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Exchange Asymmetries for Bads? Experimental Evidence*

Markus Dertwinkel-Kalt† and Katrin Köhler‡

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Abstract

Whereas exchange asymmetries for goods are well known, we provide a first incentivized test of exchange asymmetries for bads (i.e., items yielding a negative utility). On the one hand, prospect theory predicts an endowment effect for goods and bads, on the other hand, attention-based theories such as salience theory predict an endowment effect for goods, but a reverse endowment effect (i.e., a particular high willingness to switch) for bads. Since both strands of research often make the same predictions concerning biased decision making, the investigation of exchange asymmetries for bads is a key element to distinguish between their validity. In our experiment, we find a strong endowment effect for bads, so that our results speak in favor of prospect theory.

JEL-Classification: D03.

Keywords: Loss Aversion, Salience Theory, Prospect Theory, Endowment Effect.

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1 Introduction

Recently, various types of attention-based theories have emerged, such as salience theory by Bordalo et al. (2012a,b), a theory of "Attention and Reference Dependence" by Bhatia and Golman (2013) or focusing theory by Kőszegi and Szeidl (2013), which tackle the prevalence of loss aversion-based theories (Kahneman and Tversky, 1979 and 1991; Kőszegi and Rabin, 2006) in behavioral economics. As shown by Bordalo et al. (2012a)'s comparison between salience theory and prospect theory, both can account for a wide range of cognitive biases relevant for decision theory. For choice under risk, for example, the Allais paradox, preference reversals and framing effects (see Bordalo et al., 2012a; Kahneman and Tversky, 1979) represent some of those biases. Concerning riskless choice, for instance the "endowment effect" (Thaler, 1980) for goods constitutes one of these biases (see Bordalo et al, 2012b; Kahneman et al., 1991). Therefore, both attention-based and loss aversion-based theories provide an explanation for many biases in decision making.

We analyze a particular effect where both classes of models show contradicting predictions, which allows to precisely distinguish the two approaches. Specifically, we investigate exchange asymmetries for unpleasant items, which yield a negative utility (bads). For valuable items, which yield a positive utility (goods), individuals typically reveal an endowment effect, i.e., subjects tend to stick to their endowments and the share of trades is significantly lower than predicted by reference-independent utility theory. According to prospect theory, this effect emerges as a result of loss aversion, whereas attention-based theories explain this result by a decision maker's overweighting of salient features of the endowment. We implement a riskless choice between two bads after being endowed with one of them. In this setting, prospect theory predicts the usual endowment effect, while attention-based theories predict a reversal of this effect. Since predictions contradict each other, testing for exchange asymmetries of bads allows to precisely distinguish between alternative classes of models.

In a laboratory experiment, we test the explicit prediction by Bordalo et al. (2012b) according to which the endowment effect will be reversed for items which yield a negative utility. In order to test for exchange asymmetries of bads, we first endow each subject randomly with one of two unpleasant tasks, either sorting a specific amount of two-colored confetti (task "sorting") or writing zeros and ones to boxes of one and a half sheets of checkered paper (task "zeros and ones"). These unpleasant tasks represent bads in our experiment. Before the start of the actual task, we allow subjects to switch their task against the other unpleasant one. Similar to classical endowment effect experiments, this approach enables us to test for specific exchange asymmetries as predicted either by loss aversion-based theories (Kahneman and Tversky, 1979 and 1991; Kőszegi and Rabin, 2006) or by attention-based theories (Bordalo et al., 2012a,b; Kőszegi and Szeidl, 2013).[1]

Our results are in line with prospect theory. We can reject salience theory's prediction of a reversal of the endowment effect for bads, and instead find predictions by Kahneman and Tversky (1979 and 1991) and Kőszegi and Rabin (2006) to be confirmed. Unlike salience theory states, people do not switch the bad they were endowed with, but reveal a robust endowment effect similar to that found for goods in Knetsch (1989) and Kahneman et al. (1990 and 1991).

We also add to the literature on differences between hypothetical and incentivized experiments. By transferring our bads to a hypothetical frame, we reproduce findings in the literature of an elimination of the endowment effect for bads (Brenner et al., 2007; Bhatia and Turan, 2012). The strong discrepancy to the incentivized setting can be rationalized as follows. As Bordalo et al. (2012b) propose, focused attention may result in an intuitive disapprobation of the assigned bad, which triggers switching and leads to the elimination of the endowment effect in hypothetical settings. In incentivized settings, however, this impulsive disappreciation of the endowed bad is superposed by the adjustment of the

[1]For specific reasons which will be discussed in Section 6, we refrained from an experiment which tests for a gap in one's willingness to accept and one's willingness to pay for our tasks.

subject's reference point and her loss aversion with respect to this. Thus, a subject may intuitively have the preference to switch her bad, which is expressed in the hypothetical studies. If she, however, has a second thought as in incentivized studies, she does not give in to this impulse since she realizes the alternative's downsides and is loss averse with respect to her endowment's upsides. This makes her refrain from switching.

In the next section, we review the related literature. Section 3 discusses the theoretical approaches to exchange asymmetries for bads, which we focus on. Section 4 introduces our experimental design, before we present the experiment's results in Section 5. We discuss our experiment and its crucial features in Section 6. In Section 7 we debate the discrepancy of the results between the hypothetical and the incentivized settings. Section 8 concludes.

2 Related literature

Existing literature does not reveal a clear indication of exchange asymmetries for items yielding a negative utility. While some studies find an endowment effect for bads, others report on a reversal of this effect for bads. However, due to the difficulty of involving bads in experiments, the literature on this topic is very limited.

Brenner et al. (2007) document the finding of the reverse endowment effect for bads in a hypothetical setting, which, however, is much weaker than the endowment effect observed in classical exchange experiments (Knetsch, 1989). The incorporated bads were additional driving lessons and payment of a certain fine. Each subject was assigned one of these two alternatives before getting the opportunity to switch to the other bad. They find that subjects endowed with a (hypothetical) bad option reveal a tendency to switch to the other bad alternative. Bhatia and Turan (2012) reconsider this hypothetical setting and replicate the reversal of the endowment effect for bad options. Additionally, by altering the subjects' focus towards the non-endowed option, they eliminate the reverse endowment effect. This finding conforms with predictions by the salience mechanism (Bordalo et

al., 2012b). Whereas we reproduce their hypothetical findings of a slight reversal of the endowment effect for bads, we focus on our incentivized study where choices have real consequences. Similar to Brenner et al. (2007) and Bhatia and Turan (2012), we incorporate non-physical items as bads, since physical items usually do not serve as bads as they may be ignored or thrown away without any costs. Therefore, we incentivize our study via assigning each subject a certain (bad) task.

In Dhar and Sherman (1996), participants were initially given the option to choose among two products with some shared characteristics and with unique features. Afterwards, subjects could switch their initially chosen item against a third option. The authors find that the unique attributes receive a higher weight in the decision making process than the shared features of the products. Overall, this study documents a strong tendency to stay with the initial choice.[2] These findings, however, do not represent an endowment effect, but could also result from one's tendency to reveal consistent preferences. The tendency to stay with the initial choice is particularly strong if the options have unique upsides, but have the downsides in common. Whereas the tendency to switch the initial choice is significantly higher if the shared characteristics are good and each product's unique attribute is bad, overall switching rates are still low. Thus, on the one hand, this study supports attention-based theories since it conforms with the prediction that choice patterns are influenced by the salience of attributes. However, on the other hand, low overall switching rates do not support an entire reversal of the endowment effect in the case of bad items. In contrast, in a related incentivized experiment in which the initial choice was replaced by an endowment, Antonides et al. (2010) discover an even stronger endowment effect if the products' unique features are bad. They rationalize their result by assuming that a subject focuses on the unique feature of the alternative, which is bad and thus makes a switch less likely. To sum up, these studies yield support for the impor-

[2]The paper's findings relate to Chen et al. (2006) who study the behavior of capuchin monkeys. Given an initial choice between two options, the monkeys reveal to be loss averse.

tance of salience and focusing in decision making, however, do not provide evidence for the existence of a reverse endowment effect. These studies also imply that incentivization may account for differences in results. In contrast to Dhar and Sherman (1996), our main study is incentivized, whereas their study contained only hypothetical choices. Different from both studies, we do not incorporate alternatives which have only one downside, but yield an overall positive utility; instead, we take alternatives which explicitly represent bads.

