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The Endowment Effect in the General Population

Abstract

We study the endowment effect and expectation-based reference points in the field leveraging the setup of the Socio-Economic Panel. Households receive a small item for taking part in the panel, and we randomly assign respondents either a towel or a notebook, which they can exchange at the end of the interview. We observe a trading rate of 32 percent, consistent with an endowment effect, but no relationship with loss aversion. Manipulating expectations of the exchange opportunity, we find no support for expectation-based reference points. However, trading predicts residential mobility and is related to stock-market participation, i.e., economic decisions that entail parting with existing resources.

JEL-Codes: C930, D840, D910.

Keywords: exchange asymmetry, reference-dependent preferences, loss aversion, field experiment, SOEP.

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1 Introduction

People tend to value objects they own more than similar objects they do not own. While this observation, commonly known as the endowment effect, is one of the most prominent behavioral anomalies in economics (Kahneman, Knetsch and Thaler, 1991; Ericson and Fuster, 2014; O'Donoghue and Sprenger, 2018), it is the subject of an ongoing debate over its robustness and underlying causes. Ever since Thaler (1980) described the endowment effect, it has been tied to loss aversion and has significantly influenced the development of theories of reference-dependent preferences. Recent theoretical advances have put more discipline on the formulation of reference points as initial models, for example, by formalizing reference points as an expectation of the outcomes (Kőszegi and Rabin, 2006, 2007, 2009). Although this approach can reconcile much of the existing, often conflicting evidence regarding the endowment effect (e.g., List, 2003, 2004; Plott and Zeiler, 2007), direct empirical evidence of the predictions of the theory of expectation-based reference points is at best mixed and confined to student samples (e.g., Abeler et al., 2011; Baillon, Bleichrodt and Spinu, 2020; Ericson and Fuster, 2011; Heffetz and List, 2014; Gneezy et al., 2017; Cerulli-Harms, Goette and Sprenger, 2019; Campos-Mercade et al., 2022).[1]

This paper adds rare representative evidence from a field experiment to this debate. We focus on the relevance of expectations and loss aversion for the decision to trade an endowed item and thus shed light on the formation of reference points in the general population. We take advantage of the Socio-Economic Panel (SOEP) setup, a representative longitudinal study of German households, and implement a simple decision experiment. As part of the survey, households receive a financial reward plus a small item as an appreciation of their time at the beginning of the in-person interview. We modified the handover of this item slightly and randomly assigned one of two equally valued items to households at the start of the survey: a microfiber towel (*"Towel"*) and a notebook with a pen (*"Notebook"*). At the end of the survey, the interviewer asked respondents whether they wanted to trade their assigned item for the alternative item. Embedding the experiment in this setting has several benefits. First, it allows us to provide not only new evidence on the exchange asymmetries of physical goods in a representative setting, but also to directly test the empirical relevance of expectation-based reference points in a broad sample of the population. Second, we can investigate the validity of the trading decision taken in the exchange task. Third, the decision experiment is minimally intrusive as it takes place in the "natural" survey setting that participating households experience each year, thereby minimizing potential experimenter demand or social desirability effects.

Assuming standard preferences and given near-zero transaction costs, we should observe that half of the population wanted to trade their assigned item because the assignment was random, and thus half of the population received their less-preferred item. In contrast, if people have reference-dependent preferences with an exogenous reference point, we would expect the overall trading rate to be strictly below 50 percent. To test the role of expectations in forming reference points, we manipulated the households' expectations of the opportunity to trade the items after the interview. More precisely, we cross-randomized the information about this trading oppor-

[1] Reference-dependent preferences are influential for decision-making in many economic domains beyond the endowment effect, ranging from financial decision-making, insurance, labor supply, and effort provision to standard market transactions. Expectation-based reference points, in particular, have informed new theoretical advances, for example, in industrial organization (Heidhues and Kőszegi, 2008, 2018), macroeconomics (Pagel, 2016, 2017, 2018), mechanism design (Herweg, Müller and Weinschenk, 2010; Dreyfuss, Heffetz and Rabin, 2022; Meisner and von Wangenheim, 2022), and have been used for the design of incentives and nudges (e.g., Hossain and List, 2012; Fryer et al., 2022).

tunity among households. Upon receiving the item, half the households were informed of the trading opportunity at the end of the interview, whereas the other half did not receive this information. We tailored this manipulation to the setting we were operating in, allowing us to test an important implication of the expectation-based model by Kőszegi and Rabin (2006). Respondents who know they can exchange the object will more likely trade their endowed item relative to those who do not expect to have the opportunity to trade.

Based on about 2,800 trading decisions, we document three key results. First, we find a large exchange asymmetry in the population: when receiving the *Towel*, about 38 percent of respondents trade it for the alternative item, while the trading rate is 28 percent when they are endowed with the *Notebook*. The overall trading rate is 32 percent, substantially lower than what standard preferences would predict. Remarkably, the trading rate is unrelated to a host of socio-demographic and economic variables except for a negative income gradient. In fact, we provide suggestive evidence of a causal interpretation of this relationship. Using variation in the interview timing and income around the payday, we show that trading is higher before rather than after the payday, corroborating recent evidence that more financial constraints lead to more trading and thus less exchange asymmetries among small-scale farmers in rural Zambia (Fehr, Fink and Jack, 2022). Together, this adds further evidence in favor of the endowment effect to the ongoing discussion about its robustness (Plott and Zeiler, 2007; Isoni, Loomes and Robert, 2011; Fehr, Hakimov and Kübler, 2015; Chapman et al., 2021) and the robustness of behavioral anomalies in general (e.g., Levitt and List, 2008; Camerer, 2015; Kessler and Vesterlund, 2015; Gal and Rucker, 2018; Mrkva et al., 2020). In particular, the results suggest that exchange asymmetries vary with economic circumstances in a predictable way and generalize across populations and contexts.

Second, manipulating respondents' expectations about the opportunity to trade does not affect their trading behavior. Respondents are no more likely to exchange the items if they know in advance that they will have the opportunity to do so. The treatment effect is virtually zero and precisely estimated. This casts doubt on the role of forward-looking expectations shaping the reference point as in the standard formulation of expectation-based reference dependence (e.g., Kőszegi and Rabin, 2006). At the same time, a zero treatment effect can be consistent with an expectations-based model assuming heterogeneity in loss aversion (see Campos-Mercade et al., 2022). However, our data reveal little to no heterogeneity in loss aversion. We measure loss aversion in a risky domain and find that it correlates with many personal characteristics, as documented in the literature, such as age, gender, education, and income. On the other hand, trading is riskless and turns out to be unrelated to loss aversion. Surprisingly few papers have directly studied the relationship between the endowment effect and loss aversion (see, e.g., Dean and Ortoleva, 2019; Chapman et al., 2021; Campos-Mercade et al., 2022; Gächter, Johnson and Herrmann, 2022). The findings are mixed, with some indication that the relationship may be domain-specific, i.e., that it depends on whether trading and loss aversion are elicited in the same context.[2] While our data is not conclusive on the relationship between trading and loss aversion, the evidence from the expectation manipulation appears consistent with the notion of a quick adaptation of expectations. That is, expectations seem to form immediately after the item is received, such that most respondents expect to keep the endowed item, regardless of the announcement of the opportunity to trade.

[2]Evidence from small-scale samples suggest a relationship between the endowment effect and loss aversion across domains (Gächter, Johnson and Herrmann, 2022) and within domains (Dean and Ortoleva, 2019). This is challenged by more recent work using representative or large-scale student samples that find either no relationship or evidence of a domain-specific relationship (Chapman et al., 2021; Campos-Mercade et al., 2022).

Third, we present first evidence on the validity of the decision to trade the endowed item. Looking at three pre-specified economic domains, we find that the decision to trade predicts residential mobility and is associated with stock market participation but not with the choice of (risky) occupations. Specifically, respondents who are willing to part with their endowment in the experiment are more likely to move within Germany a year later and are more likely to participate in the stock market. Residential mobility and holding stocks involve giving up habits, amenities, or existing assets, just like giving up the endowed item. This finding has a number of implications. First, observing that trading is related to structurally similar economic decisions suggests that the same underlying process guides these decisions. To the extent that loss aversion is part of this process, it is consistent with the view that the relationship is domain-specific and the measurement domain of loss aversion matters (see also Campos-Mercade et al., 2022). Second, it suggests that the endowment effect has relevance beyond the context of the pure exchange of goods.

