



Exchange asymmetry in experimental settings

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ABSTRACT

We review past trading experiments and present 11 new experiments designed to show how the trading rate responds to alterations of the experimental procedure. In agreement with earlier studies, results show that if the trade decision is converted to one resembling a choice between goods the exchange asymmetry disappears, but otherwise the asymmetry is remarkably robust. Results also indicate that when trading is public herding can occur, which may have caused some of the more extreme examples of the exchange asymmetry, and that some of the lack of trade may result from preference indifference. Nevertheless, some form of status quo bias, which may consist of a combination of loss aversion, gain attraction, regret avoidance, and dislike of trading, and which may be enhanced by indifference between the goods offered, probably remains as an important influence on the trading rate.

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1. Introduction

In 1989, Jack Knetsch presented what seemed the most cogent evidence of loss aversion obtained in an economic setting (Knetsch, 1989). The beauty of his 1989 experiment was its simplicity, for it avoided altogether the need to decide on a price. Subjects were merely asked if they wanted to trade their item for an alternate item of roughly equal market value. Knetsch's experiment suggested loss aversion more cleanly than previous experiments of purchase and sale decisions (Knetsch and Sinden, 1984), or than subsequent experiments asking subjects for the prices at which they would be willing to buy or sell an item (e.g., Kahneman et al., 1990).

Knetsch's experiment demonstrated an extreme absence of trading, which led him to question the foundation of consumer theory. Using coffee mugs and large chocolate bars, Knetsch (1989) showed that when offered a choice between the two items about half of the subjects selected the mug and half the chocolate bar, but when subjects of another group were each given a chocolate bar and then offered the opportunity to trade it for a mug only about 10 percent did so. Similarly, when subjects of a third group were each given a mug and then offered the opportunity to trade it for a chocolate bar only

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about 10 percent did so. Of course, if the two goods were preferred about equally across the sample, as the choices of the first group indicated, we would expect about half of the subjects in the second and third groups to trade when given the opportunity. Knetsch not only argued that this lack of trading was evidence of loss aversion—the notion that a loss is of greater consequence than an equal-sized gain—but also proposed that the presence of loss aversion indicates that a single indifference curve is inadequate to depict the tradeoff between any two goods.

Knetsch's trading experiment went largely unchallenged during the 1990s but has subsequently received increasing interest. Recent experiments have replicated his findings, tested the effect of altering aspects of his experimental procedure, and questioned the loss aversion claim. These experiments reflect a growing understanding in economics that experimental details, even apparently inconsequential ones, may matter. As Smith (2010) stated, "Testing involves a blizzard of narrowly prescribed circumstances that are not part of the theory." In the spirit of this realization, the following question about Knetsch's experiment, also addressed by Plott and Zeiler (2007), arises: is the observed lack of trade a fundamental human tendency that transcends methodological details, or rather an artifact of the particular methods he used? A second pertinent question is: if the experimental result holds up to scrutiny, is loss aversion the indisputable source of the anomalous lack of trading?

We have two objectives for this paper. The first is to reach a better understanding of how the procedures of a trading experiment affect the trading rate. Although the exchange asymmetry¹ has been replicated in numerous experiments, including several reported for the first time here, none of the newer trading experiments has found as little trading as did Knetsch in his early experiments. We examine how changes in methodology have led to this narrowing of the exchange asymmetry. We find that the exchange asymmetry is remarkably robust—that the only experiments that have not resulted in a significant asymmetry use either exceptional subjects or methods that may dilute the sense of endowment that is thought to cause the lack of trade. However, the ultimate reason for the lack of trading remains a matter of conjecture.

Our second objective is to reconsider the reasons for the exchange asymmetry. We review the various explanations that have been offered for the exchange asymmetry—loss aversion, gain attraction, regret avoidance, and anti-trade bias—and show that preference indifference or imprecision can also contribute to the observed asymmetry.

2. Background

2.1. Review of past findings

Knetsch's (1989, 1992) early trading experiments prompted several others. We focus here on experiments that, like Knetsch's, involve subjects interacting with the experimenter.² Most of the experiments reported early on by Knetsch and in seven subsequent papers are summarized in Table 1. The basic procedure used in these experiments is a simple split sample, where one group of subjects is endowed with good A and then offered the opportunity to trade it for good B, and another group of subjects is endowed with good B and then offered the opportunity to trade it for good A. Given finely tuned (i.e., complete) preferences and no aversion to trading, we would expect that both groups would end up with an equal proportion of subjects owning good A (and good B), and hence that the average of the trading rates of two groups of subjects would equal 50%.