Psychological studies, for example Lerner et al. (2004), find that negative emotions, induced in a pre-test situation which was irrelevant to the later economic decision, eliminate or even reverse the endowment effect for goods. These findings are rationalized by carry-over effects of subjects' emotions on subsequent decision making. By incidentally induced bad emotions, subjects may value the endowed good itself as a bad, as if the negative emotion was rooted in the endowment. The desire to change one's (emotional) circumstances may result in the desire to get rid of the assigned object. Different from our setup, the endowed items in these experiments do not represent bads, i.e., the items do not give a negative utility to the owner. Most related to our experiment, Dhar et al. (1999) study the effects of initial comparisons of two alternatives on subsequent preference decisions between them. For this purpose, at the first stage, subjects assessed either how similar or how dissimilar two options are; in each case, one of the two options was focal. At the second stage, subjects had to indicate their preferences for the alternatives. Similar to the salience mechanism (Bordalo et al., 2012b), the authors implement a focus-shift model in which they argue that the initial comparisons carry over to subsequent preference judgments such that relative weights assigned to certain attributes in the preference decision are affected by the initial comparison. Using two relatively unattractive alternatives, they show that a selective focus on the negative attributes of the focal option in the first stage carries over to the second stage, which decreases the preference for the focal option. They explain this result by the negative attributes being more salient in this

setting and thus more likely to be considered.

However, different from our setup, options in their study do not represent bads in a narrow sense, i.e., considered objects do not yield an overall negative utility. Furthermore, in contrast to our experiment, their study was not properly incentivized.

3 Exchange asymmetries for bads: predictions

We compare two strands of behavioral research with respect to their predictions about exchange preferences for bads in typical exchange experiments. Whereas we restrict our analysis to the case where each bad is distinguished by a unique downside, the predictions are robust to settings in which both bads share the same downsides, but where each bads' most severe downside is different. In the following, we argue why attention-based theories predict a reversal of the endowment effect, whereas loss aversion-based theories make the opposite prediction. We model the experimental behavior according to both strands of research and derive the different predictions.

3.1 The reverse endowment effect according to attention-based theories

Attention-based theories and especially Bordalo et al. (2012b) and Bhatia and Golman (2013) explicitly predict a reverse endowment effect for bads. We shortly illustrate the procedure yielding this effect according to Bordalo et al. (2012b); via focusing theory (Kőszegi and Szeidl, 2013), the reverse endowment effect can be explained similarly. Typically, experiments incorporating endowments to test for exchange asymmetries can be modeled by a two-stage procedure as follows. Suppose somebody is endowed with one of two different bads, each of which has a different downside (*the first stage*). Under rational

considerations, both tasks yield the same utility.[3]

Later on, she gets the chance to switch her endowment against the alternative item (*the second stage*). In the following, we illustrate the two-stage procedure which predicts the reverse endowment effect.

According to the salience mechanism (Bordalo et al., 2012b), exchange asymmetries of an item root in the overweighting of salient features. At the moment an endowment is assigned, it is valued in comparison to the status before in which no item was held. Thus, at the first stage, a subject who is endowed with a bad overvalues its salient downside. This leads to an exaggerated bad valuation of the respective item. At the second stage, the subject has the opportunity to exchange the bad item for an alternative one, which is equally bad under rational considerations. At this stage, the endowment and the alternative are valued equally since both have, relative to each other, one downside and one upside. But, according to Bordalo et al. (2012b), the first stage valuation persists partly to the second stage due to the "cold glow of ownership" for bads. Thus, the final valuation of the endowment is a compound of the first stage's exaggerated bad valuation and the second stage's equally bad valuation for both alternatives, so that finally the endowment is valued lower than its alternative. This predicts a particularly high willingness to switch bads (for details, see Appendix A).

According to focusing theory (Kőszegi and Szeidl, 2013), the endowment effect can be explained similarly. An individual focuses more on attributes in which her options differ more, i.e., in which her range of choice is broader, and thus overweights these attributes. Since at the first stage only the item which she is endowed with is available, she compares her assigned alternative against the option of holding nothing. Her options differ especially concerning the attribute the endowed item is particularly bad in, such that she focuses on and overweights this negative attribute of the endowed bad. As according to the salience

[3]For illustrative reasons, we make this restrictive assumption (which is supported by our experiment and our data). In general, it suffices that none of the options is universally preferred by all subjects.

mechanism, this results in a first-stage undervaluation of the endowed bad. At the second stage, the endowed bad is valued rationally due to the equal range of choice in both items' attributes. Considering the final valuation as a compound of the valuations at both stages leads to the prediction of the reverse endowment effect.

Thus, attention-based theories make a precise prediction concerning exchange preferences for bads. As we have argued, according to this strand of research we expect to find a reverse endowment effect in experiments testing for exchange asymmetries of bads. Consequently, we hypothesize that switching rates for bads in our experiment are significantly above 50%.

3.2 The endowment effect according to loss aversion-based theories

This section investigates predictions of loss aversion-based theories regarding exchange preferences for bads, which are applicable to our experimental setup. At this point, we focus on prospect theory itself (Kahneman and Tversky, 1991), whereas we provide the analysis according to prospect theory's variant by Kőszegi and Rabin (2006) in Appendix A.

Existing literature finds that prospect theory does not yield the reverse endowment effect for bads as attention-based theories do. As Bhatia and Golman (2013) state, prospect theory does not distinguish between a reference point in the gain or loss domain of utility, so that the size of the endowment effect is not expected to differ between goods and bads. Antonides et al. (2010) argue that, as negative outcomes receive a higher weight according to prospect theory, the perceived difference between products with unique bad features (like the two unpleasant tasks incorporated in our experiment) is increased compared to products with unique good features (as in the classical mugs-candy bars experiment, see Knetsch, 1989), so that the exchange rates of endowments should be even lower for bads than for goods.

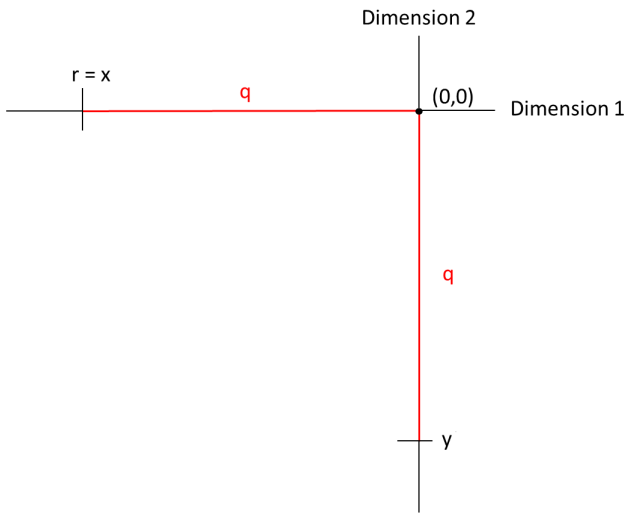
Subsequently, we argue that prospect theory predicts the endowment effect for bads by using the reasoning of Kahnemann and Tversky (1991).

We consider two bads x and y, each of which has a different, unique negative feature. This is illustrated in Figure 1, where the negative features are indicated by item x's negative value in dimension 1 and item y's negative value in dimension 2. Since options are designed to have an equally bad, but different feature, we assign option x value $-q$ in dimension 1 and option y the very same value in dimension 2. Thus, x is described by the attribute pair $(-q, 0)$ and option y by $(0, -q)$. We assume that a subject's utility inferred from an item is given by an additively separable, piecewise linear utility function putting equal weights on the item's dimensions. Furthermore, according to prospect theory, the utility derived from each dimension can be assessed relatively to an exogenously given reference point via a positively sloped value function. This value function is assumed to be convex in the loss domain (i.e., below the reference point) and concave in the gain domain (i.e., above the reference point), putting greater weight on losses than on equally sized gains. Before assignment of any endowment, state $(0, 0)$ gives a subject's reference point. After an item is assigned, a subject adjusts her reference point accordingly. As depicted in Figure 1, if a subject is endowed with item x, her reference point r will adjust to x. If hereafter she is allowed to switch her bad x for the alternative bad y, she sticks to her endowment due to the following reasoning. Choosing option x yields a negative utility which results from its negative feature in dimension 1 (given by the value $-q$). If instead she switches to option y, the improvement q in dimension 1 cannot compensate the worsening $-q$ in dimension 2, since relative to the reference point $r = x$ the perceived "gain" in dimension 1 is rated lower than the perceived "loss" in dimension 2. Thus, option x is preferred to option y.[4]

Different models of prospect theory do not deliver different predictions concerning our

[4]This finding holds also under the weakened assumption that both bads share both downsides, but have different most severe downsides, i.e. $x = (-q, -p)$ and $y = (-p, -q)$ with $q > p$.