2 Experimental Setup

We implement our study in the Innovation Sample of the Socio-Economic Panel (SOEP-IS), which is a representative longitudinal survey of German households (see Richter and Schupp, 2015, for more detail). The SOEP-IS offers the possibility to implement tailor-made survey modules including innovative tools such as behavioral experiments.

Setup and decision task. We use a unique feature of the survey organization for our purpose. Households receive a monetary reward for their participation and, in addition, a small item as an appreciation of their time. By default, the interviewers hand this item over to the respondents before the start of the survey. We modify this standard procedure and implement the exchange paradigm introduced by Knetsch (1989) endowing respondents with one of two items that they can later trade against each other. We use two equally valued items: a microfiber towel ("Towel") and a notebook with a pen ("Notebook"), both worth about 5 euros (see Figure A1 in the Appendix). The two items are randomly assigned to households such that half of the respondents receive the *Towel* and the other half receive the *Notebook*. The randomization was implemented in the survey software at the household level. Before beginning the interview, the interviewers showed both items and handed over the item as indicated by the survey software.[3] They registered any spontaneous request of a respondent to trade the two items on the spot, but followed the strict no-trade policy at this point. At the end of the household survey, the interviewers offered respondents the opportunity to trade the endowed for the alternative item. Respondents indicated whether they want to keep the assigned item or trade it for the alternative item. Trades (if any) were completed immediately by the interviewer, keeping transaction costs near zero. Finally, the interviewers continued with the individual surveys for all household members.

Treatments. We randomize respondents – typically the head of the household or the person with the best knowledge of the household – into three conditions. In about 10 percent of the total sample, we ask respondents to choose between the two items ("Choice") at the beginning of the survey. This gives us a sense of respondents' preferences for the two items. The remaining respondents participate in the exchange experiment described above, in which half of them received the *Towel*

[3]We follow the procedures suggested by Plott and Zeiler (2007), though recent evidence from a large-scale sample suggests that implementation details have no bearing on exchange asymmetries (Fehr, Fink and Jack, 2022).

and the other half received the *Notebook*. In addition, we cross-randomized the available information about the trading opportunity. In about 45 percent of the sample, respondents receive no information about the trading opportunity ("No Trading Information"). That is, after receiving the item they continue with the interview, and interviewers surprise them with the trading opportunity at the end of the household survey. Note that there is no reason to believe that respondents expect this trading opportunity since they are used to receiving an item at the start of the survey, with no option to trade at any point during the survey. In the remaining 45 percent of the sample, respondents learn about the trading opportunity ("Trading Information"), immediately after they receive the item. Otherwise, the procedure is exactly the same as when they have not been informed of the trading opportunity. This variation allows us to present a straightforward test of two important implications of the Kőszegi and Rabin model (2006) (see Section 3). Importantly, the manipulation is easy to understand and well-suited for implementation in a survey setting with a general population sample.[4]

Loss aversion. We elicit the respondents' loss aversion with the help of two separate questions. The first question is asked before the respondents get the opportunity to trade the item and consisted of six hypothetical lotteries that involve an equal chance of a gain or a loss. The gain (45 euros) is kept constant, and losses increase from 5 to 55 euros in increments of 10 euros (e.g. Gächter, Johnson and Herrmann, 2022; Fehr and Goette, 2007; Trautmann and Vlahu, 2013). For each lottery, respondents have to indicate whether they accept or reject it. The earlier a respondent switches from accepting to rejecting a lottery, the more loss averse they are.[5] In our analysis, we define respondents as loss averse if their switch point from accepting to rejecting a lottery is below the median switch point. After the possibility to trade and as part of the individual survey, we elicit a second measure of loss aversion consisting of two short questions. Specifically, we elicit the hypothetical minimum gain (x, y) to accept a fair gamble in which a respondent has an equal chance to win x or lose 25 euros in one question and win y or lose 100 euros in the other. We calculate the loss aversion parameters $\theta_1 = x/25$ and $\theta_2 = y/100$, and take the average of the two as our second measure of loss aversion.

Covariates and Balance. We control for a set of observables such as age, gender, education, marital status, number of siblings, employment status, household income, homeownership, regional indicators, residence prior to German unification, and risk preferences. In Appendix Table A1, we present the p-values of a set of linear regressions assessing the balance of these observables across the treatment and control groups as well as the item assignment. The results of the regressions (including an F-test for the joint significance of all covariates) indicate that the observables are balanced over conditions. Nevertheless, we will show all empirical results with and without controlling for the observables.

Implementation. Our study was conducted in the 2018 wave of the SOEP-IS that was in the field from September 2018 through March 2019. We ran our study with the entire SOEP-IS panel

[4]Previous studies have implemented more involved probabilistic versions of this expectations variation in the lab that would require substantially more explanation and survey time (see, e.g., Ericson and Fuster, 2011; Heffetz and List, 2014; Cerulli-Harms, Goette and Sprenger, 2019).

[5]Note that five out of six lotteries have a non-negative expected value. Observing that respondents reject these small-stakes lotteries may reflect loss aversion rather than risk aversion because under standard EUT this would imply unrealistic levels of risk aversion in high-stakes gambles (Rabin, 2000).

which consists of five independent and nationally representative samples, totaling $N = 3,223$ households. For logistical reasons, the interviewers received all material required for the survey and the experiment at the beginning of the field start, including a sufficient number of *Towels* and *Notebooks*. Before the interviewer starts with the household survey, we screen the availability of items, and in 77 cases (out of 3,223) the interviewer only had one or no item left. In these cases, the survey software skipped our experiment, and respondents received the available item as in previous years without the possibility to exchange it (i.e., these households did not take part in our study and are not part of the analysis). Note that the randomization into treatment took place after the screening question about the available items; thus, there is no selective attrition in the treatment arms. In total, we collected responses from $N = 3,146$ respondents: 339 respondents in the "Choice" condition, 1,411 respondents in the "No Trading Information" condition, and 1,396 respondents in the "Trading Information" condition. We registered the study in the AEA RCT Registry (AEARCTR-0003992) including a pre-analysis plan (see Appendix A.4). The analysis follows this plan unless noted otherwise.

3 Theoretical Considerations

To guide the analysis and the interpretation of the results, we present a unified framework based on Kőszegi and Rabin's (2006) model of reference-dependent preferences. Consider a consumption bundle $c = (c_T, c_N)$, where subscript T stands for *Towel* and subscript N for *Notebook* and let $r = (r_T, r_N)$ be the corresponding reference bundle. Utility depends on both consumption c and reference point r, and is given by

$$U(c|r) = u(c_T) + u(c_N) + \eta \sum_{k \in \{T,N\}} \mu(u(c_k) - u(r_k))$$

where $u(c_k)$ is the intrinsic consumption utility of item $k \in \{T, N\}$ and μ is the gain-loss utility representing reference dependence weighted by $\eta > 0$. Following standard practice, we assume that gain-loss utility is piece-wise linear with $\mu(x) = x$ for $x \geq 0$ and $\mu(x) = \lambda x$ for $x < 0$, where $\lambda \geq 1$ is the loss aversion parameter.

Let us first consider the case in which the reference level is given by the status quo. A person endowed with a *Towel* has a reference point $r = (1, 0)$ and will only trade the *Towel* against the *Notebook* if $U(T|r = (1,0)) \equiv u(c_T) \leq U(N|r = (1,0)) \equiv u(c_N) + \eta(u(c_N) - \lambda u(c_T))$, or $u(c_N) \geq u(c_T)(1 + \eta\lambda)/(1 + \eta)$.[6] Because $\lambda > 1$, even someone with utility $u(c_N) \geq u(c_T)$ can choose to keep the *Towel* to avoid the loss.

