Reconsidering the Effect of Market Experience on the "Endowment Effect"

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Reconsidering the Effect of Market Experience on the "Endowment Effect"[*]

Dirk Engelmann[†]and Guillaume Hollard[‡]

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Abstract

Simple exchange experiments have revealed that participants trade their endowment less frequently than standard demand theory would predict. List (2003a) finds that the most experienced dealers acting in a well-functioning market are not subject to this exchange asymmetry, suggesting that a significant amount of market experience is required to overcome it. In order to understand this market-experience effect, we introduce a distinction between two types of uncertainty, choice uncertainty and trade uncertainty, both of which could lead to exchange asymmetry. We conjecture that trade uncertainty is most important for exchange asymmetry. To test this conjecture, we design an experiment where the two treatments impact differently on trade uncertainty, while controlling for choice uncertainty. Supporting our conjecture, we find that "forcing" subjects to give away their endowment in a series of exchanges eliminates exchange asymmetry in a subsequent test. We discuss why markets might not provide sufficient incentives for learning to overcome exchange asymmetry.

JEL Classification: C91, D12.

Keywords: Endowment effect, exchange asymmetry, market experience.

1 Introduction

Simple exchange experiments, starting with Knetsch (1989), have shown that participants trade their endowments less frequently than standard demand theory would predict. This could suggest that individuals value objects differently according to whether they possess them or not. Taken at face value, this exchange asymmetry or "endowment effect"[1] implies that subjects may well miss out on beneficial trades. To evaluate the impact of such anomalies in actual markets, it is important to understand whether this exchange asymmetry disappears with market experience.[2] List (2003a) ran an experiment where the subjects were dealers acting in a well-functioning market. He shows that the most experienced dealers are indeed not subject to any exchange asymmetry. List's experiment has played a prominent part in the debate about the robustness of laboratory results in the field.

This raises the question of what it is that the market actually does that makes people rational. The answer is generally twofold: the market selects rational individuals – the market acts as a filter for irrational behavior – and provides incentives to correct any mistakes – the market acts as a teacher (see List and Millimet, 2008, and the numerous references therein). Selection by markets is easy to understand. Those who make too many mistakes perform poorly on the market and either choose to withdraw or go bankrupt. But little is known about how market experience succeeds in teaching participants to avoid

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[1]We agree with Plott and Zeiler (2007) that the term "endowment effect" is problematic, as it already entails an interpretation of the phenomenon observed, namely that too little trade in simple exchange experiments is driven by the fact that players are endowed with one of the goods and experience loss aversion with respect to their endowment. We follow Plott and Zeiler in using the term "exchange asymmetry".

[2]A natural question is how individuals can lack market experience since most of us are active in the market nearly every day. This is certainly true with respect to buying. However, most individuals, including typical laboratory subjects, have little experience as sellers. As pointed out by Kahneman et al. (1990) among others, the pathologies in question are most likely to occur on the selling side of the market.

anomalies, such as exchange asymmetry. We make the following four observations should be taken into account to understand this learning process.

First, in List's (2003a) experiments, only traders with intense market experience overcome exchange asymmetry. Specifically, the experienced traders for whom no significant exchange asymmetry is detected are those with six or more trades a month, and they have typically had this experience for several years. Thus, learning seems to be fairly slow.

Second, List (2003a) also reports experiments in which subjects take part in four trading sessions, each separated by a week. He notes a decline in, although not the complete elimination of, exchange asymmetry, concluding that these results "reinforce the notion that useful cognitive capital builds up slowly, over days or years, rather than in the short run of an experiment" (p. 67), as noted previously by Camerer and Hogarth (1999).

Third, the experiments which test for exchange asymmetry are very simple and do not require any computational skills, nor inferences about others' behavior. Subjects are simply asked whether they want to exchange their object for another one. It is thus surprising that a great deal of experience is required to perform such a simple task adequately.