Figure 1: *An illustration of the setup: endowed option x and alternative y.*



experimental setup. Whether the reference point is given by the status quo (Kahneman et al., 1991; Samuelson and Zeckhauser, 1988) or by a subject's expectations (Kőszegi and Rabin, 2006) does not alter the predictions regarding exchange preferences for bads, since in our setup the reference point equals the task the subject is endowed with in either case. While the preceding reasoning assumes that the reference point equals the status quo, we provide the derivation of the endowment effect for bads according to Kőszegi and Rabin (2006) in Appendix A. Thus, the prevalent versions of loss aversion-based theories yield the explicit prediction of the endowment effect for bads, which is expected to be as strong as the endowment effect for goods. Consequently, in our experimental setting, prospect theory yields a prediction which is contrary to salience theory's conjecture.

4 Experimental design

In this section, we provide the experimental setup for both our incentivized and the hypothetical studies. Our focus lies on the incentivization of our experiment, which is a novelty to the economic literature. However, to replicate the literature (Brenner et al.,

2007; Bhatia and Turan, 2012) we also provide hypothetical studies in section 4.2.

4.1 Incentivized setup

Our incentivized study which tests for exchange asymmetries of bads is designed as follows. Two unpleasant tasks serve as bads in our experiment. The first task consists of a basket of mixed black and white confetti which has to be sorted according to color (task "sorting").[5] The second task consists of one and a half sheets of checkered paper to be completely filled with zeros and ones in alternating order (task "zeros and ones").[6] Instructions, questionnaires, the decision sheet and detailed information about the procedure of the experiment can be found in Appendix B.

After arriving at the laboratory, subjects were randomly assigned one cubicle to sit in, which was already endowed with the material for one of the tasks. To eliminate a subject's potential tendency to take a task her neighbor or her friend has (for example, to compete against her), cubicles were separated. Otherwise, such competition effects could potentially have biased our results.

The instructions informed subjects about their own and the alternative task and provided general information about the experimental procedure. Questions during the experiment were answered privately by the experimenter. To ensure that subjects read the instructions for both tasks, control questions had to be answered subsequently. Concerns about differences in time consumption between the two tasks were eliminated by informing subjects that both tasks were calibrated to be manageable within 30 minutes and that

[5] In the instructions, confetti is termed paper snips to avoid any positive association with the task.

[6] The bads' two dimensions salience and prospect theory refer to may be defined as follows. The first dimension states how "fiddly" a task is (which is the unique negative feature of the task sorting), whereas the second dimension states how "exhaustive" a task is (which is the unique feature attributed to the task zeros and ones). Weakening this assumption such that one task is considered more fiddly whereas the other task is considered more exhaustive does not change the prediction of either approach in the previous section qualitatively.

they could continue working until they were finished in the unlikely case of not fulfilling the task in time.[7] Additionally, participants were told that if they finished before time ran out, they would have to wait for the remaining time before the inspection of the results and the according payment. Furthermore, subjects were informed that the payment would be independent of the time they needed for completing the task. The overall payment for subjects who succeeded in their task was 12 Euro. In the case of errors or a cancellation of the task, only 4 Euro would be paid, a case which did not occur. In spite of the payment effect, we consider these two tasks as bads since a payment of 12 Euro is expected anyway for participating in an experiment of about an hour. However, these two tasks are extraordinarily unpleasant compared to the usual tasks incorporated in such laboratory experiments.[8]

Like announced in the instructions, in a subsequent introductory round participants familiarized themselves with their task and filled out a questionnaire on their task.[9] As

[7]Upfront, we carefully calibrated the tasks, so that both were properly doable within 30 minutes. For example, we used a pilot session to ensure that tasks are balanced concerning time exposure. According to questionnaires subjects filled out, tasks were balanced and needed on average 23.4 minutes for sorting and 26.7 minutes for zeros and ones according to those participants who answered the respective question on the questionnaire.

[8]To ensure that the disutilities of both tasks were generally balanced, we ran an anonymous online survey with 677 participants, mostly students, which was announced on facebook and asked for subjects' preferences with respect to sorting two-colored confetti for 30 minutes and writing zeros and ones on checkered paper for 30 minutes. 51% of subjects preferred the sorting task, 34% preferred the task zeros ans ones and 15% were indifferent between the two tasks. Thus, our tasks are roughly balanced. In this online survey, we also asked for subjects' relative preferences concerning other tasks like cutting out boxes of checkered paper or painting checkered paper in two colors. We decided for the two tasks due to their balance. For a further discussion of our bads, see Section 6.

[9]This procedure is supposed to help getting subjects' focus on the endowed bad as needed by attention-based theories in order to predict the reverse endowment effect. It is also in line with conventional studies on exchange asymmetries, where subjects get some time to inspect their endowment. However, this introductory round was omitted in a second treatment.

subjects knew beforehand from the instructions, they had the possibility to test their task. If, however, people started working on their tasks, then advances at this stage were made undone by exchanging partly filled out sheets and remixing the confetti at the end of the introductory phase.[10]

Afterwards, people were informed that, before the actual task starts, they were given the choice to switch their endowed task against the other one described in the instructions; up to this point, subjects did not know about the opportunity to switch their task later on. Subjects were handed out a decision form with two boxes, one of which needed to be checked. One box represented "switching", the other one "not switching". Switchers were instantaneously endowed with the material for their desired task.[11] Then, all subjects were allowed to start working on their task for 30 minutes. Progress of time could always be checked via a large analog clock that was projected onto the laboratory's walls during the experiment. As soon as the 30 minutes were over, subjects were given a final questionnaire to be filled out after finishing their task. The answered questionnaire and the material were handed out to the experimenter privately, and, after a check for correctness and completeness of the task, subjects were paid.

In order to rule out that learning effects during the introductory phase may have driven our results, we had another treatment, in which subjects were not allowed to practice their task before the beginning of the official working time.[12] We modified our instructions insofar as in the introductory phase subjects had to fill out a questionnaire

[10]Importantly, to eliminate any biases in subjects' decisions due to the practice phase in this treatment, in the second treatment subjects were not given the opportunity to test their endowed task.

[11]With this procedure, we expect to eliminate transaction costs possibly going along with switching. Furthermore, we do not provide a default option to eliminate biases in decision making in favor of default options (Sunstein and Thaler, 2003). It was made clear that the payment for the task is independent of one's switching decision.

[12]Since this additional treatment was intended as a robustness check, we planned only three sessions to be conducted on the same day.

on their assigned task exclusively.[13]

We ran this experiment at the laboratory of DICE, University of Düsseldorf. Subjects were recruited via ORSEE (Greiner, 2004) and the experiment was carried out with paper and pen. All subjects finished and fulfilled their respective task correctly, so that earnings amounted to 12 Euro per subject. On average, the experiment took about 55 minutes.

To investigate the validity of the different mechanisms yielding the endowment effect for goods, we test the respective predictions concerning exchange preferences for bads, i.e., we test if the switching rate in our experiment strictly exceeds 50% significantly. If we can reject this hypothesis, we have an indication that the endowment effect carries over to bads as predicted by prospect theory, whereas otherwise we obtain an indication that the endowment effect is eliminated or even reversed for bads as predicted by salience theory. Thus, we test the following conjecture,

Hypothesis: The probability for switching the endowed bad is strictly above 50%.

4.2 Hypothetical setup

In line with Brenner et al. (2007) and Bhatia and Turan (2012), we designed hypothetical treatments where subjects had to make their decision without experiencing real consequences. In order to underline that we have indeed incorporated bads in our experiment, we intend to replicate Brenner et al. (2007) with the only change that their bads are substituted by our tasks. Consequently, however, our hypothetical setup cannot entirely represent a hypothetical version of our incentivized setup. As a compromise and to have some comparability to existing hypothetical studies„ we replicated the structure of Brenner et al. (2007) with as few changes of our incentivized setting as possible.

We conduct two different treatments, one with a strong and one with a neutral frame. The first treatment was designed to strengthen the downsides of the tasks by explicitly

[13]The questionnaire said: "Please write three sentences on your task. What do you think about your task?".

stating that both tasks are unpleasant, that the task sorting is especially fiddly and the task zeros and ones is especially exhausting. The other one was neutrally framed, by removing these negatively connoted words from the instructions.[14] Besides these slight differences, instructions for both treatments did not differ. Both setups reflect the structure proposed in Brenner et al. (2007) of first introducing the assigned bad, and bringing in the alternative only later on as the possibility of switching is mentioned. This ensures that also in the hypothetical treatment the subject's focus lies on the own task and not on the alternative. Besides these modifications, we did not alter the incentivized setup. Instructions for the neutrally framed hypothetical treatments are provided in Appendix B. Table 1 provides a short overview over the different treatments we implemented along with the number of participants within each of the treatments. The results are presented in the following section.