Kőszegi and Rabin (2006) advanced the discussion over the appropriate reference point by modeling it as (lagged) expectations over outcomes. These expectations involve uncertainty, since several reference outcomes can materialize (i.e., the reference state can be viewed as a lottery). In our setting, a respondent thus compares the outcome of their decision to every possible reference outcome. For example, if a respondent comes to believe that they will end up with their endowed *Towel* for certain because they were never given the option to trade in previous years, the reference point is $r = (1, 0)$ as with an exogenous reference point. To generalize this reasoning and endogenize expectations, assume that the trading opportunity obtains with some probability p. A respondent who is endowed with a *Towel* then faces a choice set

[6] Using a similar reasoning, a person endowed with a *Notebook* has a reference point $r = (0, 1)$ and will keep the *Notebook* if $u(c_N) \geq u(c_T)(1 + \eta)/(1 + \eta\lambda)$.

$\{R_T \equiv (r = (1,0), p = 1), R_N \equiv (r = (0,1), p; r = (1,0), 1-p)\}$. If they expect to keep the *Towel*, they will end up with it for certain and the reference lottery is R_T. However, if they expect to trade, they part with the *Towel* with probability p and keep it with probability $(1-p)$ and the reference lottery is R_N. Now, suppose that the respondent plans to trade the *Towel* for the *Notebook* if they get the opportunity to do so. To follow this plan, the decision has to be consistent with their expectations. That is, a respondent trades the endowed item if and only if $U(N|R_N) \geq U(T|R_N)$, or $u(c_N) + (1-p)\eta(u(c_N) - \lambda u(c_T)) \geq u(c_T) + p\eta(u(c_T) - \lambda u(c_N))$, which can be rewritten as

$$u(c_N) \geq \frac{1 + \eta(p + (1-p)\lambda)}{1 + \eta(1 - p + p\lambda)} u(c_T). \tag{1}$$

Note that the expression on the right-hand side is decreasing in p, thus the likelihood of trading increases in p.

Our setting implements the two extremes of p, where the respondent either learns that there is a trading opportunity at the end ($p = 1$) or the respondent learns nothing ($p = 0$). For $p = 0$, equation (1) reduces to $u(c_N) \geq u(c_T)(1 + \eta\lambda)/(1 + \eta)$, which is the same as when the reference point is determined by the status quo outlined above. On the other hand, when $p = 1$ there is no scope for forward-looking expectations; ex ante the expected gain-loss utility is zero. To see this, note that the expected utility of planning to trade the *Towel* for the *Notebook*, given by $pu(c_N) + p(1-p)\eta(u(c_N) - \lambda u(c_T)) + (1-p)u(c_T) + (1-p)p\eta(u(c_T) - \lambda u(c_N))$, has to be greater than the expected utility of planning to not trade the *Towel*, which is simply $u(c_T)$.[7] Rearranging this inequality gives

$$u(c_N) - u(c_T) + (1-p)\eta(1-\lambda)(u(c_N) + u(c_T)) \geq 0 \tag{2}$$

It is easy to see that with $p = 1$ gain-loss utility plays no role and a respondent will choose the *Notebook* whenever $u(c_N) \geq u(c_T)$.

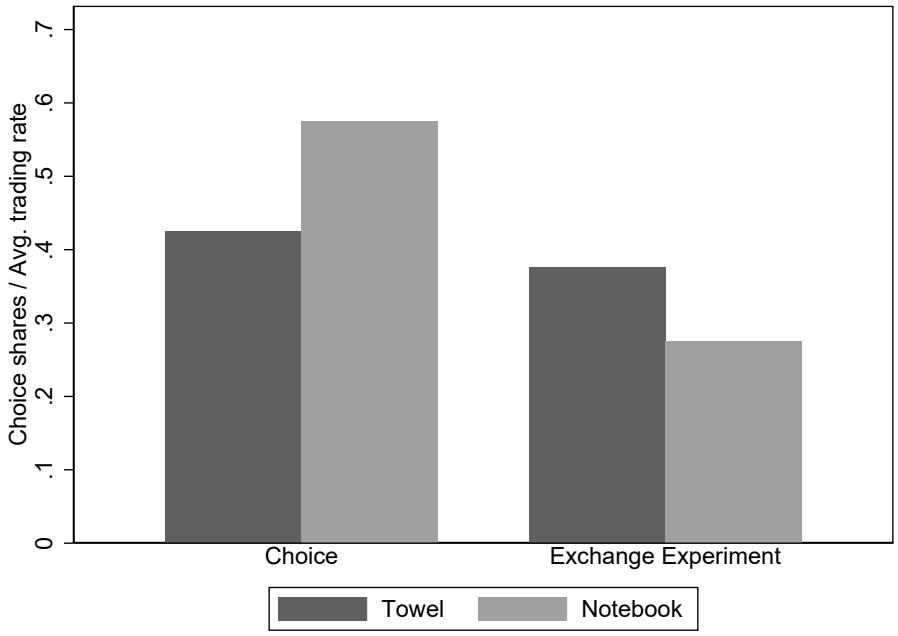
To summarize, we expect trading rates below the normative benchmark if respondents are not informed about the trading opportunity, but substantially higher trading rates when they are informed that trade is possible.

4 Results

Descriptives. We start by studying the preferences for the two items and the decision to trade in the different conditions. Figure 1 gives an overview of preferences in the "Choice" condition and the trading decision in the pooled sample. We observe a slight imbalance of preferences for the two items. When respondents can freely choose among them, about 58 percent choose the *Notebook* over the *Towel*. Second and more importantly, we observe a clear tendency of respondents to keep their endowed item. Respondents endowed with a *Towel* trade in 38 percent of cases and respondents endowed with a *Notebook* in 28 percent of cases. Together, this presents strong evidence of an exchange asymmetry: the overall trading rate is about 32 percent, which is significantly below the predicted trading rate of 50 percent (t-test, $p < 0.001$). Table 1 shows the raw data for

[7] We impose here a preferred personal equilibrium (PPE) as a refinement of personal equilibria (Kőszegi and Rabin, 2006). A personal equilibrium (PE) formalizes the idea that the reference point is determined by one's planned choice, which then determines a respondent's optimal choice. Equation (1) characterizes the condition for all possible consistent plans of a respondent in our case, which supports keeping both the *Towel* and *Notebook* as a personal equilibrium. A PPE then picks the plan with the highest ex ante utility. Formally, a choice is a PPE if it is a PE and $U(N|R_N) \geq U(T|R_T)$ for any PE.

Figure 1: Preferences over items and pooled trading rates in exchange experiment



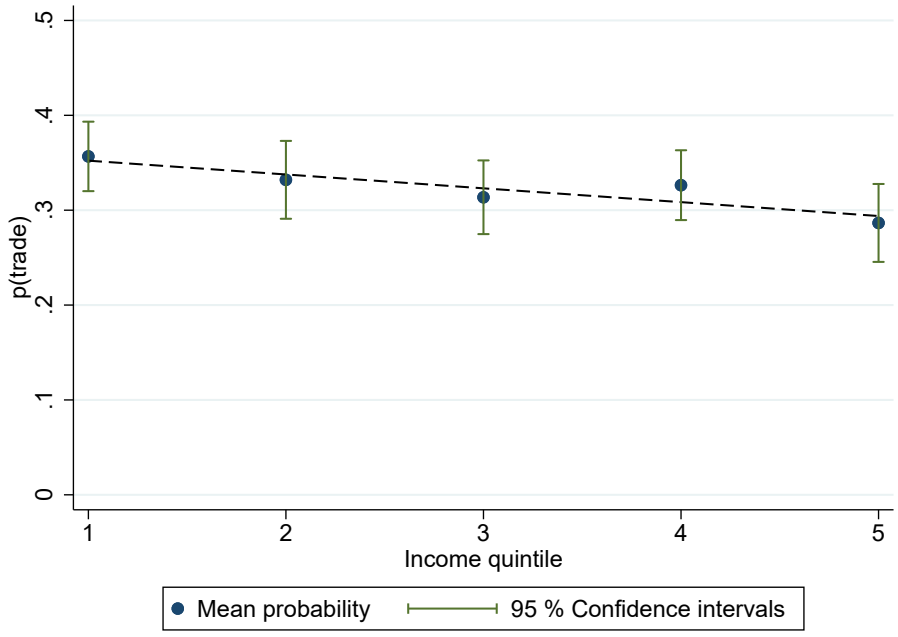
Notes: Share of respondents choosing *Towel* and *Notebook* ("Choice") and avg. trading rate when endowed with *Towel* and *Notebook* in both treatments ("Exchange Experiment").