Finally, while List (2003a) uses unique sports collectors' items, List (2004) replicates a classic experiment in which choices involve mugs and chocolate bars, again using participants who have experience in a sports-card market. As in List (2003a) only the participants with very intensive market experience in the sports-card market, here twelve or more trades a month, exhibit no exchange asymmetry in the simple choice experiment with mugs and chocolate bars. These results also suggest that trading behavior learned in one market can be carried over to other markets in substantially different goods.

Taken together, these four observations imply that the market is not easily able to eliminate exchange asymmetry and that learning is slow. Alternatively, it could be the case that no learning occurs at all, but that there is selection (those who are not subject to

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exchange asymmetry simply trade more both in the market and in the laboratory). Given the slow speed at which participants overcome exchange asymmetry (if they learn at all), we may well wonder whether the market is a poor teacher of this subject[3] or rather whether the lessons to be learned to overcome exchange asymmetry are simply very difficult.

One plausible hypothesis is that market experience helps to overcome exchange asymmetry by reducing the uncertainty that participants face. On the one hand, experience with trading certain types of goods should reduce traders' uncertainty about their preferences regarding these goods. On the other hand, the learning spillovers mentioned in the final point above suggest that market experience may also reduce the perceived uncertainty in the trading process. Note that in typical exchange experiments, the objects are carefully chosen to have roughly equal market value. Any small amount of uncertainty may then affect subjects' behavior, even if it appears to be negligible in other experiments. Overall, there are many types of uncertainty that subjects may perceive in the face of a trade opportunity, but we argue that these fall into one of the following two distinct categories: choice uncertainty or trade uncertainty.

Choice uncertainty covers all of the potential sources of uncertainty that matter when an individual has to *choose* between two or more objects. The relative value of the objects at stake could be uncertain, individuals may have incomplete or fuzzy preferences, and so on. Choice uncertainty thus subsumes what we might call object or product uncertainty as well as preference uncertainty. This only addresses uncertainty about which of the consumption bundles is preferred, but does not include phenomena such as loss aversion.

Trade uncertainty concerns market procedures. Individuals sometimes overestimate the cost or risk associated with market transactions. They may thus be reluctant to trade

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[3]We stress that learning in the marketplace regarding other issues may be much faster. For example, market participants may learn quickly how prices are formed on markets.

if the benefits of doing so are too small, judging that these benefits will not cover the transaction costs (including any risks from trade). In general, trade uncertainty concerns any uncertainty regarding the trading procedure itself. At the most basic level, trading, in contrast to choosing, involves a (human) partner. This implies that issues like other-regarding preferences might come into play. For example, as Plott and Zeiler (2007) argue, the typical designs found in exchange experiments entail the potential risk of offending the experimenter by rejecting an initial endowment perceived as a gift. Anything that could be interpreted as uncertainty regarding transaction cost falls into the category of trade uncertainty. If individuals are biased against trading because of such risks or due to a general dislike of controversy or bargaining, or even thinking and deciding, they will exhibit exchange asymmetry that may suggest that they are loss-averse.

This distinction[4] helps us to make sense of the four observations listed above. If subjects perceive trade uncertainty, then in order to realize that trading is not as risky as they feared, they need to experience trade in precisely those situations where they were reluctant to trade. If they are free to choose when to trade, however, they will only very rarely make such trades, for example only if the good to be obtained holds the promise of a substantial gain. The market would thus be a poor teacher because traders will avoid those trades that would teach them the crucial lessons. Hence, if trade uncertainty is largely responsible for exchange asymmetry, this is consistent with learning to overcome it being slow, if it occurs at all. On the other hand, if people learn new trading strategies, they can also apply these

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[4]A similar distinction between what we interpret as trade and choice uncertainty has been suggested by, e.g., Plott and Zeiler (2007) and Braga and Starmer (2005), who distinguish between "institutional learning" and "value learning". This distinction parallels to a certain extent our own between trade and choice uncertainty, but Braga and Starmer's institutional learning is more related to subjects' understanding of the technical functioning of the mechanism (similar to Plott and Zeiler, 2005), whereas trade uncertainty captures the (social) risk associated with the mechanism.

to different types of goods, so that the spillover effects observed in List (2004) are plausible, whereas they cannot be explained by a reduction in choice uncertainty. Furthermore, trade uncertainty makes it plausible that exchange asymmetries appear for everyday consumable goods as those used in List (2004), which are unlikely to cause any choice uncertainty.