Treatment	Description	# of subjects
Incent., with practice	subjects could practice on their task initially	79
Incent., w/o practice	no practice, only a questionnaire on the endowed bad	50
Hypot., framed	tasks are described with negatively connoted words	85
Hypot., neutral	evaluative words are omitted	71

Table 1: *An overview over the different treatments.*

5 Experimental results

5.1 Incentivized setup

We ran five sessions with 79 participants in the main treatment, of which 18 subjects switched their task and 61 subjects stayed with their task. Table 2 gives the absolute

[14]One of the three slight changes between the wording in the treatments is the following: "You have been assigned the unpleasant task sorting" became "You have been assigned the task sorting".

numbers of switching and non-switching subjects for each endowment. A large part of participants stayed with their assigned bad, irrespective of the task they were endowed with.

		Decision	
		Switch	No Switch
Endowment	Sorting	6	32
	Zeros and Ones	12	29

Table 2: *Absolute numbers of subjects' switching decisions for each endowment.*

For the Hypothesis, the one-sided binomial probability test gives $p<0.00001$. Thus, significantly less than 50% of the subjects, i.e., about 23%, switched their tasks. This replicates switching rates from conventional papers on exchange asymmetries for goods such as Knetsch (1989), who finds an average switching rate for endowed goods of approximately 10%, or Kahneman et al. (1991), who sum up results of various replications of exchange-asymmetry studies by stating that traded volume was always less than half of the expected volume, i.e., less than 25%.

Considering tasks separately reveals switching rates significantly below 50% for each of the tasks. From 38 subjects who were endowed with confetti, only 6 subjects switched ($p=0.000012$); from 41 subjects who were endowed with zeros and ones, 12 switched ($p=0.0058$). Consequently, we obtain a strong indication that the endowment effect carries over to the unpleasant tasks incorporated in this experiment.

For the second treatment, in which subjects could not practice anymore, results are not significantly different. The observed choice patterns did not change as only 9 out of 50 participants (18%) switched their task in this additional treatment. Pooling the data of both treatments further increases the significance of our results since only 21% of overall subjects in our incentivized treatments switched their initially endowed task.

5.2 Hypothetical setup

In both treatments, switching rates were above 50% for both tasks, see Tables 3 and 4. Whereas in the first hypothetical treatment, 55% of the subjects switched their task, the number of switchers was even larger in the neutrally framed treatment (58%). However, results in both hypothetical treatments were not significantly different, so that we pooled the data. Altogether, significantly more than 50% of the subjects switched their task ($p = 0.064$, one-sided binomial probability test), which reveals a (slight) reverse endowment effect and thus reproduces findings of Brenner et al. (2007) and Bhatia and Turan (2012). In particular, the switching rate in the hypothetical treatment is significantly higher than the switching rate in the incentivized treatments ($p<0.00001$).

		Decision	
		Switch	No Switch
Endowment	Sorting	24	20
	Zeros and Ones	23	18

Table 3: *Subjects' decisions in the strongly framed hypothetical treatment.*

		Decision	
		Switch	No Switch
Endowment	Sorting	20	15
	Zeros and Ones	21	15

Table 4: *Absolute numbers of subjects' switching decisions in the neutrally framed hypothetical treatment.*

6 Discussion

In this section, we discuss different features of our experiment and possible objections against our findings. In particular, we argue why we indeed incorporated bads in our

experiment and why learning effects or issues raised by Plott and Zeiler (2007) cannot explain our findings.

In our experiment, we considered two unpleasant tasks as bads. We assume that a utility of zero represents a condition in which a subject feels neither pleasure nor displeasure. Thus, an item yielding a negative utility (a bad) means that a subject is exposed to unpleasant conditions. Incorporating bads is not easy in a laboratory experiment. Physical bads, like annoying waste, do a poor job in an experiment since ignoring them can be assumed to give a utility close to zero. Pain or a punishment yield displeasure and negative utilities, but are not feasible in an experiment.[15] Therefore, we decided to incorporate unpleasant tasks as bads. Subjects in laboratories who are endowed with bads always need to be compensated with a lump-sum payment which exceeds their willingness to pay to get rid of their endowed bad, since otherwise abandoning the experiment is the natural option. Even though subjects are rewarded by monetary payoffs for accomplishing their task, we consider these unpleasant tasks as bads. Subjects expect to get paid anyway, just for participating in a laboratory experiment. However, in other experiments run at the economics' laboratory in Düsseldorf, tasks are far from being comparably unpleasant. Thus, both tasks in this experiment are worse than expected, so that according to subjects' expectations, fulfilling the specific task is a certain discomfort and therefore a bad.

Further evidence that our tasks indeed serve as bads is provided by an evaluation of questionnaires filled out in the experiment. Around 50% of the subjects describe their task as "strongly boring", "unpleasant", "laborious" or via a synonym, implying that the endowed task exposes them to unpleasant conditions and thus represents a bad. Out of these 38 subjects, the switching rate is not higher than the overall switching rate, only 9 of these subjects switch. Furthermore, a majority of 58 subjects use very negatively

[15] One exception is a paper by Berns et al. (2007), who study probability weighting in lotteries with "non-monetary adverse outcomes" (electric shocks).

connoted words like "stupid", "boring" or "senseless" to describe their task, which strongly supports our assumptions on the tasks.[16]

Finally, the hypothetical treatment supports our assumption that we involved bads in our experiment, too. Since the endowment effect for goods is a very robust finding both in hypothetical and incentivized studies (see for example Kahneman et al., 1991; Horowitz and McConell, 2002), its entire elimination in our hypothetical setup indicates that the tasks indeed do not represent goods, but bads.

To implement the choice between different non-risky options, we made the alternative options clearly visible and included an illustrating picture in the instructions. Furthermore, we incorporated tasks which everybody is in principle familiar with, like writing numbers or doing fiddly stuff. However, subjects may have been uncertain concerning the probability of accomplishing the respective task on time. Therefore, we assured that both tasks are doable within the provided 30 minutes, that quicker performance bears no advantage and that, if necessary, subjects could also extend their working time. Thus, we think that the introductory phase does not eliminate uncertainty in favor of the endowed option, so that our findings are not driven by issues of risk. Especially, our robustness-check supports this view, since here the chance to reduce risk associated with the own task by practicing in the introductory phase is explicitly excluded. Furthermore, we consider the incorporated tasks as not too similar since sorting and writing demand different capabilities. Sorting needs more concentration and patience, whereas writing is more strenuous. Thus, people indeed face a decision between different, rather non-risky options.

Furthermore, we think it is appropriate to consider tasks as endowments. Brenner et al. (2007), Bhatia and Turan (2012) and Dhar et al. (1999) have investigated exchange asymmetries for bads without incorporating physical endowments, too. Although different from our endowments, the common feature of these studies and our experiment is

[16]Further evidence that subjects really disliked their tasks is given by the fact that material to build up the separated cubicles was demolished partly and by comments like "If the next experiment where I take take part in is comparably stupid, I will quit going to experimental sessions" or "I hate the tasks".

that endowments are not physical ones like mugs, pens or chocolate bars. Additionally, experiments studying exchange asymmetries have incorporated a large variety of physical and non-physical items (see Horowitz and McConell, 2002; 2003), so that in the light of previous studies our tasks should serve as endowments, too.

Learning effects cannot explain our results in the incentivized setup since they were explicitly ruled out in our additional treatment, which we ran as a robustness check. Further evidence that learning was also no issue in our main treatment is provided by the average time switchers needed to fulfill their task. Subjects switching from sorting to zeros and ones did not need significantly more time, i.e., 27.4 minutes compared to 26.7 minutes, whereas switchers from zeros and ones to sorting needed on average 23.4 minutes, exactly as long as non-switchers needed on average.

Since findings of the endowment effect are often due to various confounds listed by Plott and Zeiler (2007), we controlled for these issues. First, to avoid emotional relations the subject might draw between the endowment and the experimenter, endowments were not given to the subjects by the experimenter, but were placed on the tables beforehand and subjects were randomly assigned to tables. Second, we incorporated a neutral wording which does not signal that staying or switching is the "correct" choice. Furthermore, the chosen language does not imply that one of the tasks might be more desirable or easier than the alternative. Third, we minimized transaction costs by requiring an active decision for one of the options and exchanging endowments instantaneously in case of switching. The switching decision did not result in any delay, not even the slightest, since any participant could only start working as soon as everybody had received her chosen material. Fourth, separated cubicles eliminated the influence of public revelation on decision making. Thus, we think that the key drivers of the endowment effect's occurrence as listed by Plott and Zeiler (2007), i.e. language, issues of relative value, transaction costs and the influence of public revelation, are no issue in our experimental setup.