In the first of the seven more recent studies, Harbaugh et al. (2001) randomly gave students in a room one or another good. The students then individually marked a form to indicate whether or not they wanted to trade their good for the alternate good (Knetsch had endowed all subjects in the room with the same good and had instructed them to raise their hand to trade). Across two separate groups of students about one-fourth traded (numbers 3 and 4, Table 1), compared with only about 10% in Knetsch's experiments.³ Notably, the decision in this experiment was private. Further, the goods were randomly distributed so that some subjects received one good and others received the other good, which would help avoid the impression that the experimenter intended that subjects accept or prefer one good over the other. These changes diminished but did not eliminate the exchange asymmetry.

In the second study, using individual subjects in a field setting, in contrast to the more common approach of using groups of subjects in a more controlled setting, List (2003) enlisted participation of consumers and dealers at trade shows and asked them to complete a brief questionnaire. In return for obliging, the subject was given a small item. Upon completion of the questionnaire, the experimenter revealed an alternate item and presented it to the respondent for inspection. The respondent was then asked if he or she wanted to trade the original item for the alternate one. Interestingly, List found across several experiments that, although only about 20% of inexperienced consumers traded, over 40% of experienced consumers and dealers traded, whether or not the goods involved were relevant to the trade show (numbers 5 and 6,

¹ Authors have used several terms, including "reluctance to trade" and "endowment effect," to describe the unexpected lack of trading that has been observed in experiments like those described here (e.g., Korobkin, 2003). As Plott and Zeiler (2007) maintain, such terms tend to suggest a reason for the lack of trade. Plott and Zeiler favor the less suggestive term "exchange asymmetry." We use that term, or simply "lack of trade", and we use "trade" and "exchange" interchangeably.

² Other experiments (e.g., van Kijk and van Knippenberg, 1998) have allowed subjects to trade among themselves.

³ Chapman (1998) was apparently the first to use a response form and randomly give subjects in the room one or another item. The study is not included in Table 1 because the paper does not report key information required for the table. Across three experiments that each used different sets of items, roughly one-third of the 122 subjects traded, resulting in a significant exchange asymmetry ($p < 0.01$).

Table 1
Prior experiments.

Number	Study	Treatment	Goods (A, B)	No. of goods endowed ^a	How goods allocated? ^b	Where the goods were ^c	Passage of time after endowment ^d	Indication of subject's decision	Number initially endowed with goods A, B ^e	Final allocation of good A (%) ^f	Test of proportions p value ^g	Average % trading ^h
1	Knetsch (1989)	Original	Mug, chocolate	One	G	S, E	Yes	Raise hand to trade	76, 87	89, 10	<0.001	10
2	Knetsch (1992)	Trade inducement	Mug, pen	One	G	S, E	Yes	Raise hand to trade	51, 49	88, 10	<0.001	11
3	Harbaugh et al. (2001)	Children pair 2	Pencil, pen	Two	H	S, E	No	Form: keep or trade	62, 63	76, 16	0.001	20
4	Harbaugh et al. (2001)	Under-grads pair 2	Pencil, pen	Two	H	S, E	No	Form: keep or trade	19, 19	63, 16	0.003	27
5	List (2003)ⁱ	Experiment 1, nondealers	Game stub, Ryan cert.	One	G	S, S	Yes	Verbal: keep or trade	35, 39	80, 26	<0.001	23
6	List (2003)	Experiment 1, dealers	Game stub, Ryan cert.	One	G	S, S	Yes	Verbal: keep or trade	35, 39	54, 44	0.179	45
7	Plott and Zeiler (2007)	Baseline procedure	Mug, pen	One	G	S, E	Yes	Raise hand to not trade	64, 65	84, 28	<0.001	22
8	Plott and Zeiler (2007)	Standard procedure	Mug, pen	One	G	S, E	Yes	Raise hand to trade	44, 52	77, 62	0.049	42
9	Plott and Zeiler (2007)	Transaction costs	Mug, pen	One	G	E, S	Yes	Form: keep or trade	53, 48	72, 50	0.013	39
10	Plott and Zeiler (2007)	Loss emphasis	Mug, pen	One	R	S, S	Yes	Form: keep or trade	44, 43	82, 74	0.283	46
11	Plott and Zeiler (2007)	Full set of controls	Mug, pen	One	R	E, S	Yes	Form: mug, pen or don't care	69, 70	54, 67	0.948	57
12	Knetsch and Wong (2009)	Not owned, strong preference	Mug, pen	One	G	S, E	Yes	Form: keep or exchange	49, 49	67, 14	<0.001	23
13	Knetsch and Wong (2009)	Owned, semi-strong preference	Mug, pen	Two	H	E, E	Yes	Form: keep or trade	46, 54	50, 31	0.030	41
14	Knetsch and Wong (2009)	Owned, weak preference	Mug, pen	One	R	E, E	Yes	Form: mug or pen	47, 42	49, 50	0.540	51
15	Engelmann and Hollard (2010)	Experiment 2, free trade	Mug, chocolate	One	G	S, E	No	Verbal: keep or trade	76, 60	50, 22	<0.001	36
16	Engelmann and Hollard (2010)	Experiment 2, forced trade	Mug, chocolate	One	G	S, E	No	Verbal: keep or trade	54, 56	28, 32	0.691	52
17	Heffetz and List (2014)	Experiment 2, more endowment	Mug, pen	Two	I	S, S	Yes	Form: keep or trade	57, 60	74, 52	0.007	39
18	Heffetz and List (2014)	Experiment 2, less endowment	Mug, pen	Two	I	S, S	Yes	Form: mug or pen	63, 53	65, 74	0.838	54
19	Ratan (2014)	Treatment H, can't change	Drink bottle, bookmark	Two	H	S, S	Yes	Form: keep or exchange	87, 100	63, 42	0.002	39
20	Ratan (2014)	Treatment S, can change	Drink bottle, bookmark	Two	H	S, S	Yes	Form: keep or exchange	68, 85	51, 52	0.514	50