In order to test our hypothesis that trade uncertainty is a major factor in explaining exchange asymmetry, we design an experiment that (1) controls for choice uncertainty and (2) impacts on trade uncertainty by providing incentives to consider new trading strategies. Our design incorporates two distinct stages. The first consists of a simple (experimental) market in which subjects can trade with each other without any restrictions on how they interact, bargain, move, and so on. After this training stage, we test for the existence of exchange asymmetry in the second stage, which is carried out in isolation where subjects can only trade with the experimenter. The second stage is identical in all of our treatments.

Our two treatments only differ in one aspect. In one treatment subjects are free to trade at the market stage, while in the other they are forced to trade, i.e. if they do not exchange their initial endowment, they lose it. This "forced" trade encourages participants to trade even in situations where they perceive considerable trade uncertainty and would hence normally avoid trade. As a result, relatively little experience can be sufficient to learn new trading strategies.

We find that when forced to overcome their reluctance to trade during the market stage, subjects no longer subsequently exhibit exchange asymmetry. In contrast, when trade in the market stage is voluntary, we detect clear exchange asymmetry in the second stage. These results support the hypothesis that the exchange asymmetry in our experiment is largely driven by trade uncertainty and probably to a greater extent than by choice uncertainty.

In Section 2 we explain our experimental design and procedures in detail. This is followed by the results in Section 3 and a discussion in Section 4.

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2   Experimental Design and Procedures

Our first experiment was run in April 2007 at the University of Antille-Guyane in Martinique with a total of 74 subjects. A replication, with a much larger number of subjects, 246, was run in September and October 2009 in Paris. Our subjects are Economics students in the first experiment and students in various disciplines in the second. Participants were exogenously sorted into the different treatments.

The laboratory consisted of a circle of 20 small tables. On entering the room, participants drew cards assigning them to one of these tables. There were two stages in the experiment. The first consisted of three interactive trading rounds that provided subjects with the opportunity of gaining trading experience. The second stage was performed in isolation and is a standard test of exchange asymmetry.

In each trading round, the participants were randomly endowed with one of two different goods. After being given the opportunity to freely inspect the goods, they were assigned one of the goods by drawing a card that was then exchanged for the respective good. All the goods had non-trivial value for the participants: a package of coffee and a package of rice (round 1), a packet of crisps and a can of cola (round 2), and a note-pad and a ball-pen (round 3). In Experiment 2, the objects were the same, except in round 1 where the rice was replaced by a set of toothbrushes.

The first stage of the experiment, i.e. the trading rounds, consisted of either "free trade" or "forced trade" rounds of exchange. Interaction, movement and communication were not restricted in any way and participants could see all of the other participants in the session at any time. In the free-trade sessions, participants were free to trade with any of the participants who was endowed with the other good. Participants could keep the good they possessed at the end of each round, whether it be that with which they were endowed at

the beginning of the round or the other. The duration of each trading round was restricted to a total of 5 minutes.

The forced-trade sessions differed from the free-trade sessions in only one respect. Participants were only allowed to keep the good they possessed at the end of the round if it was *not* the type of good with which they were originally endowed. If they were still in possession of their endowment good, they had to return it to the experimenter. They were thus "forced" to trade, in the sense that they had to trade with a participant who was endowed with a different good in order not to forfeit their goods. This procedure was aimed as a shock-therapy for participants who are generally reluctant to trade.

In all sessions, we gradually introduced an imbalance in the endowments over the three rounds. In the first round, the two goods at stake were given out in equal numbers. So exactly half of the participants received one good and the other half received the other good. In the second round, there were two more items of one of the goods than of the other good, and four more in the third. This increases the number of players who are unable to trade. The aim here was to create pressure on the participants with the good in excess supply to trade fast, in particular in the forced-trade sessions.