We avoided training rounds for both tasks or pre-test trading rounds as comparable to

those in Engelmann and Hollard (2010) in order to keep the clear prediction of a reverse endowment effect by attention-based theories. In the case of prior experience with both tasks, it is unclear which degree of attention is designated to which task when a subject makes her final decision. Therefore, by being endowed with a certain bad after experience was gathered for both tasks, the available bad alternative may be equally vivid in subjects' minds such that the salience-mechanism which we introduced may not apply in this setup. Thus, it remains unclear if attention-effects indeed predict a reverse endowment effect for bads. Consequently, by introducing prior practice rounds, predictions by attention-based theories would become much fuzzier.

Finally, we decided against a study investigating willingness-to-accept (wta) and willingness-to-pay (wtp) gaps since the presence of an endowment effect for money would create an important confound. In detail, a *wta-wtp* study would look as follows. There are two treatments, the wtp and the wta treatment, in each of which the Becker-DeGroot-Marschak (BGM) mechanism is incorporated for the range of 0 Euro to w Euro. In the wtp-treatment, subjects in the laboratory are endowed with a certain bad and an amount of money which we assume to equal z Euro. Via BGM, we may elicit the subject's $wtp = x \in [0, w]$ Euro as the maximum amount of money the subject would give up in order to get rid of the bad. In the wta-treatment, a subject is endowed with $z - w$ Euro and via BGM, the smallest acceptable amount of money $y \in [0, w]$ for which the subject is willing to take the bad could be elicited. Whereas rational choice would predict that $x = y$, salience theory and the reverse endowment effect for bads would imply that $wtp > wta$, i.e. a subject perceives a bad to be worse if she is endowed with it. This setup, however, bears the following confound. If we find that $wtp \leq wta$, we cannot necessarily infer that the reverse endowment effect for bads does not exist. If subjects have a strong endowment effect for money, then the wtp will be relatively low, regardless of the existence or non-existence of the reverse endowment effect for bads. The endowment effect for money may be especially strong for experimental subjects as the main motivation for

participating in laboratory experiments is the money subjects could take home. We are also not aware of any studies testing the *wta-wtp* gap for bads, neither hypothetical nor incentivized. In contrast, the endowment effect for money cannot superpose the endowment effect for goods. Consequently, we consider a test for exchange asymmetries for bads as advantageous towards a test for $wta - wtp$ gaps in order to decide if the reverse endowment effect for bads does or does not exist.

7 Hypothetical versus incentivized experiments

In this section, we focus on the difference which we have found between the incentivized and the hypothetical treatments. The results in our incentivized setup challenge findings of hypothetical studies (Brenner et al., 2007; Dhar et al., 1999; Bhatia and Turan, 2012) which report a reversal of the endowment effect for bads. These differences may come from the following procedure. Assigning somebody with a bad makes her feel uncomfortable with her endowment. Intuitively, she may like to switch her bad against anything else just to get rid of it, as Bordalo et al. (2012b) propose. Thus, if outcomes are hypothetical, decision making may be based on the subject's first intuition.

This, however, may not reflect her actual choice when facing real consequences. Exchange experiments like ours give a subject much more time to empathize in the situation of the incentivized compared to the hypothetical treatment. As Loewenstein and Adler (1995) find, there is an empathy gap which prevents subjects from anticipating how the endowment will make them feel. This reasoning might apply to our hypothetical experiment. Only if the decision making is incentivized, the decision maker is truly involved in the setting and thus has a second thought. This involvement into the situation shifts the reference point towards the endowment. By adapting the endowment as a reference point, however, the subject refrains from switching since she would gain in one dimension, but lose in the other dimension. In contrast to her first desire to get rid of the bad, after adopting her reference point, she refrains from switching since she is loss averse. This

prediction by prospect theory is perfectly in line both with our observations and with subjects' comments on the questionnaires like the following: "I already prepared myself mentally to do the assigned task" or "In the beginning, I thought the other task would be better, but then I did not switch because I already adapted myself to my task". These comments indicate that the mechanism proposed by prospect theory is really at work.

Hereby, we add to the literature which finds important differences between hypothetical and incentivized studies. For example, concerning the domain of risk aversion, Harrison (2006) finds that subjects respond differently to risky prospects when they face real economic consequences of their choices instead of hypothetical economic consequences. Vlaev (2012)'s results call into question established methodologies that rely on hypothetical answers with respect to social interaction. Most interesting, Azar (2007) tests his theory of "relative thinking" in a field experiment, which shares its central prediction with salience theory of consumer choice (Bordalo et al., 2013). He tests the hypothesis that given the choice between two vertically differentiated goods (where the less-quality good is cheaper), an uniform increase in prices shifts demand towards the more expensive, higher-quality good. Whereas he rejects this hypothesis based on his field experiment's data, he finds it to be confirmed in an hypothetical setup. This indicates differences in incentivized and hypothetical choice situations if salience plays a major role, which is also given in our experiment. Our results clearly indicate a difference between setups with real, bad consequences and hypothetical decisions on bads. Since we are the first to provide an incentivized test of the endowment effect for bads, we suppose that our findings resemble real behavior in a more appropriate way than the aforementioned (hypothetical) studies do.

8 Conclusion

In this experiment, we investigate exchange asymmetries for bads. Whereas prospect theory (Kahneman and Tversky, 1979 and 1991) and related models (Kőszegi and Rabin,

2006) predict an endowment effect for bads, attention-based theories (Bordalo et al., 2012a,b; Kőszegi and Szeidl, 2013; Bhatia and Golman, 2013) presume a reversal of the endowment effect. Both classes of models share a number of behavioral predictions concerning biased decision making. In order to distinguish between the theories' validity, it is necessary to consider a case where predictions of both approaches contradict each other. Thus, we analyze exchange rates for bads in an incentivized laboratory experiment.

In favor of prospect theory, but contrary to the prediction of attention-based theories, in our incentivized settings we find a robust endowment effect for bads. Both interpretations of prospect theory, the first one incorporating the status quo as the reference point (Kahneman et al., 1991; Samuelson and Zeckhauser, 1988) and the second one defining a subject's expectations as the reference point (Kőszegi and Rabin, 2006), predict our experimental finding of an endowment effect for bads, since in our experiment a subject's expectations coincide with the status quo: after being endowed with a task, each subject expects to carry out her assigned task. As prospect theory does not distinguish between a reference point in the gain or loss domain of utility, the endowment effect is predicted not only for goods, but also for bads. However, the salience mechanism (Bordalo et al., 2012b) - which considers the endowment effect as a result of biased attention - predicts a reverse endowment effect, contrary to our findings. As our results are highly significant, we find a clear indication that the endowment effect is indeed a loss aversion-based and not an attention-based effect. Attention effects may not be strong enough to carry over to the two-stage procedure described in Bordalo et al. (2012b). Whereas attention-based theories have a strong descriptive power in general, our study implies that they may have a weak predictive power, at least with respect to exchange asymmetries for bads.

Furthermore, our findings stress the universality of the status quo bias. The results imply that people do not only have strong preferences in favor of the status quo if this is a pleasant one, but also if the status quo is rather unpleasant. Therefore, our findings may indicate that people are locked in bad jobs or marriages instead of opting for other

(bad) alternatives, e.g. looking for another poorly paid job or loosing tax advantages by being single. While reference-dependence can explain the reluctance to decide for other (bad) options, attention-based theories would predict the opposite. Another implication of the presence of an endowment effect for bads might be customer loyalty towards low quality products which might be exploited by firms. Consequently, an endowment effect for bads may also have important practical implications.

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Appendix

Appendix A: Exchange asymmetries for bads

The reverse endowment effect according to the salience mechanism

We illustrate the salience mechanism presented in Bordalo et al. (2012b), according to which each item's attribute has a certain degree of salience. A local thinker (LT), i.e., an individual susceptible to salience, assigns more weight to an attribute the more salient it is. An item's attribute is the more salient the more it differs from the average within the consideration set, the set comprising all options which are considered by a subject. Thus, attributes of an item which match the average within the consideration set tend to be neglected, whereas attributes differing from the average tend to be overvalued.

The degree of over- and undervaluation is indicated by a parameter $\delta \in (0, 1]$. The smaller δ, the higher is the susceptibility to salience, with $\delta = 1$ indicating the rational

individual, and $\delta \to 0$ indicating the extreme local thinker valuing almost only the most salient attribute. Assuming two attributes, the weight on the overvalued attribute is given by $\frac{1}{1+\delta}$ and the weight on the undervalued feature by $\frac{\delta}{1+\delta}$.

