leads to increased rates of bankruptcy for others in the area. While the former studies depict correlations, the latter does not discuss through which personal channels peer effects work and who is more responsive to social comparison.

Many of the aforementioned field studies implicitly explain higher “visible” consumption merely as a result of conspicuous consumption. In reality it is hard to disentangle whether persons want to convey status with their visible consumption or they just “learned” from the visible consumption of others and want to conform. There is a substantial literature on how social -peer- information affects pro-social behavior and charitable giving (e.g [Frey and Meier, 2004](#); [Shang and Croson, 2009](#); [Smith et al., 2015](#)) but less so on (debt-financed) consumption.

We see two major contributions of this lab experiment. First, we are able to study

the mechanisms behind an effect that is found in a number of studies based on field experiments. Second, we test if effects, such as the social comparison effect that holds in studies on consumption, also hold once consumption can be financed through debt.

The paper proceeds as follows: Section 2 explains the experimental design and the data we collect. In Section 3, the results for each treatment are reported. 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.

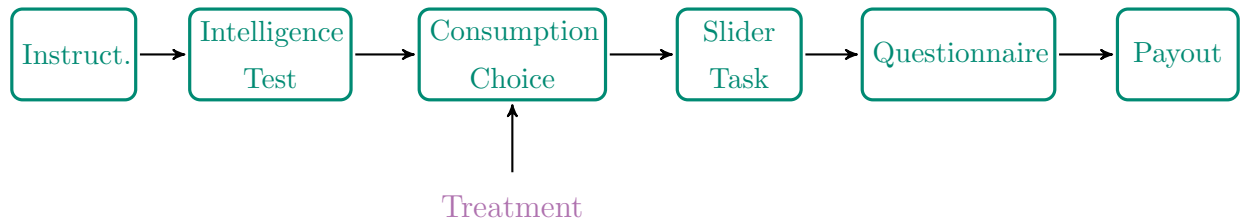


Figure 1: Experimental Flow

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 because we believe it is intrinsically desirable for our student sample to perform well on it. 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€. 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. For more details, see section [2.3](#).

One of the reasons why we choose pens is that we assume that preferences are rather uni-dimensional in the sense that the price is the most decisive factor in the preference relation for the five pens, at baseline. This might be not the case for products like chocolate (see the Descriptives in Section [3.3](#)). 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 for that it is cheaper. However, this means even in a standard economic framework without peer effects, incentives to buy a lower quality pen than one 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. Thus, pens are less likely to confound our treatment than other products, as the

preference is more easily malleable.

2.3 Treatments

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:¹

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

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 5 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:

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

With these assumptions in mind, 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 intervention. 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). 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. The order in which participants stand up is random. They are informed beforehand that announcing the consumption decision is part of the procedure and are shown by the experimenter how they have to do it. Given

¹ Our notation is largely taken from [Bursztyn and Jensen \(2017\)](#).

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. Thus, we assume that low performers 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 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 (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 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. We expect to find that participants in this treatment will follow the decision of those who have already made their decision.

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.

The average number of sliders set correctly is 23.83 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 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.²

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 complement 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 are 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](#)

² 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.

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; 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 own 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 Technical University 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.³ 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-0003597.⁴

In Table 1, we present the descriptive statistics for the whole sample and the control group as well as the differences between 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 3 semesters, a monthly income of ca. 690€, and already participated in at least one other experiment in the lab. There is also no difference in intelligence as proxied by absolute performance in the IQ-quiz and cognitive reflection between treatments. At a first glance, the number of imbalances seem to be particularly high in our experiment in comparison to other studies. However, given the sample size and the large number

³ 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.

⁴ See <https://www.socialscisearch.org/trials/3597>.

of variables we are looking at, this is actually 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 most of our specifications.