After the third trading round, participants were given an additional good as compensation while filling out a survey. In any one session all participants were given the same good. They were informed that they could do whatever they wanted with this good. They were then asked one by one to proceed to an adjacent room with their goods (the ones they kept from the trading rounds and their additional endowed good). Once in isolation, a short exit interview was conducted (which, as expected for such a simple experiment, did not reveal any particular misconceptions by subjects regarding the experimental procedures). The experimenter then offered the opportunity to exchange their additional endowed good

		Endowment DVD	Endowment Paper	Total
Experiment 1	Free Trade	free-D (18)	free-P (20)	38
	Forced Trade	forced-D (16)	forced-P (20)	36
	Total	34	40	
		Endowment Choc	Endowment Mug	Total
Experiment 2	Free Trade	free-C (60; 3)	free-M (76; 4)	136
	Forced Trade	forced-C (56; 3)	forced-M (54; 3)	110
	Total	116	130	

Table 1: Summary of the experimental treatments. Endowment is the type of good given as compensation for participation after the end of the first stage. The number of participants in each treatment is given in parentheses. For Experiment 2 the number of independent observations is also given in parentheses. Totals refer to the total numbers across categories.

for another one. This stage was identical in both the forced-trade and the free-trade sessions. The extent of trade at this last stage then serves to test for any exchange asymmetry. The procedure in this stage closely follows that in List (2003a) so as to make our results comparable.

Plott and Zeiler (2007) have demonstrated that exchange asymmetries can be influenced by experimental procedures. In order to control for any aspects that might influence the results, we applied exactly the same procedure in each session.[5]

We hence have a 2x2 design, with one dimension being the type of market experience (free vs. forced) in the first stage of the experiment and the other the type of good with which subjects were endowed in the second stage. In Experiment 1, the second stage involves either a re-writable DVD (D) or a package of copy paper (P). In Experiment 2, we returned to the classic mugs and chocolates. Pre-tests with other students revealed that all of these goods were of non-negligible value for participants. Table 1 summarizes the

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[5]Specifically, in all sessions, the object received at the end of the trading rounds was put in front of the subjects. It was made clear to them that the object was theirs and that they were free to use it as they wanted, with the precise wording fixed in advance.

	Treatment	Endowment	Leaves with A	Leaves with B	Trade Rate
Experiment 1	Free Trade	A (Paper)	15	5	25%
		B (DVD)	7	11	38.9%
		Average			31.9%
	Forced Trade	A (Paper)	9	11	55%
		B (DVD)	6	10	37.5%
		Average			46.3%
Experiment 2	Free Trade	A (Chocolate)	47	13	21.7%
		B (Mug)	38	38	50%
		Average			35.9%
	Forced Trade	A (Chocolate)	38	18	32.1%
		B (Mug)	39	15	72.2%
		Average			52.2%

Table 2: Goods subjects leave with conditional on goods endowed with in the free-trade and forced-trade treatments. In the absence of exchange asymmetry the good the subjects leave with would have to be independent of their endowment.

treatments. The number of participants in each treatment is shown in parentheses. We carried out four independent sessions in Experiment 1 and 13 in Experiment 2, with 16 to 20 participants in each session.

3   Experimental Results

In the absence of any exchange asymmetry, the average trade rate across endowments in the final stage should be close to 50%. Exchange asymmetry implies a smaller average rate of trade. If, as we hypothesize, trade uncertainty is largely responsible for exchange asymmetry, the latter should be substantially reduced after forced trade. It is also reasonable to expect that the participants in the free-trade sessions who trade more frequently would exhibit less exchange asymmetry than participants who trade less frequently. This would,

however, not allow us to infer that the former participants learn to trade, as this could just reflect a selection effect with those who are generally more willing to trade trading more, both in the three rounds of free trade and in the second stage of the experiment.