In the following, we illustrate how the salience mechanism predicts a reversal of the endowment effect for the bads incorporated in our experiment. First, we assume that an individual's utility function is linear and additively separable with respect to an item's attributes. We consider two alternatives with a different, but equally bad, unique feature. Each alternative can be described by values in the same two dimensions: the first one indicates how fiddly the task is, the second one indicates how exhausting the task is. We suppose that the task "zeros and ones", abbreviated by Z, can be described by the pair of attribute values $(0, -q)$ since this task is assumed to be exhausting, but not fiddly. The minus sign indicates that the respective utility obtained from the attribute is negative. The task "sorting" (S), however, can be described by the pair $(-q, 0)$, indicating that it is fiddly, but not exhausting.

At the first stage, each subject is assigned one task, for example the fiddly task S. The consideration set of the subject now contains the two elements S and $(0, 0)$, where the latter indicates being endowed with neither a fiddly nor an exhausting task, which represents the subject's pre-assignment status and her pre-assignment expectations. The second dimension (exhaustiveness) of the task fits the average within the consideration set, since both elements are described by value zero in the second attribute, whereas the item's first dimension differs from the average within the consideration set, $-q < \frac{-q}{2}$. Therefore, the fiddliness of S is salient and thus overvalued. The local thinker's valuation of task S at the first stage is given by $v_1^{LT}(S) = \frac{\delta}{1+\delta} \cdot 0 - \frac{1}{1+\delta} q$.

At the second stage, people may switch their endowment so that the second stage's consideration set comprises the two tasks given by $(-q, 0)$ and $(0, -q)$. Since for both items each attribute's difference from the average within the consideration set is in absolute value $\frac{q}{2}$, both items have one relative upside and one relative downside and consequently

they are assessed equally. Since Bordalo et al. (2012b) assume diminishing sensitivity, each item's upside is salient at this stage since the difference between 0, i.e., the value each item's upside is described by, and $-\frac{q}{2}$, i.e., each attribute's average within the consideration set, is perceived stronger than the difference between $-q$, i.e., the value each item's downside is described by, and $-\frac{q}{2}$. Thus, the weight on each item's upside is $\frac{1}{1+\delta}$ and the weight on each item's downside is $\frac{\delta}{1+\delta}$. Consequently, both items' valuation at the second stage is given by $v_2^{LT}(S) = v_2^{LT}(Z) = \frac{1}{1+\delta} \cdot 0 - \frac{\delta}{1+\delta} \cdot q$.[17]

However, according to the salience mechanism, the first stage's valuation of the endowed item partly persists to the second stage, so that the final assessment of the endowment is a compound of the first and the second stage valuation. The weights of the first and second stage valuation which are relevant for the final assessment are assumed to be given by $\gamma \in (0, 1]$ and $1-\gamma$. Thus, the initial task is valued $v(S) = \gamma(-\frac{1}{1+\delta}q) + (1-\gamma)(-\frac{\delta}{1+\delta}q)$. This is smaller than the valuation of the alternative Z, which is given by $v(Z) = -\frac{\delta}{1+\delta}q$. Thus, local thinkers are expected to switch bads against each other, at least as long as our assumption holds that preferences are not spread very heterogeneously among tasks.

The endowment effect for bads according to Kőszegi and Rabin (2006)

We show that predictions of loss aversion-based theories with respect to the endowment effect do not change for bads compared to goods when the reference point is given by a decision maker's expectations. According to Kőszegi and Rabin (2006), overall utility from consuming c given reference point r is given by

$$U(c|r) = m(c) + \mu(m(c) - m(r)), \tag{1}$$

[17] If the assumption of diminishing sensitivity is dropped, like in focusing theory, both items are valued equally at the second stage, too. If neither the up- nor the downside are assumed to be salient in a direct comparison of both bads, then both are assessed rational at this stage. In either case, the subsequent argumentation and the prediction of the reverse endowment effect remain valid.

where $m(c)$ is the standard utility derived from the consumption of c, and $\mu(m(c)-m(r))$ is a function depending on the difference between the actual consumption utility and the reference consumption utility, which gives the gain-loss utility relative to the reference point. We assume that the standard utility and the gain-loss utility are additively separable across dimensions and, additionally, that the properties of the value function of Kahneman and Tversky (1979) are satisfied by μ.[18] This is for example provided by a piecewise linear function μ, given by $\mu(x) = \eta x$ if $x > 0$ and $\mu(x) = \eta\lambda x$ if $x \leq 0$, where parameter $\eta > 0$ is a measure of the weight a decision maker assigns to the gain-loss utility and λ is a coefficient of loss aversion. As according to prospect theory, we assume that losses are weighted more than gains relative to the reference point, i.e., $\lambda > 1$. Here, S gives the multiplicity of sorting-tasks an individual is endowed with (either 0 or 1) and Z gives the multiplicity of zeros and ones-tasks an individual is endowed with (either 0 or 1). We assume that the task sorting yields a negative utility of -1 derived from its first dimension fiddliness and a utility of zero derived from its second dimension, whereas this is vice versa for the task zeros and ones.[19] A subject's reference point concerning task $X \in \{S, Z\}$ is denoted $r_X \in \{0, 1\}$, which stands for the expectation of either fulfilling the task (i.e., $r_X = 1$) or not (i.e., $r_X = 0$). For example, $r_S = 1$ denotes a subject's expectation to carry out task S, which enters with utility -1 into the argument of function μ in (1). Thus, we have

$$U(S, Z|r_S, r_Z) = -S + \mu(-S - (-r_S)) - Z + \mu(-Z - (-r_Z)).$$

Without loss of generality, we suppose that the subject is endowed with the sorting task, i.e., $r_S = 1$ and $r_Z = 0$.

First, in case she does not switch, we have $S = 1$ and $Z = 0$ and her utility is given by $U(S, Z|r_S, r_Z) = -1+\mu(-1+1)+0+\mu(-0+0)$, which is equivalent to $U(S, Z|r_S, r_Z) = -1$.

[18] We assume that the gain-loss utility function μ is the same for both dimensions.

[19] This is very similar to the modeling of the tasks in the previous subsection with the only difference that we have substituted parameter q by value 1 here.

Second, in case she switches, we have $S = 0$ and $Z = 1$ and her utility is given by $U(S, Z|r_S, r_Z) = 0 + \mu(0 + 1) - 1 + \mu(-1 + 0) = -1 + \eta(1 - \lambda)$. As we assume $\lambda > 1$ and $\eta > 0$, she does not opt for the alternative, but sticks to her endowment.

Appendix B: Experimental procedure of the main treatment

How the sample size is determined is reported, as well as data exclusions (if any), all manipulations, and all measures in the study (Simmons et al., 2012).[20]

1) We welcome subjects and let them randomly draw a number between one and eighteen. Material is already set at the eighteen cubicles: For cubicles 1-9, confetti is installed, and cubicles 9-18 are endowed with zeros and ones. We let subjects sit in the cubicle with the number of their draw.

2) We give the instructions to the subjects and emphasize that they are to be read for both tasks. In the end, subjects have to answer control questions on both tasks to ensure that they read both descriptions.

3) After all subjects answered the control questions correctly, a questionnaire for the assigned task is handed out (see Figure 5) and the introductory period is started.

4) After a few minutes, the trial phase ends and questionnaires are collected. Already sorted confetti is remixed and filled out paper sheets are replaced.

5) Subjects are orally informed about the possibility to switch the assigned task: "Before the 30 minutes start, you have the option to switch from your assigned task to the other task described in the instructions. You will receive a decision form in which you need to check one of the two boxes, one for staying with your assigned

[20] We choose sample sizes (of around 80) which are typical for individual decision-making experiments and for studies on the endowment effect. To mention one example, 74 subjects participated in Experiment 1 of Engelmann and Hollard (2010).

task and the other one for switching to the other task. Before the task starts, you will receive the material for the task you chose. The payment for the alternative task is exactly the same: fulfilling the task correctly and completely gives you 8 Euro, independent of whether you switch tasks or not. Once the 30 minutes start, there is no further opportunity to switch tasks, but you need to finish your chosen task."

6) The decision form is handed out (see Figure 4).

7) The decision form is collected and each switcher is endowed with the desired task.

9) The task is started and the time for accomplishing the task begins (30 minutes).

10) After the 30 minutes, the final questionnaire is handed out (see Figure 6).