^a Number of different goods initially given to subjects in the room: one = all subjects get the same good; two = some subjects get good A, others good B.

^b G = all subjects received the same good, experimenter decides which; R = all subjects received the same good based on a random process (e.g., coin flip); H = about half of the subjects in the room received one good, remaining subjects received the other good; I = subjects individually flipped a coin to determine which good they received.

^c Location of endowed good, alternate good, at time of decision (S = with subject, E = with experimenter).

^d Was a questionnaire administered after the endowed good was distributed and before the alternate good was presented?

^e Initial number of good A owners, good B owners.

^f Percent of good A owners who kept good A, good B owners who traded for good A.

^g Significance level (p value) of the z-test of equality of proportions, one-sided. Ho: $p_1 = p_2$; Ha: $p_1 > p_2$; p_1 = the proportion of good A owners who kept good A, p_2 = the proportion of good B owners who traded good B for good A.

^h The simple average of the percentages of the two groups of subjects that trade for or otherwise choose the other good.

ⁱ Unlike in the other experiments, in this study only one subject was present at a time. [List \(2004\)](#) reports similar results.

Table 1). List concluded that trading experience played a significant role in eliminating the exchange asymmetry. List's result with inexperienced subjects is notable because each subject had both the endowed good and the alternate good in hand when he or she decided whether or not to trade, an issue we return to below.

The third study, by [Plott and Zeiler \(2007\)](#), systematically examined several features of Knetsch's methods. By carefully controlling aspects of the social setting of the experiment and how information and instructions were communicated, Plott and Zeiler obtained trading rates of around 40% in several experiments (numbers 8–10, Table 1). For example, they lessened the transaction cost of trading by leaving the alternate good but not the endowed good with the subject when the trading decision was made (number 9, Table 1). In an additional experiment (number 11, Table 1) they achieved a trading rate above 50%. However, their procedures in that exceptional experiment may have overly de-emphasised the sense of endowment upon which the exchange asymmetry may rely, as well as initiated a herd effect (see also [Knetsch and Wong, 2009](#)).