Table 1: Descriptive Statistics across Treatments

	Full Sample	Control	Control-Public	Control-Info
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
Openess	-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

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

Participants in the public treatment have a higher level of education but are slightly less likely to study than participants in the control treatment. They are less risk-seeking and less extraverted 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 less participants per session in the public and information treatment (which is, however, exogenous to the participants), participants in these treatment are more likely to know another person in their session. Since this study analyses 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

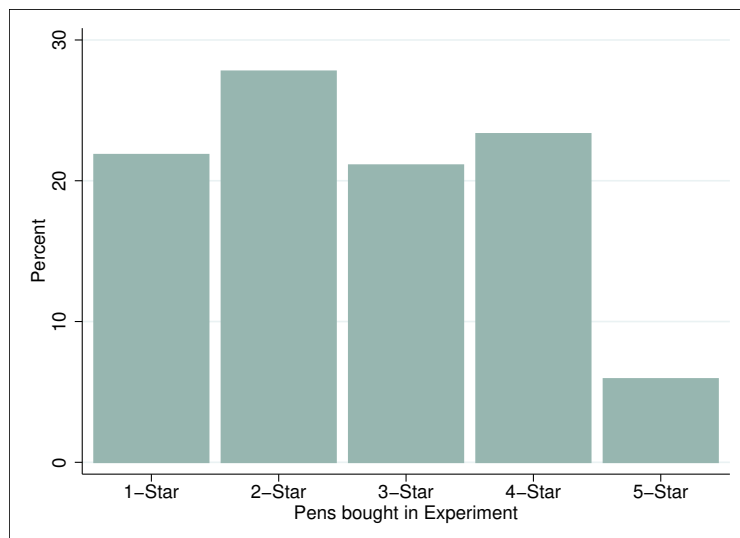
Overall, around 20% of the participants actually take a loan and the average loan amount conditional on take-up is about 1.30€. This means that, on average, participants take up a loan to buy a pen that is one quality level higher than the one they can afford with quiz earnings. However, as can be seen in Table 2, these numbers differ across treatments.

Table 2: Summary Statistics Outcome Variables

	Observations	Mean	Stand. Dev.	Minimum	Maximum
<i>Control Treatment</i>					
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
<i>Public Treatment</i>					
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
<i>Info Treatment</i>					
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

Summary statistics are given in Euro for *Loan Amount* and *Cond. Loan Amount*.

Figure 2: Distribution of Pens Bought in the Experiment



In Figure 2, we see that participants borrow, with a considerable number buying the five-star pen, for which there is no corresponding earnings level. The two-star pen is the most frequently bought pen, whilst the three-star pen is the least popular. Most importantly, we can see from the graphic that people buy pens that do not correspond to their earnings level, as not all bars are of the same height.

3.2 Loan Take-Up

In Table 3, the effects of the socially contingent treatments on loan take-up are estimated.⁵ For both treatments, there are no significant effects on whether participants took a loan nor on the amount, 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 absolutely no effect on the loan dummy when we control for imbalances.

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 $\alpha=5\%$). 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 positive direction. Hence, we are relatively confident that participants, in general, do not take a larger loan due to social image concerns.⁶

⁵ In all our regressions, we estimate standard errors that are bootstrapped and clustered at session level. However, given the subsequent small number of clusters, we also calculate p-values using wild cluster bootstrap following the advice of Cameron et al. (2008). All our results of interest are robust to this specification.

⁶ 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.

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 (0.118)	0.041 (0.088)	-0.017 (0.137)	0.006 (0.095)
Info Treatment	0.086 (0.126)	0.033 (0.078)	0.073 (0.114)	0.003 (0.073)
Mean Control Group	0.220	0.172	0.220	0.172
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

Effect sizes for the loan amount are larger in the information treatment, but standard errors are considerably high. However, observations in the information treatment might be path-dependent within each session and, in this Table, we do not account for this. Table 4, tries to take this into account. In Columns (1) and (2), we control for the place in the order in which participants decide in the information treatment. Those who have to decide later are more likely to adjust their behavior as they receive more information.⁷ We find a significant order effect on the probability to take a loan at all, however, no significant effect on the amount taken. The effect size is small, which might be the case because early deciders can either set a high or a low benchmark. If early deciders choose low quality pens, there might be no reason for followers to take a loan. Therefore, in Columns (3) and (4), we control for the average of pens bought up to the point when the respective participant has to decide. Here, we find an insignificant, albeit positive, effect on loan take up and a negative effect on the likelihood. Eventually, it seems that there is some adjustment in the information treatment. Thus, 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.⁸

⁷ Since in the other two treatments there is no order that matters for the decision, we use the subject number to order these observations in the various specifications.

⁸ 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.