11) Results are inspected and subjects get paid privately against a receipt.

On the next pages, we provide a translation of the instructions for subjects in the main treatment (i.e., where subjects could also try their task in the introductory phase) endowed with the task sorting. Instructions for subjects endowed with the task "zeros and ones" were analogous. For the robustness check, instructions were the same except for the fact that subjects were not allowed to practice their task but only to inspect it and to fill out the questionnaire. For the hypothetical treatment, the neutrally framed instructions are provided in Figures 2 and 3. In Figures 7 and 8, we provide pictures of the cubicles the subjects were seated in.

Instructions

Welcome to today's experiment. Please do not talk to other participants from now on. If you have any questions during the experiment, please raise your hand. We will answer your question privately. Please read the instructions carefully.

Please fill in the blanks before you read the instructions:

Your age: _____

Your major: _____

Your sex (m/ w): _____

By randomly drawing a number for a cubicle to be seated in, one of the following two tasks was randomly assigned to you. **Your task is ``Sorting''** (see next page). **You only need to fulfill this task**.

Nevertheless, please read the instructions for both tasks. Thus, please also read the instructions for task `` Zeros and Ones''. Both tasks will be paid equally. You have 30 minutes to fulfill your task. Correctly finishing the task will earn you 8 Euro. In total you can earn 12 Euro for participating in this experiment.

In the following both tasks are described in detail.

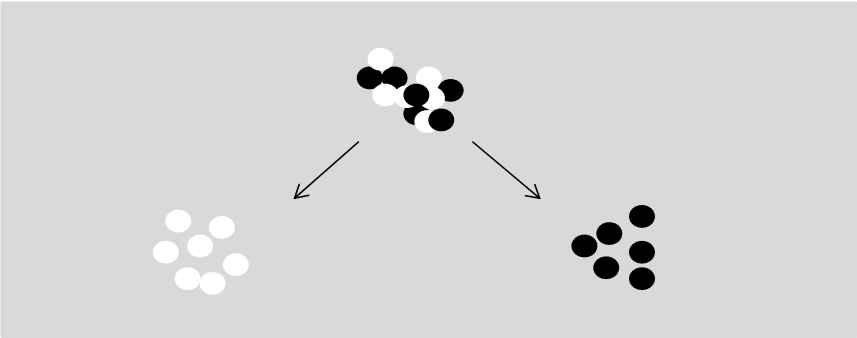
TASK 1: SORTING (Your task)

In your task you have to sort a certain amount of paper snips according to color. You receive a basket with black and white paper snips and additionally two empty baskets. Please sort the black paper snips in one empty basket and the white ones in the other empty basket. At the end of the experiment, the baskets with the sorted material are handed out to the experimenter.

For this task you have 30 minutes. The amount of paper snips is calibrated such that you can easily manage this task within time given an appropriate speed. If you finish before the 30 minutes are over, you will have to wait until time runs out. Therefore, you gain nothing by working very fast. In case you do not manage to finish within the given time, you get some additional minutes to finish the task.

Completely and correctly sorted paper snips are mandatory for getting the payment! We will control both the amount and the correctness of sorting before we pay you accordingly. Therefore, please make sure you do not lose some paper snips.

Illustration of the task:



TASK 2: ZEROS AND ONES

The other task requires writing ``0'' and ``1'' on one and a half sheets of checkered paper in alternating order. The first box in a row should be started with a ``0'', the second box should be filled with a ``1'', the third one with a ``0'' and so on until the end of a row. The first box of the next row should be started again with a ``0'', and then it should be proceeded as in the previous row in alternating order. This is to be done for the given sheets of paper. At the end of this task every single box should contain one number. In the end of the experiment, all filled sheets are handed out to the experimenter.

For this task you have 30 minutes. The amount of paper is calibrated such that you can easily manage this task within time given an appropriate speed. If you finish before the 30 minutes are over, you will have to wait until time runs out. Therefore, you gain nothing by working very fast. In case you do not manage to finish within the given time, you get some additional minutes to finish the task.

It is necessary that every single box contains one number (either a zero or a one) in the correct order to get the payment. We will control the sheets before we pay you accordingly.

Illustration of the task:

HEINRICH HEINE
UNIVERSITÄT DÜSSELDORF

Procedure of the experiment

Before the actual task starts, there is a trial period in which you can familiarize yourself with your assigned task (Sorting). We will hand out an additional questionnaire for your task. Please fill out this questionnaire during this period. The time for the trial does not count for the 30 minutes. Thus, time does not run during the trial. What is sorted by you during this time does also not count for the amount to be sorted within the 30 minutes. Everything that has been sorted will be remixed before the actual task starts. Therefore, you cannot work in advance. For this part of the experiment (trial and questionnaire) you earn 4 Euro.

After that, you have 30 minutes for the actual task. Please carry out your task correctly. In case time runs out before you finish your task, you will receive some additional minutes. If you finish earlier we ask you to wait silently at your cubicle until the 30 minutes are over. Fulfilling the task correctly gives you 8 Euro.

In total you can earn 12 Euro for participating in this experiment: 4 Euro for the trial and the questionnaire and 8 Euro for the correctly fulfilled task, Sorting.

Control questions (only to make sure you read the instructions for both tasks):

Please provide short answers:

1) What needs to be done for the task SORTING?

_____

2) What needs to be done for the task ZEROS AND ONES?

_____

3) What happens if you are finished after 20 minutes?

_____

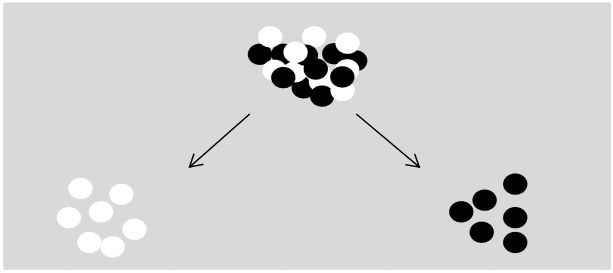
4) Which task is yours?

_____

Instructions:

Please, imagine the following situation and answer honestly.

You participate in an economics experiment for about an hour and you will earn 12 Euro for fulfilling all requirements correctly. While you are used to do tasks at the PC in such experiments, this time, you have been assigned the task **sorting**: Sort within 30 minutes a given and exactly calibrated amount of mixed black and white paper snips completely according to the color. In case you need more than 30 minutes you get up to 5 additional minutes until all the paper snips are completely sorted. In case you finish before the 30 minutes are over, you need to wait silently at your seat until the time runs out; working fast does not bear any advantage. The following picture illustrates this task.



You need to have sorted the given amount completely and correctly to receive the payment. The paper snips are small, made from low-grade paper and may stick together partly.

Before now the actual task starts, you get the surprising opportunity to switch your task immediately against another task named **zeros and ones**: Write zeros and ones in alternating order in every box of one and a half sheets of checkered paper. This task takes approximately as much time as the first task. The conditions with respect to time for fulfilling the task and the payment are the same for both tasks. The following picture illustrates this task.

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Figure 2: Hypothetical instructions for those endowed with the task “sorting”, page 1.