We find clear support for our main hypotheses. Table 2 shows the relationship between the initial endowment and the good the subjects leave with and the trade rates. The average trade rate is substantially below the rational level in the free-trade treatments.[6] There is thus considerable exchange asymmetry in the free-trade treatments of both experiments. Fisher's exact test indeed rejects for the free-trade treatments the H_0 that the good subjects leave with is independent of their endowment (two-sided, $p = 0.047$ in Experiment 1 and $p = 0.001$ in Experiment 2).[7]

In contrast, the forced-trade treatments in both experiments yield average trade rates much closer to 50%. As expected, for the forced-trade treatments Fisher's exact test cannot reject the H_0 that the good subjects leave with is independent of their endowment at any conventional level of significance ($p = 0.741$, in Experiment 1 and $p = 0.68$ in Experiment 2, and similarly for a χ^2-test $p = 0.65$, in Experiment 1 and $p = 0.617$ in Experiment 2).

The greater number of independent observations in Experiment 2 allow for more in-depth analysis. Using these data only, we carried out two additional tests. We restrict this analysis to Experiment 2, because we used different goods in Experiment 1, which was also run in a different location. Pooling both experiments would thus require adding a number of controls for only slightly more observations.

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[6]Overall, we observed a general preference for chocolate in Experiment 2. This general preference is not a problem, as the mean exchange rate controls for any bias in general preferences.

[7]For a χ^2-test we get $p = 0.024$ in Experiment 1 and $p = 0.001$ in Experiment 2. A caveat regarding these tests is that they treat the data as independent even though the participants interacted before we employed our measure of exchange asymmetry. We note, however, that the last stage was conducted independently for each participant and involved trade with the experimenter with a one-shot option, in contrast to open haggling in a large group.

	Free Trade	Forced Trade
Endowment Choc	$0.2; 0.2; 0.25$	$0.35; 0.22; 0.39$
	$-0.07; -0.07; -0.02$	$0.08; -0.05; 0.12$
Endowment Mug	$0.6; 0.44; 0.4; 0.56$	$0.75; 0.69; 0.72$
	$0.01; -0.15; -0.19; -0.04$	$0.16; 0.1; 0.13$

Table 3: Trade rates (top) and normalized trade rates (bottom) in the individual sessions in Experiment 2. The normalized trade rate is calculated by subtracting the average trade rate for the endowed good.

First, to compare the trade rates between treatments while taking any possible dependence of the data within sessions into account, we ran Mann-Whitney tests comparing the trade rates per session between the free-trade and forced-trade treatments. The trade rates for the sessions endowed with mugs are generally higher (59.2% overall) than for those endowed with chocolates (26.7%). Therefore, in order to be able to compare the impact of free and forced trade using all sessions, while controlling for preferences regarding the endowed goods, we normalize the trade rate in each session by subtracting the average trade rate for this good (note that this is the average across all sessions endowed with the good, both those in free trade and forced trade). See Table 3 for the trade rates and normalized trade rates in the individual sessions of Experiment 2. The normalized trade rates are significantly higher in forced trade than in free trade ($z = -2.575, p = 0.01$).

Second, we ran a probit regression with the dependent variable being whether the participant leaves with the mug or not, and the explanatory variable being a dummy for whether she was endowed with the mug, $MugEndow$ (with robust standard errors clustered at the session level), as shown in Table 4. We can see that the endowment matters in the free-trade treatments (see column (1)) but not in the forced-trade treatment (see column (2)). Moreover, if we run this regression pooled over both the free- and forced-trade treatments of Experiment 2 and include a dummy for the forced-trade treatment $Forced$ and an in-

	(1)		(2)		(3)	
	Free Trade Only		Forced Trade Only		Both Treatments	
	Coeff.	p	Coeff.	p	Coeff.	p
$MugEndow$	0.7835 (0.1233)	< 0.001	−0.1257 (0.1309)	0.337	0.7835 (0.1188)	< 0.001
$Forced$					0.3198 (0.1255)	0.011
$MugEndow$ $XForced$					−0.9092 (0.1720)	< 0.001
Constant	−0.7835 (0.0501)	< 0.001	−0.4637 (0.1220)	< 0.001	−0.7835 (0.0483)	< 0.001