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 (0.210)	-0.173 (0.118)	-0.016 (0.466)	0.070 (0.368)
Order	-0.017 (0.014)	-0.010* (0.006)		
Interaction Order*Info	0.028 (0.025)	0.028** (0.014)		
Mean Prev. Pens			0.023 (0.114)	0.051 (0.116)
Interaction Mean*Info			0.034 (0.169)	-0.015 (0.138)
Mean Control Group	0.220	0.172	0.220	0.172
Correction	Order	Order	Mean Pen	Mean Pen
Observations	248	248	226	226

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

Summarized, when looking at between-subject effects, results are rather surprising. The public treatment has no effect on loan take up, if anything it seems that people are borrowing less in the public treatment. The information treatment seems to have larger effects. In the next subsection, we examine within-subject results to gain further insights. Results on how different personal characteristics interact with peer effects are in Online Appendix [I.II](#).

3.3 Deviation from Pre-Experiment Choice

In this subsection, we compare pen choices in the pre-experiment survey to pen choices during the experiment. Hence, we can examine whether our treatments let participants choose to buy a different pen from the one they claimed to use in everyday life. Therefore, we compare the pen that participants actually buy in the experiment to the pen they buy and use most in everyday life as stated in the online survey. 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 of the experimental design in general. In this sense, we are 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. The latter 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, 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.

Unfortunately, despite having more survey responses than participants, not all our participants answered the online survey or 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 we do not (see Appendix Table II.I).

In Table 5, we regress the different pen choices on treatments. As expected, there are no significant effects on pre-experiment choices (Column (1)). However, there are also no significant effects on choices in the experiment (Column (2)). Interestingly, there is a change in signs, which means that there is a considerable difference between the two coefficients. This difference is marginally significant in the public treatment but only if we do not control for imbalances. Nevertheless, it seems that participants in the public treatment not only take a smaller loan but choose a cheaper pen in general.

Table 5: Pre-Experiment Choice and Adjustment

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

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

Similar to Table 4, Table 6 shows the results for the info treatment, but controlling for order effects. Here, we find highly significant effects. Participants in the information treatment who decide later in the order buy a more expensive pen and, thereby, a pen

that is further away from their pre-experimental choice.

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

	Before (1)	After (2)	Difference (3)
Info Treatment	-0.101 (0.473)	-0.550* (0.326)	-0.567 (0.520)
Order	0.035 (0.045)	-0.064*** (0.023)	-0.102*** (0.036)
Interaction Order*Info	-0.022 (0.055)	0.092*** (0.035)	0.134** (0.054)
Mean Control Group	2.00	2.69	0.71
Correction	Order	Order	Order
Observations	201	248	201

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. 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 and, hence, more likely to buy a more expensive pen, if they have to make their choice later in the order. At the same time, we have seen that, in the public treatment, there is neither a significant effect on loan take up nor on the choice of pens compared to the choice in the pre-experiment survey. The surprising non-results in the latter treatment seem to not only be driven by small effect sizes in combination with a small sample, but effects seem to be non-existent or actually go in the opposite direction.

We here examine this further by looking at 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 determine if people leave money on the table and how much they leave. Results are striking, as participants in the public treatment buy significantly more lower quality pens and are more likely to do this than those in the control treatment. As expected, this effect is driven by high performers, which means we have an asymmetry: high performers 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*** (0.043)	0.076** (0.030)	-0.066 (0.073)	-0.033 (0.044)
Info Treatment	0.068 (0.056)	0.067 (0.053)	-0.018 (0.044)	0.005 (0.051)
Performance			0.002 (0.003)	0.002 (0.003)
Interaction Rank*Public			0.026* (0.015)	0.016** (0.007)
Interaction Rank*Info			0.013 (0.009)	0.009 (0.008)
Mean Control Group	0.038	0.043	0.038	0.043
Observations	248	248	248	248

Control treatment is reference category. Controlled for variables with significant differences. 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, in line with previous results, this effect is counteracted if participants make their choices later in the order. Thus, in contrast to the public treatment, there seems to be a rather symmetric adjustment in the information treatment.

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** (0.078)	0.183** (0.085)	0.203** (0.091)	0.233** (0.107)
Order	0.012* (0.007)	0.012* (0.006)	0.014* (0.008)	0.013* (0.007)
Interaction Order*Info	-0.020** (0.010)	-0.020* (0.010)	-0.021* (0.011)	-0.026** (0.012)
Mean Control Group	0.038	0.043	0.038	0.043
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

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. 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 effort 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. One can only speculate about the reasons behind this. It is 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. All the results combined indicate that having taken a loan in the consumption round may have demotivating effects later in the experiment.