Table 1: Descriptive statistics by treatment

	Choice	No Trading Information			Trading Information	
			End item		*End item*	
		Start item	*Towel*	*Notebook*	*Towel*	*Notebook*
Towel	144 (0.42)	*Towel*	425 (0.62)	259 (0.38)	432 (0.63)	257 (0.37)
Notebook	195 (0.58)	*Notebook*	201 (0.28)	526 (0.72)	194 (0.27)	513 (0.73)
Test	Fisher's exact test:		$p = 0.000$		$p = 0.000$	
Trading Probability			0.327 ($p < 0.001$)		0.324 ($p < 0.001$)	
N	339		1,411		1,396	

Notes: The table displays the choices in the different treatments. Shares are in parentheses. Fisher's exact test for the hypothesis of no exchange asymmetries within treatment. Trading probability is the overall trading rate in a treatment and the corresponding p-values are derived from a t-test of H_0: trading rate equals 0.5 vs. H_A: trading rate is below 0.5.

Figure 2: Probability of trading by household income



Notes: Trading probability by quintile of household income. 95% confidence intervals based on standard errors.

preferences and trading in both treatments separately along with (non-)parametric tests. There is a significant exchange asymmetry in both treatments ("No Trading Information" and "Trading Information"): respondents were more likely to end up with their endowed item than with the alternative item (Fisher's exact test, $p = 0.000$). Consequently, the trading rates are significantly below 50 percent in both treatments (t-test, $p < 0.001$).

Correlates of the trading decision. Before turning to the estimation of the treatment effects, we explore the role of socio-economic and demographic characteristics for the trading decision (see Table A2 for details).[8] Two findings are noteworthy. First, while the decision to trade shows no association with almost all the observables, it is negatively related to household income. We plot the trading probability as a function of household income quintiles in Figure 2. The negative gradient indicates that richer households have a lower propensity to trade and thus display a larger exchange asymmetry. Going from the first to the fifth quintile is associated with a seven percentage-point lower trading rate. Although the confidence intervals are not small, this difference is significant ($p < 0.02$). This is in line with causal evidence on the trading behavior of small-scale farmers in Zambia who trade less after an exogenous liquidity injection, thus resulting in a larger exchange asymmetry relative to financially constrained farmers (Fehr, Fink and Jack, 2022). In Table A4, we provide suggestive evidence on a causal link from income to trading behavior using variation in the timing of the interview and income around payday (see, for example,

[8] In the pre-analysis plan, we proposed exploring the relationship between trading behavior and socio-economic and demographic characteristics in one subsample to formulate and pre-specify hypotheses to test in the remaining subsamples. However, in the absence of correlations, we deviate from this plan here. Instead, we explore the relationship between trading and income using payday and interview time variation, which we did not specify in advance.

Akesaka et al., 2021, who adopt a similar variation to investigate the impact of income fluctuations on risk preferences). In the two weeks before payday, when financial resources are more severely depleted, the trading probability is about 9 – 10 percentage points higher than in the week after payday, conditional on day-in-the-week, week, and month fixed effects (see Appendix A.1 for details on the estimation strategy). This is consistent with the notion that a lack of resources is associated with reduced exchange asymmetries, as in Fehr, Fink and Jack (2022).

Second, we find no evidence of a relationship between trading behavior and various measures of loss aversion, with a correlation coefficient of $\rho = 0.01$ (see Table A5). However, two observations regarding loss aversion stand out. Overall, respondents are highly loss averse in our sample. The median respondent only accepts the two lotteries with the highest expected value, and around 94 percent of respondents reject at least one lottery with a non-negative expected value (implying a loss aversion parameter of $\lambda \geq 1$). At the same time, our measure of loss aversion relates to individual characteristics as documented in the literature (see Table A2), suggesting that our loss aversion measure is valid. For example, we see that women, older people, and people with more siblings are more loss averse, while education, income, and risk attitudes are negatively related to loss aversion (see, e.g., Tanaka, Camerer and Nguyen, 2010; Wang, Rieger and Hens, 2017; Mrkva et al., 2020; Gächter, Johnson and Herrmann, 2022).

While the lack of a relationship between trading behavior and loss aversion seems puzzling at first sight, it is in line with recent evidence from a representative sample of the US population (Chapman et al., 2021). There are at least two possible explanations for this finding. First, it may result from an insufficient variation in loss aversion in our sample, as almost all respondents reject at least one lottery with a non-negative expected value. Second, the relationship may depend on the context. Recent work suggests, for example, that it is crucial to elicit both behaviors in the same domain (see, e.g., Campos-Mercade et al., 2022) while our evidence comes from different domains. In our context, trading is riskless, and loss aversion is based on risky lottery choices. Supporting the domain-specific explanation, we provide evidence below that economic decisions that are structurally similar to the trading decision are associated with trading behavior.

Treatment effects. Next, we compare trading behavior across the two information treatments. Table 1 indicates that trading behavior is strikingly similar across the two information conditions. In Table 2, we present additional statistical support for this observation, taking advantage of our data-rich setting. In particular, we estimate the following model:

$$Trade_i = \alpha + \beta \times TI_i + \gamma \times \mathbf{X_i'} + \epsilon_i, \tag{3}$$

where $Trade_i$ is an indicator for trading the endowed item, TI_i is an indicator of the condition under which respondents are informed about the trading opportunity (*Trading Information*), and X_i' is a vector of individual demographic and socio-economic characteristics.

Column 1 in Table 2 shows that there is virtually no treatment effect. The point estimate is zero, statistically insignificant, and precisely estimated (90% CI of $[-0.032, 0.026]$). Including a set of individual controls does not greatly change the coefficient estimate (90% CI of $[-0.028, 0.030]$). The result is also robust to including the respondents' loss aversion and cognitive ability (columns 3 – 5). In particular, we control for loss aversion by including an indicator for respondents with a below-median switch point in the lottery task where a lower switch point indicates more loss aversion. This leaves the coefficient estimate unaffected (column 3). To control for cognitive ability, we rely on two short tests that measure crystallized and fluid intelligence, available in three of

Table 2: Probability of Trading

	Trade=1				
	(1)	(2)	(3)	(4)	(5)
Trading Information	-0.003	0.001	0.004	-0.016	-0.013
	(0.018)	(0.018)	(0.019)	(0.024)	(0.025)
Covariates	No	Yes	Yes	Yes	Yes
Loss aversion	No	No	Yes	No	Yes
Cognitive Ability	No	No	No	Yes	Yes
Region FE	No	Yes	Yes	Yes	Yes
Observations	2,806	2,806	2,614	1,600	1,495
R^2	0.00	0.01	0.01	0.01	0.02

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

OLS regressions with standard errors in parentheses. The dependent variable is "Trade=1" indicating that a respondent traded his endowed item (*Towel* or *Notebook*) for the alternative item (*Notebook* or *Towel*). "Trading Information" is a treatment indicator for respondents who were randomly informed about the trading opportunity at the end. Covariates include age, no. of siblings, household income, and general risk attitudes (higher values indicate higher willingness to take risks) as well as indicators for gender, education, marital status, employment status, homeownership, region, and residence prior to German unification. Cognitive ability is the average of the standardized test score in a short task measuring crystallized intelligence (Multiple-Choice Vocabulary Intelligence Test) and another short task measuring fluid intelligence (symbol-digit-correspondence task). Loss aversion indicates a below-median switch point in the lottery task. The lottery task presents six lotteries that hypothetically pay 45 euros and a loss (varying from 5 to 55 euros) with equal probability. The switch point indicates when respondents switch from accepting to rejecting a lottery, with a lower switch point implying more loss aversion.

our five subsamples.[9] This reduces the number of observations, but again leaves our estimate unaffected (column 4). Moreover, cognitive ability is not related to the trading decision. Column 5 includes all controls at once, but again this has no measurable effect on the treatment estimate.[10]

An essential assumption in the analysis above is that all respondents treat gains and losses similarly around the reference point. However, this is at odds with empirical evidence suggesting, to some extent, substantial individual variation in loss aversion (e.g., Brown et al., 2022; Chapman et al., 2021; Campos-Mercade et al., 2022). Considering this heterogeneity, expectation-based models make distinct predictions depending on loss aversion. Specifically, more loss-averse people should be reluctant to trade if the status quo determines their reference point. But they should more likely trade when the reference point carries greater expectations of trading and vice versa for gain-seeking people (see Campos-Mercade et al., 2022, for evidence). However, our data do not support these predictions. In Table A6, we show that different degrees of loss aversion do not explain the zero average treatment effect we observe. Our measure of loss aversion is neither

[9] Crystallized intelligence is measured through the Multiple-Choice Vocabulary Intelligence Test, and fluid intelligence is measured through a symbol-digit-correspondence task. Both tests are adapted to the use in surveys. We construct our measure of cognitive ability by taking the average of the two standardized test scores.