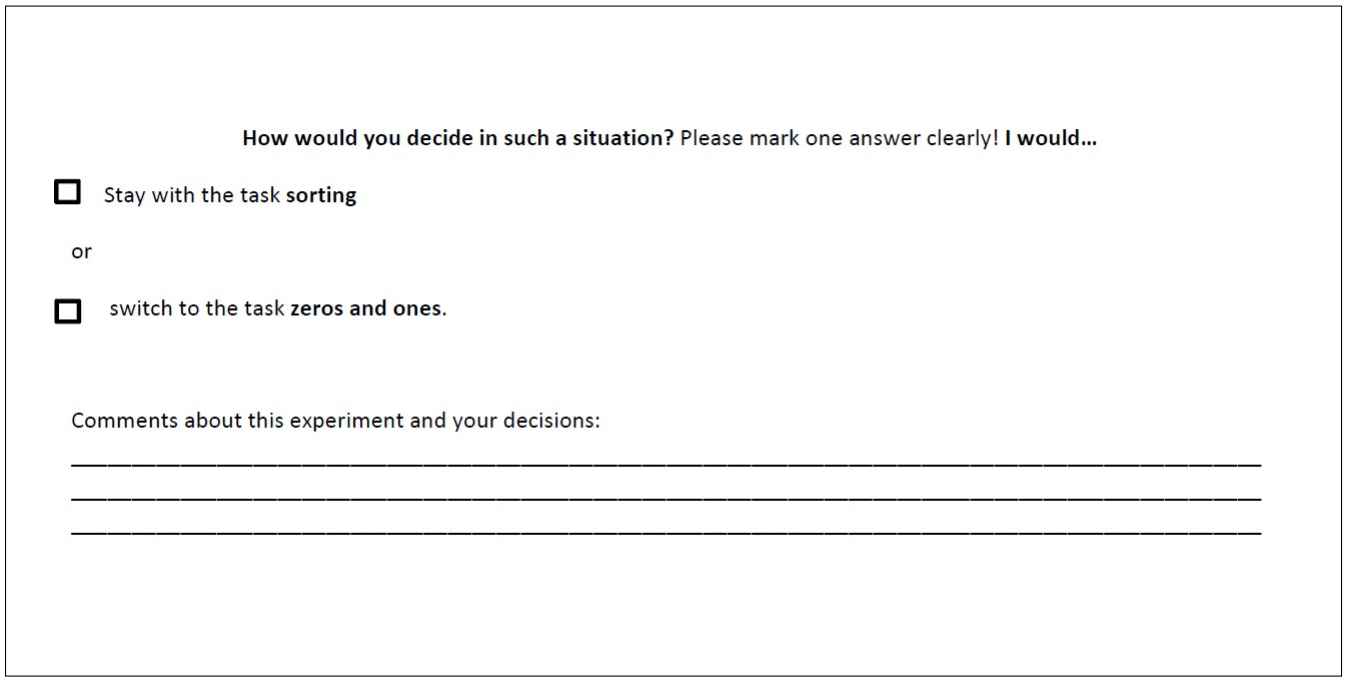

Figure 3: Hypothetical instructions for those endowed with the task "sorting", page 2.

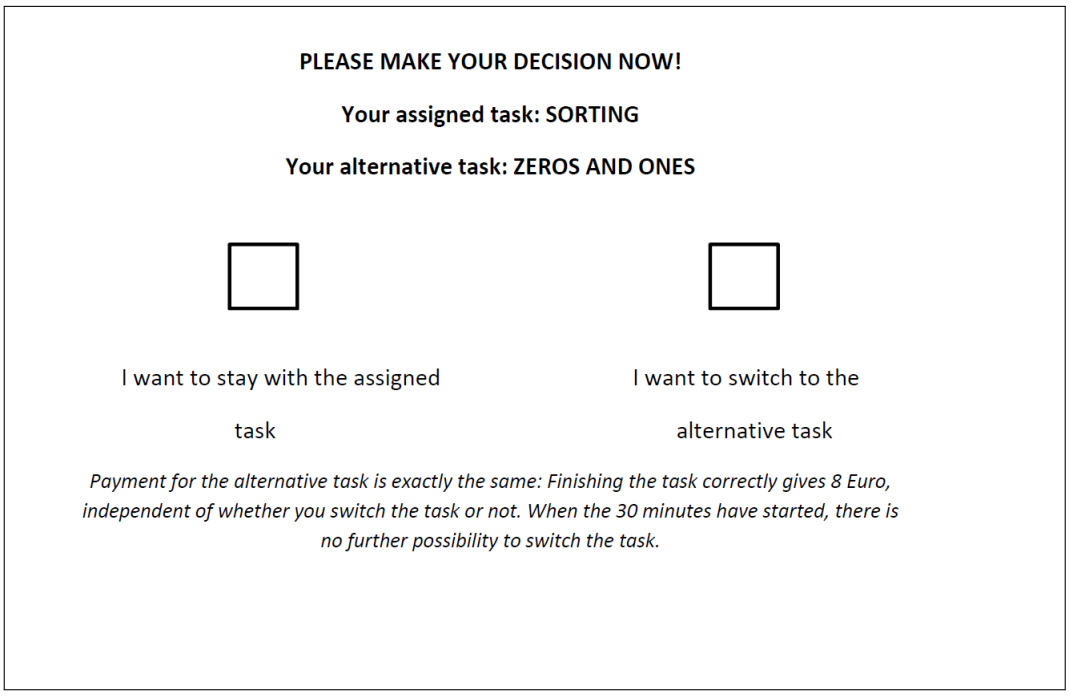

Figure 4: The decision form for subjects endowed with the task "sorting".

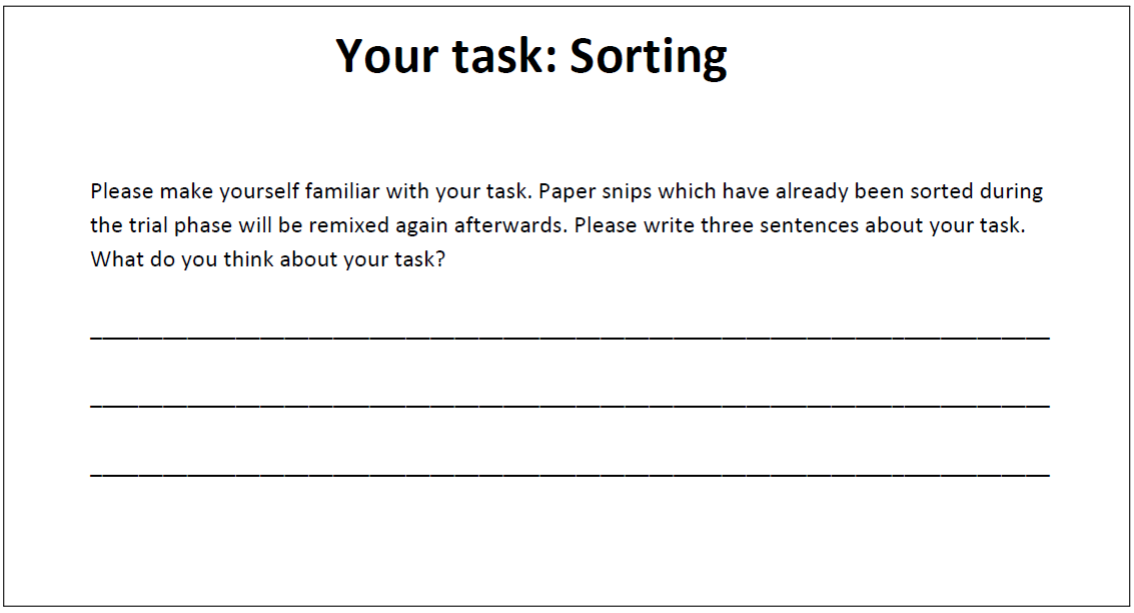

Figure 5: Questionnaire for subjects endowed with the task "sorting".

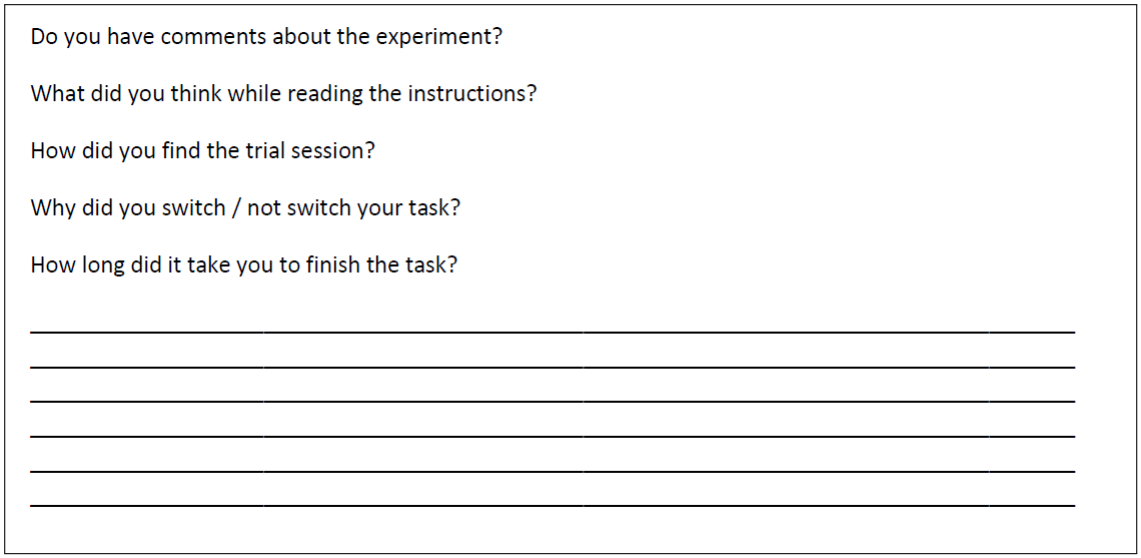

Figure 6: Final questionnaire.

Figure 7: Cubicle for subjects endowed with task “zeros and ones”.

Figure 8: Cubicle for subjects endowed with task “sorting”.

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