Table 9: Effort and Loan Take-Up

	Effort Slider Task (1)	Effort Slider Task (2)	Effort Slider Task (3)
Public Treatment	1.064 (1.386)	0.952 (1.416)	1.931 (1.551)
Info Treatment	2.131 (1.547)	2.053 (1.555)	2.145 (1.647)
Loan Amount	-3.397*** (0.862)	-3.254*** (0.866)	-2.023* (1.172)
Performance		0.348* (0.190)	0.359* (0.191)
Interaction Loan*Public			-4.023** (1.574)
Interaction Loan*Info			-0.748 (2.091)
Mean Control Group	22.61	22.61	22.61
Observations	248	248	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

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.

4 Robustness

Controlling for the Pre-Experiment Choice There are slight, albeit not significant, imbalances across treatments in the pen participants have chosen 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. Interestingly, 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 fun 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 the likelihood to buy the preferred pen, which is, however, not significant. If we control for order effects, the treatments seem to increase the likelihood to buy the preferred pen for first movers, but this effects 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 dependency 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 provide, 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.

5 Discussion

We find some results in this paper that we did not hypothesize. Our two main findings regarding the debt taking and consumption choices are, first that participants buy worse quality pens than they can afford. This effect is weaker for participants in the info treatment who make their choice later in the order of participants. Secondly, and most strikingly, participants do not want to signal intelligence to other participants. Here, we discuss four potential reasons for the observed findings. These are “standing-out-aversion,” “smarty-pants-effect,” “blame aversion,” and conformity.

“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 Appendix Table II.VI). Jones and Linardi (2014) find females are especially likely to be wallflowers and, if anything, our coefficients point in the exact opposite direction.⁹

“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).

“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. Eventually, with both kinds of explanations,

⁹ 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.

smarty-pants-effect and blame aversion, participants in the public treatment might have social image concerns, just not the ones we anticipated.

Conformity Looking at the coefficients for the information treatment and controlling for order effects, we find a significantly high and positive effect of buying a too cheap pen. This slowly goes down with the order of deciding. Additionally, the number of different modes in bought pens is smaller in the information treatment than in the control treatment, albeit not significantly. Standard errors are large but the effect size is relatively large as well (see Appendix Table II.VII). Since we also find some significant, though much smaller, effects for loan take-up, it seems that decisions in the information treatment are mildly converging to some lower midpoint. In contrast to the public treatment, however, participants cannot observe the individual behavior of others in this treatment. Hence, the last two paragraphs presented explanations for the observed behavior in the public treatment, however, not for the information treatment. A preference for conformity seems to explain the observed pattern in the information treatment fairly well. 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](#)). Alternative motives, like self-image concerns and pure information gathering, are unlikely in our setting as participants especially adjust from above and pens are everyday products. Overall, some participants are actually willing to incur cutbacks as either they have to take on debt or end up with a lower quality pen to conform.

6 Conclusion

The number of over-indebted households is increasing worldwide ([IMF, 2017](#)). Hence, it is increasingly important to understand the drivers behind this process. This paper contributes to the emerging literature on household borrowing behavior. It analyzes the effects of social comparison on debt taking, examining two potential channels. Here, we argue that social comparison is one of the reasons leading to increased debt taking, which in turn leads to overindebtedness.

It is our aim to disentangle two channels that underlie social comparison. Therefore, we take our research question to the lab, as it is difficult to do this outside the lab. 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.

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 and that participants thought of the possible loan they could take as creating a temporary debt. The fact that participants who took a loan did not work more means that they actually left the lab with less money than the others.

Our results on how social comparison might affect borrowing are quite surprising. Social image concerns lead to underspending in our setup, hence, the exact opposite of what we expected. Potential reasons are that participants do not want to be perceived as more intelligent, which is contrary to our expectation, or that the more successful do not want to be blamed for the failures of others. We acknowledge that these reasons are peculiar for our setting and might differ in other environments where, for example, status is not only defined by intelligence and no perfect correlation between success of one group and failure of another group exists. We find striking results on peer information. There is convincing evidence for an intrinsic inclination to conform, which leads less to more debt taking by individuals in the lower tail but more to underspending by those in the upper tail of the performance distribution.