Table 4: Probit regressions for the event that the participant leaves with the mug in Experiment 2. Robust standard errors (clustered at the session level) in parentheses.

teraction effect $MugEndowXForced$ (see column (3)), we find a significant effect of both $MugEndow$, representing the trade asymmetry in the free-trade treatment, and $Forced$, indicating that participants endowed with the chocolate are more likely to leave with the mug after forced trade than after free trade. This is consistent with forced-trade training making the endowment less relevant. Most importantly, the interaction effect is highly significant and negative with a slightly larger absolute value than that of the coefficient on $MugEndow$. Under forced trade the trade asymmetry is not only significantly smaller than under free trade, but it disappears completely.

To address the additional question whether exchange asymmetry is stronger for those participants in the free-trade treatment who trade only little in the three rounds of market trade, we split the sample according to the number of trades. The results are given in Table 5. Most subjects either never trade or do so only once (60 or 58, respectively), and only 10 trade twice and 8 trade three times. The average trade rates in the second stage are almost identical for the subjects who do not trade in the first stage of the free-

Experience	Endowment	Leaves with Chocolate	Leaves with Mug	Trade Rate
	Chocolate	15	5	25%
0 trades in stage 1	Mug	17	23	42.5%
	Average			33.8%
	Chocolate	23	4	14.8%
1 trade in stage 1	Mug	17	14	54.8%
	Average			34.8%
	Chocolate	9	4	30.8%
2-3 trades in stage 1	Mug	4	1	80%
	Average			55.4%

Table 5: The goods that the subjects leave with, conditional on their endowment goods in the free-trade treatment in Experiment 2, separated by the number of trades made in the free-trade training rounds ("Experience"). In the absence of exchange asymmetry the good the participants leave with should be independent of their endowment.

trade treatment and those who did so only once. In both cases there is significant trade asymmetry according to Fisher's exact test ($p = 0.027$ and $p = 0.022$, respectively, and $p = 0.017$ and $p = 0.013$ for a χ^2 test)) and probit regressions (see Table 6, columns (1) and (2), respectively).

For the subjects who traded two or three times in the training rounds of the free-trade treatment, however, being endowed with the mug has no significant effect on the likelihood of leaving with the mug, according to Fisher's exact test ($p = 1$, and for a χ^2 test $p = 0.648$) and the probit regression (column (3) in Table 6). Moreover, in a pooled probit over all subjects in the free-trade treatment, with a dummy for the subject trading at least twice, and an interaction of this dummy with the mug-endowment dummy, the latter is negative and significant at the 10% level (see column (4) in Table 6), showing that trade asymmetry is significantly smaller (to the extent that it disappears) for the subjects who trade at least twice in the training rounds. Since we have relatively few subjects who trade at least twice

	(1)		(2)		(3)		(4)	
	0 Trades		1 Trade		2-3 Trades		All	
	Coeff.	p	Coeff.	p	Coeff.	p	Coeff.	p
MugEndow	0.8636 (0.2912)	0.003	0.9228 (0.2195)	< 0.001	−0.3392 (0.6796)	0.618	0.9254 (0.1881)	< 0.001
Minimum 2 Trades							0.3700 (0.5002)	0.459
Minimum 2 TradesX MugEndow							−1.2646 (0.7345)	0.085
Constant	−0.6745 (0.2619)	0.010	−1.0444 (0.0719)	< 0.001	−0.5024 (0.3449)	0.145	−0.8724 (0.1602)	< 0.001

Table 6: Probit regressions for the event that the participant leaves with the mug in the free-trade treatment of Experiment 2. Robust standard errors (clustered at the session level) in parentheses.

in the training rounds, and there is no difference between those who trade never or only once (and those who traded three times in the training rounds actually traded less frequently in the second stage than those who traded twice), the impact of the number of trades is not entirely robust.[8]

More importantly, in contrast to our comparison between free and forced trade, when we look at the effect of the number of trades carried out in the free-trade training rounds, we cannot distinguish learning from selection. Some subjects may be more prone to trade, and would thus trade more in both the first and second stages of the experiment.