[10] In Appendix Table A7, we replicate these results using an alternative specification estimating the trading probability directly.

Table 3: Association of Trading Behavior with Other Economic Decisions

	Moved=1		Shareholder=1		Self-employed=1	
	(1)	(2)	(3)	(4)	(5)	(6)
Traded endowed item=1	0.017**	0.015**	0.064***	0.067***	-0.007	-0.006
	(0.008)	(0.008)	(0.024)	(0.024)	(0.010)	(0.009)
Covariates	No	Yes	No	Yes	No	Yes
Region FE	No	Yes	No	Yes	No	Yes
Observations	2,806	2,806	772	772	2,806	2,806
R^2	0.00	0.04	0.01	0.09	0.00	0.09

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

OLS regressions with standard errors in parentheses. "Moved=1" indicates whether a household moved to a new address (within Germany) a year later, "Shareholder=1" denotes stock market participation, "Self-employed=1" denotes self-employment and serves as proxy for occupational risk-taking. "Traded endowed item=1" indicates that a respondent traded his endowed item (*Towel* or *Notebook*) for the alternative item (*Notebook* or *Towel*). Covariates include age, no. of siblings, household income, and general risk attitudes (higher values indicate higher willingness to take risks) as well as indicators for gender, education, marital status, employment status, homeownership, region, and residence prior to German unification.

related to trading when people are not informed about the trading opportunity, and the reference point is the status quo, nor when they are informed and can expect to trade.

While these findings suggest that expectation-based reference points do not play a decisive role in our trade setting, there is suggestive evidence that some respondents plan to exchange the endowed item and follow through with this plan. Immediately after assigning the item, the interviewer registered whether a respondent spontaneously expressed the wish to trade the endowed against the alternative item. According to the interviewers' perceptions, this was the case in about 23 percent of cases in each treatment. If respondents carry out their plans, we should observe trading if they get the opportunity (irrespective of the treatment). In fact, we observe that 73 percent of these respondents trade away their endowed item.

Overall, the findings are consistent with a quick adaption of reference points: the majority of respondents expect to leave the survey with the endowed item and subsequently keep it even if given the chance to trade, whereas a minority of respondents quickly opt to trade the endowed item for the alternative item, form the corresponding expectations, and then implement this plan if given the opportunity.

Trading decision and other economic choices. We now take a different perspective and ask whether trading behavior predicts or correlates with other important economic decisions. Specifically, we explore how the decision to trade the endowed item relates to three pre-specified decisions: residential mobility, stock market participation, and having a risky occupation. We hypothesized that these economic decisions relate to the trading decision since they entail giving up existing assets. For example, people become accustomed to the amenities of their neighborhood (shops, public transport, etc.). Moving to a new neighborhood implies that people have to give up these amenities and acquired location-specific habits, which may be related to a higher willingness

to part with existing assets, i.e. trading behavior.[11] A similar reasoning applies to shareholders who may find it easier to part with their endowed item because they also buy and sell assets regularly.[12] Finally, trading behavior may be related to the choice of occupation: entrepreneurial activities and self-employment are typically riskier, involve more investment decisions, and are typically performed by individuals who are less loss averse than others (e.g., Caliendo, Fossen and Kritikos, 2014; Koudstaal, Sloof and van Praag, 2016).

To address the relationship between trading behavior and economic decisions, we use the rich data of the SOEP-IS. Table 3 presents the results. The propensity to trade predicts residential mobility (columns 1 – 2). That is, respondents who traded the endowed item were more likely to move in the year after the experiment.[13] Second, trading is strongly associated with stock market participation. Respondents who own stocks are six to seven percentage points more likely to trade their endowed item (columns 3 – 4).[14] Third, we find no relationship between trading and self-employment, which serves as a proxy for occupational risk-taking. The point estimates are virtually zero and precisely estimated (columns 5 – 6).

Overall, these findings suggest that the trading decision is associated with economic decisions that entail giving up something, i.e., decisions that are structurally similar, but not with decisions that involve more or less risk, such as being self-employed.

5 Conclusion

We provide field-experimental evidence of the endowment effect in a representative population sample. Using the infrastructure of a well-established longitudinal study, the German Socio-Economic Panel (SOEP), we run an exchange experiment with more than 3,100 respondents. Overall, we observe a trading rate of 32 percent, substantially below the normative benchmark of 50 percent.

Beyond adding rare representative evidence to the voluminous literature on the endowment effect, the present study provides two key takeaways. First, we find no support for expectation-based reference points in the aggregate: when respondents are informed about the trading opportunity upon assignment of the item, trading rates are the same as when respondents are left in the dark about this opportunity. At the individual level, however, there are indications that respondents who form the plan to exchange the items follow through with their plan. Second, we document that trading behavior is related to important economic decisions, such as residential mobility and stock-market participation. This suggests that the decision to trade, which constitutes a simple, but fundamental economic decision, and more complex, but structurally similar

[11] For example, Jaeger et al. (2010) document that individuals who are willing to take more risks are also more likely to migrate within Germany (see also Dohmen et al., 2005). Clark and Lisowski (2017) present similar results for Australia. They argue that housing tenure, duration at dwelling, and neighborhood SES (which they label as a proxy for the endowment effect) provide additional explanatory power for the decision to move.

[12] While experienced traders may find it easier to part with their existing assets (e.g., List, 2003, 2004), recent evidence from IPOs in India documents that even experienced investors can be reluctant to trade their randomly allocated IPO shares (Anagol, Balasubramaniam and Ramadorai, 2018).

[13] This relationship is further corroborated by a negative relationship between trading and homeownership. Homeownership is positively correlated with household income and negatively with moving. The estimated coefficient is -0.038 (s.e. 0.018).

[14] Because information on shareholdings is not routinely asked in the SOEP-IS, we can only rely on information about shareholdings from one subsample. While this reduces sample size substantially, we note that each subsample is representative of the German population.

economic decisions are governed by the same underlying process. While our study is inconclusive about this process, it raises new and important questions about the behavioral forces that govern trading behavior.

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Appendix – For Online Publication Only

The Endowment Effect in the General Population

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A.1 Payday variation

Figure 2 shows a negative household income gradient indicating that richer households have a lower propensity to trade and thus display a larger exchange asymmetry. While this is correlational evidence (i.e., income is correlated with other factors that may affect trading), we provide more plausible exogenous variation below, suggesting a causal link from income to trading behavior. To do so, we use income variation around payday and variation in the timing of the interviews. In Germany, wages and salaries are paid at the end of the month, while rent, bills, and most other recurring payments are due in the first half of a month. This likely creates income cycles: increasing financial constraints in the two weeks before payday and less financial constraints in the two weeks after payday. Previous work has shown that financial scarcity consistently leads to higher trading rates and thus smaller exchange asymmetries (Fehr, Fink and Jack, 2022).

We restrict our focus to the working population in the sample for which we can precisely determine the payday, as bank transfers take place on the last bank working day in a given month. The interviews are randomly dispersed over the month, and thus respondents who are interviewed before payday likely face more financial constraints than respondents interviewed proximately after payday. We estimate the following specification.

$$Trade_i = \alpha + \beta_1 \mathbb{1}(\text{Before payday}) + \gamma \mathbf{X_i'} + D_t + W_t + M_t + \epsilon_i, \tag{4}$$

where $Trade_i$ is an indicator for trading the endowed item, $\mathbb{1}(\text{Before payday})$ is an indicator for interviews in the two weeks before payday, and the two weeks after payday is the baseline. We focus on the two weeks before and after payday to increase power. X_i' is a vector of individual demographic and socio-economic characteristics, and D_t, W_t, and M_t are day-in-the-week, week, and month fixed effects.