Our findings highlight that not only is borrowing underresearched but also intrinsic motivations like conformity and their effects on consumption and borrowing. Conformity leads to “sub-optimal” decisions on both sides of the distribution in our experiment as participants deviate from their intrinsic preference elicited before the experiment took place. In real life, conformity might disadvantageously hurt the 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. Furthermore, that the upper end of the distribution is adjusting more in our setting 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 peer information effects on debt-financed consumption, similar to what is done in the domain of pro-social behavior.

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

Contents:

I: Susceptibility to Social Comparison

II: Additional Results

III: Experimental Material

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 subject 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

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.

in field studies on conspicuous consumption, e.g. cars, travel destinations, restaurant 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
<i>Cognitive Reflection</i>		
CR	0.100*** (0.031)	0.094*** (0.028)
Interaction CR*Public	-0.128 (0.084)	-0.116* (0.065)
Public Treatment	0.249 (0.210)	0.244 (0.153)
<i>Locus of Control</i>		
LOC	0.056 (0.082)	0.054 (0.036)
Interaction LOC*Public	-0.094 (0.104)	-0.101 (0.064)
Public Treatment	-0.012 (0.138)	0.013 (0.093)
<i>Self-Esteem</i>		
GSE	-0.100 (0.080)	-0.049 (0.071)
Interaction GSE*Public	-0.047 (0.131)	-0.027 (0.092)
Public Treatment	0.006 (0.146)	0.023 (0.102)
<i>Self-Monitoring</i>		
SM	0.046 (0.049)	0.023 (0.036)
Interaction SM*Public	0.017 (0.072)	0.020 (0.069)
Public Treatment	-0.016 (0.137)	0.003 (0.096)
<i>Extraversion</i>		
EV	-0.030 (0.055)	-0.001 (0.048)
Interaction EV*Public	0.148 (0.098)	0.093 (0.066)
Public Treatment	-0.010 (0.134)	0.013 (0.098)
Observations	248	248

Control treatment is reference category. Coefficients on info treatment not reported. Controlled for variables with significant differences and performance in IQ-quiz. SE in parentheses, bootstrapped and clustered on session level.

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 again 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. 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 artefact 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
<i>Openness</i>			
OP	0.288** (0.127)	0.018 (0.063)	-0.316** (0.127)
Interaction OP*Info	-0.372* (0.215)	0.168 (0.107)	0.580** (0.263)
Info Treatment	-0.187 (0.245)	-0.010 (0.149)	0.162 (0.258)
<i>Agreeableness</i>			
AG	0.118 (0.158)	0.083 (0.124)	0.009 (0.159)
Interaction AG*Info	-0.162 (0.252)	-0.154 (0.185)	-0.057 (0.327)
Info Treatment	-0.235 (0.257)	0.017 (0.195)	0.255 (0.300)
Observations	201	248	201

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences and performance in IQ-quiz. SE in parentheses, bootstrapped and clustered on session level.

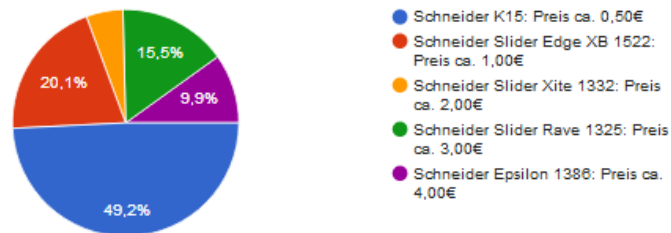
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