Three recent trading studies provide clever additions to this literature. First, [Engelmann and Hollard \(2010\)](#) designed a two-stage experiment to examine the possibility that uncertainty about the trading procedure, or a basic bias against trading, could cause an exchange asymmetry. In stage 1, subjects were presented sequentially with three separate opportunities to trade among themselves; in each of these three trading rounds some subjects were given one good and others were given another good. In stage 2 subjects were all endowed with a certain good and then individually and privately offered the opportunity to trade the endowed good for an alternative. When the trading during stage 1 was voluntary (their "free trade" condition), 36% traded in stage 2, but when the trading in stage 1 was forced, 52% traded in stage 2 (numbers 15 and 16, Table 1).⁴ Of particular interest is the behavior of subjects in Engelmann and Hollard's voluntary trading condition. When stage 1 trading was voluntary, 44% of the subjects did not trade during any of the three opportunities and another 43% traded only once. Both of these groups of subjects subsequently showed some reluctance to trade in stage 2, as about 34% of each group traded. However, the minority of subjects who traded most of the time during stage 1 (13% of the subjects traded in at least two of the three opportunities) showed no reluctance to trade in stage 2 (55% traded). This result suggests that whereas most subjects are reluctant or at least cautious traders, other subjects exhibit no such reluctance. This finding is reminiscent of List's finding that some people are more active traders than others, and suggests the hypothesis that some subjects enjoy the act of trading, while others do not.⁵

Second, [Heffetz and List \(2014\)](#) experimented with, among other things, the degree to which the instructions emphasize endowment. With both items in front of each subject, they individually tossed a coin to determine which item was endowed. When ownership of the endowed good was emphasized and subjects were asked if, later given the opportunity, they would trade the endowed good for the alternative good, there was a significant lack of trade (number 17, Table 1). However, when ownership of the assigned good was not emphasized and subjects were asked which good they would choose, there was no reluctance to switch to the other good (number 18, Table 1). Their results clearly show that de-emphasizing endowment and asking for a choice between goods rather than a decision about trading something one owns clearly affects the outcome.⁶

In the third recent study, [Ratan \(2014\)](#) examined the effect of providing subjects an opportunity to change their minds if they decided after the experiment that they had made the wrong decision. Respondents were each assigned one of the two available goods, with the assignment roughly equal among subjects in the room. Later the other goods were handed out, so each subject had both goods in their possession. All subjects were then given the opportunity to "exchange" their good for the alternate good. The "can change" treatment was identical to the "can't change" treatment except that, before subjects made their exchange decision, they were told that within the next 24 h they could switch goods as long as the good they left the experiment with was still unused. The familiar lack of trade was found in the "can't change" treatment but it was absent in the "can change" treatment (numbers 19 and 20, Table 1). Ratan argued that offering subjects the chance to reverse their decision removed the worry about future regret that might keep some subjects from willingly trading away their endowed good.

Table 1 summarizes key features of 20 experiments. The experiments listed in the Table differ in (1) the goods used; (2) whether, when a group of subjects was present, every subject in the room was endowed with the same good (either A or B) or some received good A and others good B; (3) when only one good was distributed, whether or not the experimenter informed subjects that the good was randomly selected between two available alternatives; (4) who had physical possession of the endowed good and the alternate good (subject or experimenter) when subjects indicated their decisions about whether or

⁴ Subjects in the forced-trade treatment of stage 1 were instructed in each round that if they did not trade they would forfeit their good. In the first round the two goods were distributed in equal numbers, so all subjects had the opportunity to trade, but in the other two rounds unequal numbers of the two goods were distributed, assuring that some subjects would fail to trade and would therefore forfeit their good. That is, failure to trade quickly was punished. The authors assert that the forced-trade treatment allowed subjects to learn that trade was not as risky as they may have expected, but another possibility is that having earlier faced penalties for failing to trade, some subjects were overly anxious to trade in stage 2.

⁵ Additional evidence of how people differ in their proclivity to trade is presented by [de Sousa and Munro \(2012\)](#), who in an online gaming environment found that accomplished gamers appeared to like to trade, as they traded about 70% of the time, in comparison to novices who were generally reluctant to trade.

⁶ In three different experiments [Heffetz and List \(2014\)](#) also examined the effect of making the choice/trade decision conditional, in the sense that when subjects made their decision they knew only the probability that their decision would affect the outcome. (This approach was first used by [Ericson and Fuster \(2011\)](#) in an experiment that assigned only one good to subjects, making it unsuitable for inclusion in Table 1.) Altering the probability of getting to determine the outcome was implemented as a way to test for the importance of expectations, the importance of which was proposed by [Kőszegi and Rabin \(2006\)](#). Contrary to the finding of Ericson and Fuster, increasing the expectation that the subject would end up with the assigned/endowed good (by lowering the probability that the subject's decision would affect the outcome) did not lower the trading rate.