A key assumption of the identification is that interview dates are random. In Table A3, we show that our standard set of observables ($\mathbf{X_i'}$) is uncorrelated to the timing of the payday. Specifically, Table A3 shows coefficient estimates for the timing of the interview (relative to the payday) along with the F-statistic and p-values for a test of joint significance of coefficients. In all regressions the test statistics are small, except for females who appear to be overrepresented in the interviews in the second week after the payday. In our main specification, we control for all observables (see above).

Table A4 presents the results on payday variation. In the two weeks before payday, when financial resources are likely severely depleted, the trading probability is about 9 – 10 percentage points higher than in the two weeks after payday, conditional on day-in-the-week, week, and month fixed effects. This is consistent with the notion that scarce financial resources move decisions closer to the normative benchmark because scarcity makes decisions more consequential (Fehr, Fink and Jack, 2022).

A.2 Additional Figures

Figure A1: Items used in the experiment

A.3 Additional Tables

Table A1: Balance: Treatment Status and Covariates

	Choice = 1	No Trade Info = 1		Trade Info=1	
	(1)	(2) Full	(3) Notebook=1	(4) Full	(5) Notebook=1
Age	0.339	0.356	0.477	0.128	0.758
Female=1	0.118	0.169	0.615	0.688	0.478
Married=1	0.939	0.436	0.060	0.407	0.597
No. of siblings	0.153	0.594	0.530	0.154	0.331
Lower secondary=1	0.668	0.597	0.156	0.426	0.751
Upper secondary=1	0.331	0.339	0.191	0.118	0.570
College=1	0.161	0.145	0.699	0.020	0.393
Retired=1	0.155	0.362	0.774	0.072	0.166
Self employed=1	0.636	0.284	0.999	0.437	0.605
Unemployed=1	0.877	0.247	0.077	0.209	0.613
Log(HH income)	0.452	0.917	0.504	0.715	0.471
Homeownership=1	0.888	0.642	0.762	0.706	0.490
General risk attitudes	0.103	0.022	0.170	0.203	0.732
East Germany before 1989=1	0.983	0.184	0.812	0.188	0.354
West Germany=1	0.176	0.210	0.477	0.036	0.720
North Germany=1	0.211	0.864	0.298	0.341	0.835
East Germany=1	0.935	0.157	0.285	0.171	0.516
South Germany=1	0.721	0.916	0.440	0.907	0.427
F-statistic	0.89	0.98	1.13	1.22	0.95
Observations	3,146	3,146	1,396	3,146	1,410

Notes: Columns 1–5 report p-values from a series of regressions of the form $y = \alpha + \beta Covariate_i + \epsilon_i$, where $Covariate_i$ is the variable listed in the row and y is an indicator for the three conditions in columns 1, 2, and 4 ("Choice," "No Trading Information," and "Trading Information") and an indicator for the random assignment of items in the two experimental conditions ("No Trading Information," and "Trading Information") in columns 3 and 5. All covariates are coded as binary variables except age, no. of siblings, household income, and risk attitudes. "West Germany" is a regional indicator for Hesse, North Rhine-Westphalia, Rhineland-Palatinate, and Saarland; "North Germany" is an indicator for Bremen, Hamburg, Lower Saxony, Schleswig-Holstein, and Mecklenburg-Western Pomeran; "East Germany" is an indicator for Berlin, Brandenburg, Saxony, Saxony-Anhalt, and Thuringia; "South Germany" is an indicator for Bavaria and Baden-Wuerttemberg. "Log(HH income) is the logarithm of monthly net household income. "General risk attitudes" are measured on a scale from 0 to 10 with higher values indicating more risk tolerance. $F - statistic$ is the test statistic from an F-test for joint significance of all covariates.

Table A2: Correlates of Trading and Loss Aversion

	Trade = 1		Loss averse = 1	
	(1)	(2)	(3)	(4)
Age	-0.001	-0.000	0.004***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Female=1	0.014	0.006	0.085***	0.049***
	(0.018)	(0.018)	(0.018)	(0.018)
Married=1	-0.021	0.006	-0.036*	0.012
	(0.018)	(0.021)	(0.018)	(0.021)
No. of siblings	-0.003	-0.004	0.022***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)
Lower secondary=1	0.025		0.039	
	(0.028)		(0.029)	
Upper secondary=1	-0.015	-0.022	0.067***	0.035
	(0.018)	(0.029)	(0.019)	(0.030)
College=1	0.005	-0.002	-0.104***	0.001
	(0.021)	(0.035)	(0.021)	(0.035)
Retired=1	-0.009	-0.011	0.141***	0.040
	(0.018)	(0.029)	(0.019)	(0.029)
Self employed=1	-0.027	-0.023	-0.064*	0.042
	(0.037)	(0.039)	(0.039)	(0.039)
Unemployed=1	-0.011	-0.057	0.049	0.015
	(0.044)	(0.047)	(0.046)	(0.047)
Log(HH income)	-0.032**	-0.037*	-0.131***	-0.100***
	(0.015)	(0.020)	(0.015)	(0.019)
Homeownership=1	-0.038**	-0.022	-0.012	-0.002
	(0.018)	(0.020)	(0.019)	(0.020)
East Germany before 1989=1	0.016	-0.003	-0.004	-0.011
	(0.022)	(0.031)	(0.023)	(0.030)
West Germany=1	0.005	-0.016	0.031	0.040
	(0.019)	(0.034)	(0.020)	(0.034)
North Germany=1	-0.010	-0.028	-0.019	-0.001
	(0.022)	(0.033)	(0.023)	(0.033)
East Germany=1	0.027		-0.021	
	(0.022)		(0.024)	
South Germany=1	-0.020	-0.030	-0.003	0.034
	(0.020)	(0.035)	(0.021)	(0.035)
General risk attitudes	-0.001	-0.000	-0.048***	-0.042***
	(0.004)	(0.004)	(0.004)	(0.004)
Observations		2,806		2,921

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

OLS regressions with standard errors in parentheses. The dependent variable in columns (1 – 2) is "Trade=1" indicating that a respondent traded his endowed item for the alternative item and in columns (3 – 4) "Loss averse=1" indicating a below-median switch point in the lottery task. The lottery task presents six lotteries that hypothetically pay 45 euros or entail a loss (varying from 5 to 55 euros) with equal probability. The switch point indicates when a respondent switches from accepting to rejecting a lottery, i.e., a lower switch point means more loss aversion. Uneven-numbered columns display coefficients from separate regressions for each covariate listed in the rows, while even-numbered columns report multivariate regressions including all listed covariates at once. All covariates are coded as binary variables except age, no. of siblings, household income, and risk attitudes. "West Germany" is a regional indicator of Hesse, North Rhine-Westphalia, Rhineland-Palatinate, and Saarland; "North Germany" is an indicator of Bremen, Hamburg, Lower Saxony, Schleswig-Holstein, and Mecklenburg-Western Pomeran; "East Germany" is an indicator of Berlin, Brandenburg, Saxony, Saxony-Anhalt, and Thuringia; "South Germany" is an indicator of Bavaria and Baden-Wuerttemberg. "Log(HH income) is the logarithm of monthly net household income. "General risk attitudes" are measured on a scale from 0 to 10 with higher values indicating more risk tolerance.

Table A3: Test for randomness of interview dates

	(1) Age	(2) Female	(3) Education	(4) Income	(5) Married	(6) Retired	(7) Unemployed	(8) Siblings	(9) Risk	(10) Home	(11) E. Germany
Week 1&2 before payday	-0.557	0.042	-0.094	-0.035	0.007	0.015	0.006	0.079	-0.079	-0.038	0.023
	(0.864)	(0.030)	(0.084)	(0.034)	(0.030)	(0.020)	(0.007)	(0.110)	(0.133)	(0.030)	(0.024)
Week 2 after payday	0.553	0.094**	-0.072	0.045	0.031	0.027	0.001	-0.105	-0.225	0.026	-0.001
	(1.068)	(0.037)	(0.104)	(0.042)	(0.037)	(0.024)	(0.009)	(0.136)	(0.164)	(0.037)	(0.030)
F-statistic	0.70	3.25	0.63	2.34	0.37	0.64	0.35	1.13	0.94	2.06	0.64
p-value	0.50	0.04	0.53	0.10	0.69	0.53	0.70	0.32	0.39	0.13	0.53
Observations	1,524	1,525	1,519	1,525	1,525	1,525	1,525	1,525	1,525	1,525	1,525

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

OLS regressions with standard errors in parentheses. F-statistic and p-values from joint test of significance of the two coefficients. Sample restricted to working respondents. "Week 1&2 before payday" is an indicator of interviews conducted in the two weeks before payday and "Week 2 after payday" is an indicator of interviews in the second week after payday. The omitted category is "Week 1 after payday." "Education" indicates the highest degree/diploma attained on a scale from 1–7, "Income" is the log of the monthly net HH income, "Siblings" is the number of siblings, "Risk" is a qualitative risk measure on a scale from 0 (very willing to take risks) to 10 (not at all willing to take risks), "Home" is an indicator of homeownership, and "E. Germany" is an indicator of having lived in East Germany before 1989.