II Additional Results

Figure II.I: Pre-Experimental Choices - Pens, Lip-Balms, and Folders

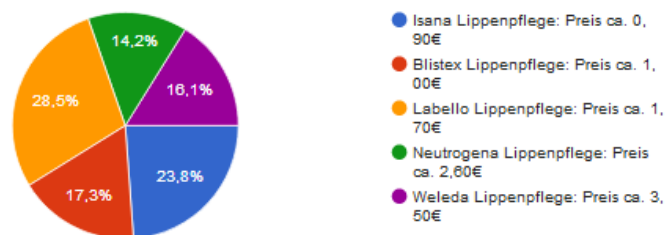
Kugelschreiber

323 Antworten



Lippenpflegestift

323 Antworten



Mappen

323 Antworten

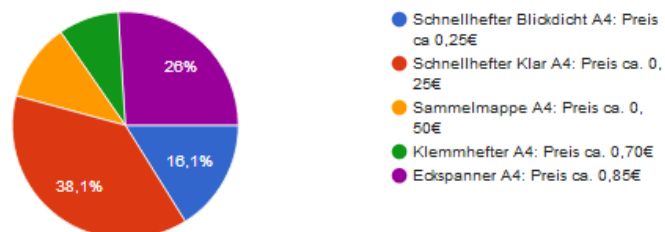


Table II.I: Descriptive Statistics across Survey Participation

	Full Sample	Online Survey	No Survey	Difference
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
Cognitive Reflection	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
Openess	-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
Self-Monitoring	-0.02	-0.04	0.09	0.14
Observations	270	219	51	270

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

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 (0.125)	-0.006 (0.088)	-0.083 (0.146)	-0.021 (0.094)
Info Treatment	0.078 (0.135)	0.030 (0.076)	0.036 (0.133)	-0.017 (0.079)
Pen Before	0.084** (0.036)	0.056*** (0.021)	0.064* (0.038)	0.039 (0.025)
Mean Control Group	0.220	0.172	0.220	0.172
Controls	No	No	Yes	Yes
Observations	219	219	201	201

Control treatment is reference category. Controlled for variables with significant differences. 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 (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Info Treatment	-0.151 (0.249)	-0.165 (0.109)	0.031 (0.586)	0.180 (0.476)
Order	-0.016 (0.022)	-0.002 (0.010)		
Interaction Order*Info	0.030 (0.027)	0.024* (0.013)		
Pen Before	0.067* (0.038)	0.040 (0.026)	0.014 (0.032)	0.019 (0.027)
Mean Prev. Pens			0.086 (0.137)	0.124 (0.141)
Interaction Mean*Info			-0.005 (0.211)	-0.067 (0.175)
Mean Control Group	0.220	0.172	0.220	0.172
Correction	Order	Order	Mean Pen	Mean Pen
Observations	201	201	183	183

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. 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 (0.066)	-0.051 (0.075)	0.198 (0.165)	0.216 (0.195)
Info Treatment	-0.020 (0.080)	-0.027 (0.100)	0.103 (0.170)	0.135 (0.201)
Order			0.012 (0.015)	0.015 (0.014)
Interaction Order*Info			-0.020 (0.023)	-0.025 (0.025)
Mean Control Group	0.312	0.312	0.312	0.312
Controls	No	Yes	No	Yes
Correction	No	No	Order	Order
Observations	219	201	219	201

Control treatment is reference category. Controlled for variables with significant differences. 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	Loan Dummy	Lost Amount	Lost Dummy
	(1)	(2)	(3)	(4)
Info Treatment	0.016 (0.193)	-0.161 (0.113)	0.094 (0.085)	0.138 (0.103)
Order 2	0.004 (0.021)	-0.007 (0.005)	-0.004 (0.004)	-0.002 (0.004)
Interaction Order*Info	0.007 (0.029)	0.025* (0.013)	-0.003 (0.008)	-0.011 (0.010)
Mean Control Group	0.220	0.172	0.038	0.043
Correction	Order 2	Order 2	Order 2	Order 2
Observations	248	248	248	248

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

Table II.VI: Decisions by Sex

	Loan Amount (1)	Loan Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Public Treatment	-0.009 (0.165)	0.034 (0.133)	0.097* (0.052)	0.071 (0.047)
Info Treatment	-0.009 (0.161)	-0.057 (0.121)	-0.002 (0.047)	0.010 (0.051)
Male	0.066 (0.108)	-0.034 (0.084)	-0.060 (0.039)	-0.045 (0.044)
Interaction Sex*Public	-0.035 (0.190)	-0.055 (0.141)	0.039 (0.111)	0.010 (0.078)
Mean Control Group	0.220	0.172	0.038	0.043
Observations	247	247	247	247

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

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

Table II.VII: Number of Modes for Pens Bought

	Number of Modes	Number of Modes
Public Treatment	-0.207 (0.573)	-0.191 (0.580)
Info Treatment	-0.415 (0.431)	-0.641 (0.451)
Mean Control Group	1.903	1.903
Controls	No	Yes
Observations	270	248