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[8]This can also be seen in a regression where we replace the dummy for having traded at least twice with the number of actual trades carried out in the training stage. The coefficient on the relevant interaction term just misses significance at the 10%-level.

4 Discussion

We have shown that a simple design feature, "forced trade", eliminates exchange asymmetry in an environment where the same amount of market experience, "free trade", yields significant and substantial exchange asymmetry. Our results strengthen those of List (2003a, 2004), and further call into question the importance of anomalies such as exchange asymmetry in settings where experienced agents populate the market. In particular, we find that even very limited experience can be effective in eliminating exchange asymmetry. We conjecture that this is so because subjects learn particularly if they are forced to make trades that they would otherwise not carry out. In what follows, we address a number of important questions regarding the causes of exchange asymmetry, what makes it disappear and whether it is a real phenomenon or an artefact of biased experimental procedures, considering the related literature.

Obviously, something changes because of forced trade. The model of reference-dependent preferences of Kőszegi and Rabin (2006) provides a useful framework to address the question of what may have changed. Their model contains three key elements of individual behavior: "consumption" utility – derived from consuming goods, "gain/loss" utility – derived from the difference between a reference point and actual consumption, and expectations about future consumption that determine this reference point. Each of these elements may be affected by our forced-trade treatment and thus drive our results.

We can safely rule out the possibility that forced trade affects consumption utility in a different way from free trade. The goods we used in our experiments are simple objects that subjects encounter often in their daily life and that they had time to freely inspect in both treatments. Furthermore, in the second stage of the experiment, the goods traded are different from those in the first stage, and it would seem implausible that experience with

the latter changes the consumption utility derived from the former. Note that what we call choice uncertainty is exactly captured by uncertainty in consumption utility.

Hence, to explain our results in the Kőszegi-Rabin framework, some change must have occurred in the gain/loss utility function or in expectations and thus the reference point. One potential change in the gain/loss utility function would come from lower loss aversion, as suggested by List (2003b, p. 23): *"the main effect of endowment is not to enhance the appeal of the good one owns, but rather the pain of giving it up (Loewenstein and Kahneman, 1991). Thus, via market interaction and numerous arbitrage opportunities, practiced agents may have learned to overcome this 'pain' and treat the good leaving their endowment as an opportunity cost rather than a loss."* According to this reading, subjects may shy away from trade when they are particularly reluctant to give up a good. Our results would then suggest that being forced to give up a good teaches subjects that their disutility from losses is actually smaller than they had expected. The results would also suggest that one voluntary trade does not lead subjects to learn sufficiently about their true loss aversion.

The third alternative within the Kőszegi-Rabin framework is then that forced trade has shifted expectations about future trading and hence the reference point. The question is then why these expectations are more receptive to forced than to free trade. In both treatments, and in sharp contrast to most previous experiments, the subjects are fully aware that the experiment in which they are taking part concerns trading (e.g. this was written explicitly on the instruction sheets). Their expectations should then be shifted towards trading in *both* treatments. Certainly, subjects who traded once or more in the training rounds of the free-trade treatment should expect to be likely to trade also in the second stage. However, the trade asymmetry is as strong for those who trade once as for those who do not trade at all (see Table 5), and the difference between treatments remains significant when we exclude the latter subjects. Expectations about the reference point thus

do not appear to account for the observed difference between treatments. It may also seem surprising that forced trade would shift the expectation exactly enough that we observe trade close to its rational rate of 50%.[9]