Table A4: Trading and Payday Variation

	Trade = 1			
	(1)	(2)	(3)	(4)
Week 1&2 before payday	0.048**	0.095**	0.098**	0.098**
	(0.024)	(0.047)	(0.047)	(0.048)
Day FE	No	Yes	Yes	Yes
Week FE	No	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes
Covariates	No	No	Yes	Yes
Region FE	No	No	No	Yes
Observations	1,525	1,525	1,518	1,518
R2	0.00	0.02	0.02	0.03

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

OLS regressions with standard errors in parentheses. The dependent variable is "Trade=1" indicating that a respondent traded his endowed item (*Towel* or *Notebook*) for the alternative item (*Notebook* or *Towel*). Variation comes from income fluctuations around payday – which is the last bank working day in a given month in Germany – random interview dates. "Week 1&2 before payday" is an indicator of interviews conducted in the two weeks before payday. The omitted category is "Week 1&2 after payday." We include day, week, and month fixed effects. Covariates include age, gender, marital status, number of siblings, household income, education, general risk attitudes, homeownership, and residence prior to German unification. Sample is restricted to working respondents.

Table A5: Correlation: Trading and Loss Aversion

	Trade = 1				
	(1)	(2)	(3)	(4)	(5)
Loss averse: below median=1	-0.010				
	(0.018)				
Loss averse: switch point		0.001			
		(0.006)			
Loss averse: reject pos. EV gamble			0.013		
			(0.039)		
Loss averse: implied lambda				-0.003	
				(0.003)	
Loss averse: avg. gain-loss ratio					-0.000
					(0.000)
Covariates	No	No	No	No	No
Observations	2,615	2,615	2,615	2,615	2,244

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

OLS regressions with standard errors in parentheses. The dependent variable is "Trade=1" indicating that a respondent traded his endowed item (*Towel* or *Notebook*) for the alternative item (*Notebook* or *Towel*). Loss aversion is measured through a lottery task that presents six hypothetical lotteries. Each lottery gives an equal chance to win 45 euros or a loss varying from 5 to 55 euros. Rejecting a lottery with a non-negative expected value indicates loss aversion. "Loss averse: below median=1" indicates a below-median switch point in the lottery task (median switch point is 3, i.e., accepting the two lotteries with the highest expected value and rejecting the rest). "Loss averse: switch point" is the switch point from accepting to rejecting a lottery with a lower switch point indicating more loss aversion. "Loss averse: reject pos. EV gamble" indicates a respondent who rejects at least one lottery with a non-negative expected value. "Loss averse: implied lambda" is the implied loss aversion parameter (lambda) of the switch point, calculated as $w^+(0.5)v(gain) = w^-(0.5)\lambda v(loss)$ (assuming that $w^+(0.5) = w^-(0.5)$ and linearity of $v(x) = x$, we have $\lambda = gain/loss$). "Loss averse: avg. gain-loss ratio" is the average gain-loss ratio calculated from the stated minimum amount to accept a lottery that gives an equal chance of winning this amount and losing 25 or 100 euros.

Table A6: Probability of Trading – Heterogeneous Effects

	Trade=1		
	(1)	(2)	(3)
Trading Information	-0.043	-0.037	-0.028
	(0.076)	(0.076)	(0.099)
Loss aversion	-0.010	-0.013	-0.005
	(0.056)	(0.056)	(0.070)
Loss aversion x Trading Information	0.046	0.040	0.014
	(0.079)	(0.079)	(0.102)
Covariates	No	Yes	Yes
Cognitive Ability	No	No	Yes
Region FE	No	Yes	Yes
Observations	2,615	2,614	1,495
R^2	0.00	0.01	0.00

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

OLS regressions with standard errors in parentheses. The dependent variable is "Trade=1" indicating that a respondent traded his endowed item (*Towel* or *Notebook*) for the alternative item (*Notebook* or *Towel*). "Trading Information" is a treatment indicator of respondents who were randomly informed about the trading opportunity at the end. "Loss aversion" indicates if a respondent rejected at least one lottery with a non-negative expected value. The lottery task presents six lotteries that hypothetically pay 45 euros and a loss (varying from 5 to 55 euros) with equal probability. About 94 percent of respondents rejected at least one lottery with non-negative expected value. Results are robust to using other definitions of loss aversion (see Table A5 for other definitions). Covariates include age, no. of siblings, household income, and general risk attitudes (higher values indicate higher willingness to take risks) as well as indicators of gender, education, marital status, employment status, homeownership, region, and residence prior to German unification. Cognitive ability is the average of the standardized test score in a short task measuring crystallized intelligence (Multiple-Choice Vocabulary Intelligence Test) and another short task measuring fluid intelligence (symbol-digit-correspondence task).

Table A7: Estimating Effect of Endowment

	Depart with *Towel*=1				
	(1)	(2)	(3)	(4)	(5)
Start item = *Towel*	0.345***	0.344***	0.331***	0.329***	0.313***
	(0.025)	(0.025)	(0.026)	(0.033)	(0.034)
Information about Trading	-0.002	0.003	0.007	-0.024	-0.019
	(0.025)	(0.025)	(0.026)	(0.032)	(0.034)
Start item = *Towel* x Information about Trading	0.008	0.000	-0.007	0.028	0.023
	(0.035)	(0.035)	(0.037)	(0.047)	(0.049)
Covariates	No	Yes	Yes	Yes	Yes
Loss aversion	No	No	Yes	No	Yes
Cognitive Ability	No	No	No	Yes	Yes
Region FE	No	Yes	Yes	Yes	Yes
Observations	2,806	2,806	2,614	1,600	1,495
R^2	0.12	0.14	0.13	0.15	0.14

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

OLS regressions with standard errors in parentheses. The dependent variable is "Depart with *Towel*" indicating that a respondent kept *or* traded for the *Towel*. "Start item = *Towel*" indicates that a respondents was randomly assigned a *Towel* at the beginning. "Trading Information" is a treatment indicator of respondents who were randomly informed about the trading opportunity at the end. "Start item = *Towel* x Trading Information" is an interaction of the random assignment of the *Towel* with the treatment. Covariates include age, no. of siblings, household income, and general risk attitudes (higher values indicate higher willingness to take risks) as well as indicators of gender, education, marital status, employment status, homeownership, region, and residence prior to German unification. Cognitive ability is the average of the standardized test score in a short task measuring crystallized intelligence (Multiple-Choice Vocabulary Intelligence Test) and another short task measuring fluid intelligence (symbol-digit-correspondence task). Loss aversion indicates a below-median switch point in the lottery task. The lottery task presents six lotteries that hypothetically pay 45 euros and a loss (varying from 5 to 55 euros) with equal probability. The switch point indicates when respondents switch from accepting to rejecting a lottery, with a lower switch point implying more loss aversion.

A.4 Pre-Analysis Plan

1. Introduction

The endowment effect describes the tendency of individuals to place greater value on objects they own than on the same or a similar object they do not own. This drives a wedge between the willingness to pay and willingness to accept for an object, or leads individuals to trade a randomly assigned object significantly less often than predicted by standard theory, resulting in an exchange asymmetry. This goes back to work by Richard Thaler (1980).

In this project, we investigate the prevalence of this prominent bias in the general population and test a leading theoretical explanation for exchange asymmetries. This pre-analysis plan presents the data source, the structure of the experiment, the hypotheses, and the empirical strategy.