Control treatment is reference category. Controlled for variables with significant differences. 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 see 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 can actually 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

Teil 1 - Vervollständigen Sie die folgenden Zahlenreihen:

2 7 13 20 28 ??	2 9 ?? 35 54 77	?? 6 -1 8 -3 10	3 ?? 7 -15 13 -27 21
-----------------	-----------------	-----------------	----------------------

Bitte speichern Sie diese 4 Antworten explizit, indem Sie auf "Speichern" klicken:

Speichern






Teil 2 - Wählen Sie jeweils das Bild von der rechten Hälfte (1, 2, 3 oder 4), welches die Bilderreihe auf der linken Hälfte sinnvoll ergänzt (Diese Antworten werden automatisch gespeichert).

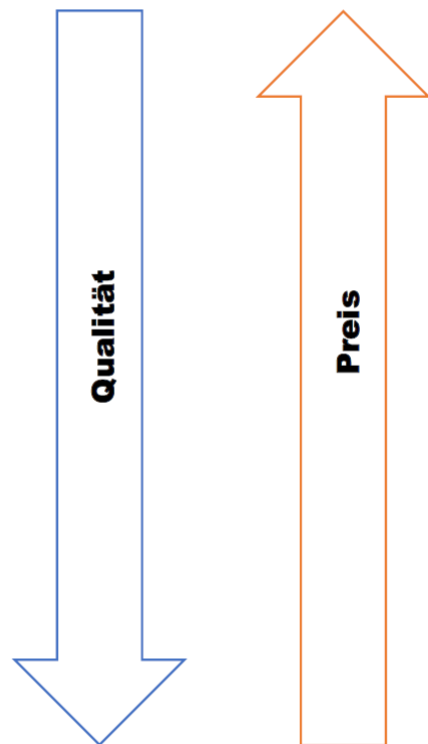
Bild: <input type="radio"/> 1 <input type="radio"/> 2 <input type="radio"/> 3 <input type="radio"/> 4	Bild: <input type="radio"/> 1 <input type="radio"/> 2 <input type="radio"/> 3 <input type="radio"/> 4
Bild: <input type="radio"/> 1 <input type="radio"/> 2 <input type="radio"/> 3 <input type="radio"/> 4	Bild: <input type="radio"/> 1 <input type="radio"/> 2 <input type="radio"/> 3 <input type="radio"/> 4

Nachste Seite

Material III.IV: Printed Paper with Pens

Stifte Auswahl

- 5-Sterne Stift:** 
- 4-Sterne Stift:** 
- 3-Sterne Stift:** 
- 2-Sterne Stift:** 
- 1-Stern Stift:** 



Material III.V: Slider Task

[illegible]

Material III.VI: Shopping Information Treatment

<p>Ihr momentaner Kontostand beträgt 0.50€. Ihr maximaler Kredit beträgt 3.50€. Sie können somit maximal 4.00€ ausgeben.</p> <p>Unten sehen Sie Bilder und kurze Beschreibungen der Stifte, die Sie kaufen können.</p> <p>Kredit, den Sie aufnehmen, wenn Sie den ausgewählten Stift kaufen: 0.00</p>		
<p>Anzahl der Produkte, die bisher gekauft wurden:</p> <p>5-Sterne Stift: 0</p> <p>4-Sterne Stift: 0</p> <p>3-Sterne Stift: 0</p> <p>2-Sterne Stift: 0</p> <p>1-Stern Stift: 0</p>		<p>5-Sterne Stift</p> <p>Preis: 4.00 €</p> <p>Kauf: <input type="checkbox"/> Ja</p>
		<p>4-Sterne Stift</p> <p>Preis: 3.00 €</p> <p>Kauf: <input type="checkbox"/> Ja</p>
		<p>3-Sterne Stift</p> <p>Preis: 2.00 €</p> <p>Kauf: <input type="checkbox"/> Ja</p>
		<p>2-Sterne Stift</p> <p>Preis: 1.00 €</p> <p>Kauf: <input type="checkbox"/> Ja</p>
		<p>1-Stern Stift</p> <p>Preis: 0.50 €</p> <p>Kauf: <input type="checkbox"/> Ja</p>

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 Vollmilch 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€