not to trade; (5) how the subjects indicated their decisions, either publically (by raising hands) or privately (by filling in a form or, in the List and the Engelmann and Hollard studies, privately telling the experimenter); (6) the amount of time passing (short versus long) between when subjects received their endowed good and when they decided whether or not to trade; (7) whether subjects were asked if they wanted to trade the endowed good for the alternate, or were asked (despite having been endowed with one of the goods) to choose between the goods, and (8) whether or not subjects were given the opportunity to later reverse their decision.⁷

As seen in Table 1, the null hypothesis of no lack of trade was rejected at the 0.05 level in 13 of the 20 experiments, based on the z-test of equality of independent proportions. In three of the seven experiments where the null hypothesis could not be rejected—numbers 11, 14, and 18 of Table 1—the subjects were asked which good they wanted to keep, not if they wanted to trade. That is, although one good was formally given to the subjects and the other good was not, subjects were nonetheless asked to choose a good. Both Plott and Zeiler (2007) and Knetsch and Wong (2009) addressed the issue of whether asking for a choice between goods is a true test of the exchange asymmetry. In particular, Knetsch and Wong argued that framing the question as one of a choice between goods removes the sense of endowment, and that if an exchange asymmetry depends on a sense of endowment, asking for a choice between goods would preclude the asymmetry.

In two of the other four experiments where a significant lack of trade was not found, the subject pool was either unique or specially conditioned. In the first, List's "dealers" experiment (number 6, Table 1), the subjects were sports card dealers. However, as mentioned above, when the same methods were used with nondealers (number 5, Table 1) the exchange asymmetry was significant (see also List, 2004). List concluded that market experience was key to avoiding the exchange asymmetry. However, another view of List's finding is that dealers have different expectations from ordinary consumers, in that dealers *expect* to exchange and thus would not consider giving up an item as a loss (Barberis, 2013; Köszegi and Rabin, 2006). In any case, the distinction is somewhat off the point here because our subjects, and those of other experiments, being non-specialized samples, have probably lacked much focused trading experience. And in the second of these two experiments, Engelmann and Hollard "forced trade" (number 16, Table 1), the subjects earlier had been put through an exercise where they forfeited their good if they did not trade, an experience that could have pre-conditioned them to want to trade.

One of the final two experiments in which the null hypothesis cannot be rejected is Plott and Zeiler's "loss emphasis" treatment (number 10, Table 1). A key difference between this experiment and most others is that in this experiment both goods (the endowed good and the alternate) were with the subject at the time of the trading decision. It is possible that having both goods so close at hand led some subjects to treat the decision as a choice between goods, even though they were asked if they wanted to trade. However, the strength of this conjecture is weakened by the fact that a significant asymmetry resulted in three other experiments (numbers 5, 17, and 19, Table 1) where subjects had possession of both goods when they made their trade decision.

In the other of the final two experiments (number 20, Table 1), simply offering subjects the option of reversing their decision removed the lack of trade. In this experiment the owned good was clearly endowed—indeed, it was "earned" following completion of a computerized task—and the trade decision was clearly one of "exchange." And the fact that both goods were close at hand when subjects made their trade decision cannot explain the lack of exchange, because that condition was also true in the control treatment (number 19, Table 1) where a significant lack of trade resulted. Ratan (2014) posits that regret avoidance, rather than an attachment to the endowed good, explains the outcome. Hesitations about this, however, are that it is difficult to separate regret from endowment, and that regret would be more likely if the act of endowment somehow made the endowed good more special. Further, it is possible that the opportunity to change one's mind removes the commitment that a final trade entails, potentially making the decision more like a choice between goods.

A final observation about the studies in Table 1 relates to experiments by Knetsch (1989, 1992) and Plott and Zeiler (2007) where subjects raised their hand to trade. Intriguingly these studies contrast with others in the amount of trading that occurs. In the studies by Knetsch only about 10% of the subjects traded, which is considerably below the trading rate of other studies. And in two very similar experiments by Plott and Zeiler, the "baseline procedure" and the "standard procedure" (numbers 7 and 8, Table 1), only 28% of subjects given a pen traded for a mug in the first case but 62% of such subjects traded in the second case. Plott and Zeiler (2007, p. 1461) suggested that the high trading rate of the second case could be due to "signals subjects send to one another when they make public choices." In other words, they suggested the possibility of herd effects, where "herding" refers to the tendency of people to follow the lead of others (Baddeley, 2010; Salazar et al., 2013). Specifically, the relatively extreme non-trading when hands were raised in the early experiments by Knetsch (1989, 1992) suggests that the herd decided not to trade, whereas the unexpectedly high level of trading among pen owners in the "standard procedure" experiment by Plott and Zeiler (2007) suggests that the herd decided to trade. Herding falls under the broad category of impressions management, the process identified in the psychology literature by which people attempt to control the impressions others form of them (Leary and Kowalski, 1990). Such impressions are thought to affect how others