2. Research strategy and design of experiment

We implement our study in the SOEP Innovation Sample (SOEP IS, Richter and Schupp, 2015), which offers the possibility to implement short survey modules including innovative tools such as behavioral experiments. The SOEP IS is a representative longitudinal survey of German households and currently consists of five independent subsamples (IE/I1, I2, I3, I4, I5). Participating households receive monetary incentives for completing the surveys and, in addition, receive a small item in the beginning of a survey as appreciation for their time (household gift).

We use this unique feature of the survey for our experiment and implemented a modified version of the exchange paradigm introduced by Knetsch (1989). That is, we randomly endow households with one of two equally valued items. The items were a microfiber towel and a notebook with a pen. Both items are worth about 5 euro. At the end of the household survey, interviewers offered respondents the opportunity to trade the endowed item for the alternative item. After completing trades (if any), the interviewers continue with personal questionnaires for all household members.

We randomize respondents – typically the head of household or person with most knowledge about household topics – into three treatments. In about 10 percent of the total sample, respondents simply have a choice between the two items, and there is be no opportunity to trade the item at the end ("choice condition"). In about 45% of the sample, we implement the "baseline condition". That is, half of the respondents receive the towel as a present ("group A" and "item A") and the other half of the respondents receive the notebook ("group B" and "item B"). More specifically, the survey software randomizes the two items in the beginning of the interview, interviewers show both items and assign the item as indicated by the survey software. At the end of the household survey, respondents can trade their assigned item with the other item. In the remaining 45% of the sample, we follow the same procedure as in the "baseline condition", but inform respondents immediately after assigning items that they have the possibility to exchange their endowed item with the alternative item at the end of the survey ("expectations condition").

In addition, we elicit respondents' loss aversion with two separate questions before and after the trading possibility. The first question consists of six hypothetical lotteries that involve an equal chance of a gain or a loss. For each lottery, respondents have to indicate whether they would accept or reject the lottery. The number of rejected lotteries is an indicator for individuals' loss aversion. We ask this question before the trading possibility in the household survey. After the trading possibility and as part of the personal questionnaire, we use two additional questions to measure loss aversion. Specifically,

we elicit the hypothetical minimum gain X to accept a fair gamble where a respondent has an equal chance to hypothetically win X or lose 25 Euro in one question and win X or lose 100 Euro in the other.

Our study was part of the 2018 wave of the SOEP-IS, which was in the field from September 2018 through March 2019. We ran our study in all five subsamples of the SOEP IS. Based on a gross sample of 3,900 households at the start of the field work and an expected panel stability of 87 percent, the expected net sample is *N=3,393*.[1] For logistical reasons interviewers receive all required survey material at the beginning of the field start, including a sufficient number of items for the exchange experiments. Although we overstocked each interviewer with items (i.e., they had a buffer of 30 percent), it is theoretically possible that they run out of one of the two items. For this reason, we included a screening question at the start of a survey. If the interviewer has only one of the two items left, the survey procedure skips our experiment and the household receives the available item as was standard in previous years (i.e., this household does not take part in our study). Note that the randomization into treatments takes place after this screening question.

We completed the pre-analysis plan in March 2019 and we had no access to the data set before the plan was registered at the AEA RCT trial (the preliminary data are made available in April 2019).

3. Definition of Outcomes

Our main outcomes of interest are (i) the share of respondents who keep their assigned item and (ii) the trading rates in groups A and B.

4. Hypotheses

4.1. Primary outcomes

Baseline condition: According to standard theory, individuals should choose the item they prefer, such that the share of individuals choosing item A is roughly the same in the group starting with item A and in the group starting with item B. Consequently, the initial assignment of items should be irrelevant. Observing a higher share of individuals who keep their initially assigned item than the share trading for this item if initially assigned the other item is evidence for the endowment effect (exchange asymmetry).

> H0: The share of respondents trading their assigned item A for item B is equal to the share of respondents keeping their assigned item B.
>
> HA: The share of respondents trading their assigned item A for item B is lower than the share of respondents keeping their assigned item B.

or equivalently

> H0: The trading rate across both groups is equal to 50 percent.
>
> HA: The trading rate across both groups is below 50 percent.

[1] Panel stability is an indicator for field work success and is calculated as the number of interviewed households in latest wave divided by the number of interviewed households in the previous wave.

Expectations condition: In this condition we manipulate respondents' expectations about subsequent trading by informing them early on about the trading opportunity. That is, respondents are certain about the trading possibility at the end of the household survey.

From a standard theoretical point of view, this variation should not matter, and respondents should pick the item they prefer, resulting in a total trading rate of 50 percent.

> H0: The share of respondents trading their assigned item A for item B is equal to the share of respondents keeping their assigned item B.
>
> HA: The share of respondents trading their assigned item A for item B is lower than the share of respondents keeping their assigned item B.

or equivalently

> H0: The trading rate across both groups is equal to 50 percent.
>
> HA: The trading rate across both groups is below 50 percent.

Previous studies have shown that respondents tend to keep their randomly endowed item rather than trade it for another (similar) item, suggesting that preferences are reference dependent. Therefore, the information about the trading possibility should have an effect on trading from a behavioral view point. In fact, if expectations determine respondents' reference points, we should observe more trading in the expectations condition, because the information prevents the item entering a respondents' endowment as is likely to be the case in the baseline condition (Köszegi and Rabin, 2006). Thus, the baseline condition is more favorable for observing an exchange asymmetry than the expectations condition.

> H0: There is an exchange asymmetry in both conditions.
>
> HA: The exchange asymmetry in the expectation condition is smaller than in the baseline condition.

Alternatively, we can compare the trading rates.

> H0: The trading rates are below 50 percent in both conditions.
>
> HA: The trading rate is higher in the expectations condition than in the baseline condition.

4.2. Secondary outcome: loss aversion

Loss aversion is a key parameter in models of reference-dependent preferences (Köszegi and Rabin, 2006), which represent a leading explanation for exchange asymmetries. We have two individual measures for loss aversion. This allows us to investigate the relationship between the trading decision and loss aversion as well as the relationship between loss aversion and individual characteristics.

Following the split-sample strategy proposed by Anderson and Magruder (2017), we will use one of the five independent and representative subsamples of the SOEP IS (subsample IE/I1) to explore these relationships and to try different specifications to account for the fact that we do not measure exchange asymmetries at the individual level. Subsequently, we will formulate our hypotheses based on this exploration (in a supplement to this pre-analysis plan) and then test these hypotheses in the remaining four subsamples.

4.3. Exploratory analysis

We also want to explore how certain socio-economic characteristics are related to the trading decision and to our first loss aversion measure (elicited before the trading decision). In particular, we will look at the following covariates: income, gender, age, education, IQ, and number of siblings.

Finally, we are interested in whether the trading decision is related to other specific economic decisions of respondents. That is, we will explore whether trading in our experiment predicts stock market participation, migration choice, and entering more risky occupations.

5. Empirical Strategy (primary outcomes)

We will use non-parametric tests and regression analyses to test our main hypotheses (primary outcomes).

In a first step, we will tabulate the frequency of trading and of keeping the assigned items in both conditions. We then use Fisher's exact test for testing our hypotheses. In a second step, we will use regressions to control for other factors that may influence the decision to trade and to compare behavior in the two conditions. We will use to two specifications (linear probability models):

$$Keep_{itemA_i} = \alpha + \beta itemA_i + \delta Expectations_i + \vartheta(Expectations_i \times itemA_i) + \gamma \boldsymbol{X} + \varepsilon_i \quad (1)$$

where $Keep_{itemA_i}$ indicates whether respondent *i* ended up with item A, $itemA_i$ is an indicator whether respondent *i* started with item A, and $Expectations_i$ is an indicator for our expectations condition, and $\boldsymbol{X}$ is a set of standard controls (including income, gender, age, education, IQ).

$$Trade_i = \alpha + \beta Expectations_i + \gamma \boldsymbol{X} + \varepsilon_i \quad (2)$$

where $Trade_i$ equals one if respondent *i* traded his assigned item and 0 otherwise. Again $Expectations_i$ is an indicator for our expectations condition, and $\boldsymbol{X}$ is a set of standard controls (including income, gender, age, education, IQ).

6. References

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