The explanation we propose is also based on changes in expectations, but does not rely on loss aversion. Assume that subjects form beliefs not only about their future reference points, but also about the possibility that trading will lead to some costly mistakes, which is what we called trade uncertainty. In this interpretation, subjects perceive trading as risky and are thus willing to trade only if they have a strong enough preference for the alternative good. Why then do subjects not appear to learn sufficiently about trade uncertainty when they trade once in the free-trade treatment? Our conjecture is that in the cases where they do trade (because they have strong preferences over the goods), they do not think much about any risk (since their thoughts are focused on the much more attractive alternative good) and hence learn little about trade risks. This is similar to the behavior often observed in experiments where subjects have to price lotteries. When the stakes are high they tend to focus on the gain, and forget about the risk. This type of behavior is a major cause of observed preference reversal (see Seidl, 2002, for a survey).[10] Forced trade, on the other hand, allows traders to learn that trade is not as risky as they expected since they are

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[9]A related issue is the possibility that our results are confounded by experimenter demand effects. Subjects may perceive the forced-trade treatment as a signal that the experimenter would like them to trade. This could bias the results in the opposite direction to any naturally-occurring exchange asymmetry, without eliminating the forces that drive the latter. If this is the case, there is again no particular reason for the exchange rate to be almost exactly 50%.

[10]Alternatively, individual subjects may not perceive the same degree of trade uncertainty in all trades. Perceived trade uncertainty may depend on a number of factors and subjects may perceive relatively little uncertainty in the market stage and greater uncertainty in the exchange with the experimenter, for example because they feel more comfortable interacting with their peers. Trading in the face of little uncertainty may well not provide any insight into trade under greater uncertainty.

forced to make trades they would otherwise shy away from exactly because they focus on this risk.[11]

Finally, we address the question of whether exchange asymmetry is a real phenomenon or an artefact of experimental procedures, as Plott and Zeiler (2007) argue, based on their demonstration that exchange asymmetry is sensitive to various experimental features. Their results are broadly consistent with ours in that the variables they identify as crucial are those closely related to trade uncertainty (such as the method of endowing subjects with a good), whereas those addressing only aspects of choice uncertainty (such as public revelation of choice) do not eliminate exchange asymmetry (although we note that these variables cannot always be unambiguously assigned to one of our two categories). Nevertheless, our research leaves open the possibility that exchange asymmetries occur outside the laboratory. Trade uncertainty may well be perceived in markets. For example, this may explain (without requiring loss aversion) phenomena such as the fact that people tend to stick with the default for pension plans. As our results suggest, markets may not provide sufficient incentives to explore new strategies that help to overcome exchange asymmetry, hence the asymmetry persists. Forced trade may well be difficult to implement outside of the laboratory, but exogenous shocks may produce comparable effects and hence enable us to study it in the field. For instance, the subprime crisis forced an unusually large number of homeowners to sell their houses. An intriguing question is whether this will have an effect on the future willingness of the affected individuals to trade on the housing or other markets (the result here could work in the direction of less trade, as the experience of having to trade may be worse than expected and thus reinforce any reluctance to trade.)

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[11]We can also interpret these different explanations from the perspective of Plott's (1996) discovered-preference hypothesis. The explanation based on consumption utility would be a direct application of this hypothesis. List's explanation would correspond to traders discovering their preferences regarding losses rather than their preferences regarding the goods. Our explanation could be interpreted as subjects discovering their preferences over trade itself.

To summarize, our forced-trade treatment appears to remove any reluctance to trade, and observed trading rates are, thus, as if subjects simply traded according to their preferences between the goods, without the endowment mattering. Our experiments certainly have not provided a definitive answer to the question of whether preferences or beliefs have changed. As a more general contribution, our experiments also represent one step in the crossing of the bridge between the laboratory and the field in both directions. It has been suggested that market experience is a powerful tool to induce rationality. The laboratory offers the kind of controls that are required to distinguish between different channels via which markets promote rationality. In the other direction, the laboratory results suggest experiments in which the effective learning devices in the laboratory can be implemented in the field. For example, we could consider whether the forced trade experienced in the laboratory spills over to trading behavior in the field, and for how long any such effects last (i.e. have we "cured" our subjects for good?).

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