⁷ Not shown in Table 1 are the various different wordings used to inform the subjects that they were being given a good. For example, Plott and Zeiler, who carefully examined the wording issue, used the following different wordings in different experiments: "The mug is yours."; "The mug is yours. You own it."; "I'm giving you the mug. It is a gift. You own it. It is yours." The different wordings do not seem to have had much effect on the results (which is not to say that other wordings would not).

think of the individual and act toward the individual (Schlenker, 1980). Impressions management is pervasive in human relations, and could play a role in trading experiments.

2.2. Summary

To summarize this review of the experimental literature, the likelihood that a subject will exchange the endowed good (E) for the alternate good (X) in a trading experiment appears to be a positive function of three factors: (1) the utility difference $U_X - U_E$, reflecting the degree to which the alternative good is preferred to the endowed good; (2) the utility the subject receives from the act of trading, irrespective of preferences for the goods or concerns about the impressions that trading may convey; and (3) the utility the subject receives from impressions created by the act of trading.

Importantly, U_E , and therefore the $U_X - U_E$ difference, is a function not only of the characteristics of good E but also potentially of loss aversion or other tendencies that may influence the valuation of the endowed good. Such tendencies may be affected by numerous characteristics of a trading experiment, including (1) the degree to which endowment of good E was emphasized by the experimenter or otherwise accented by the experimental procedure, (2) the degree to which the decision is characterized as one of trading versus one of choosing, (3) the amount of time the endowed good was in the subject's possession, (4) whether the subject's decision was final or could later be reversed, and (5) the locations of the two goods at the time of the trading decision. As emphasized by Plott and Zeiler (2007), the first of these characteristics may be influenced by aspects of the experimental protocol such as whether the endowed good was characterized as a gift from the experimenter, whether it was perceived as personally selected by the experimenter or determined randomly, and the degree to which the experimenter emphasized that the subject owns the endowed good.

Regarding the third factor, concerns about the impressions subjects make on others may consciously or unconsciously lead them to trade, or not trade, depending on the circumstances. For example, if other subjects are trading, the subject may act similarly to be perceived as part of the group, or in solidarity with those who are trading. Or if the subject would like to leave a positive impression with the experimenter and perceives that the experimenter would prefer that the subject keep the endowed item, trading is less likely. The role of impressions is affected by the response mode, as a public mode offers more opportunity to make an impression than does a private mode.

In addition to these factors, we add a possibility that has received little attention—that the subject is unable to rank the two items at issue.⁸ If an individual trades good X for good Y we know he (or she) prefers Y to X , but if he does not trade we do not know that he prefers X to Y —we know only that he does not prefer Y to X . In the absence of other influences such as those just described (e.g., loss aversion, herding), a lack of trade could occur if the subject prefers the endowed item, or if the subject is either indifferent between the two items or cannot decide which item is better. Indifference ($X \sim Y$) is not the same thing as indecision; rather, it indicates finely tuned preferences. In contrast, inability to decide which item is best suggests preference imprecision, which as Mandler (2004) explains is an indication of incomplete preferences. Whether due to preference indifference or imprecision, an individual who cannot order the items is likely to fall back on the default position, the status quo, because it requires less active engagement than trading does (Mandler, 2004). Although preference indifference or imprecision could occur when other influences are at play, it is also reasonable to expect that preference indifference or imprecision could occur in the absence of such influences.

3. Further tests of exchange asymmetry

3.1. Introduction

We ran a series of experiments designed to further test for the influence of various characteristics of a trading experiment. The experiments differed first of all in the goods used. Based on others' findings (Table 1), we hypothesized that a significant lack of trade would result for all pairs of goods used. Second, we tested for the effect of the following characteristics that may affect the sense of endowment: good allocation (whether all subjects in the room were endowed with the same good or half were endowed with one good and the other half with the other good); time in possession (the amount of time that passed between the endowment and the trade decision); and the location of the goods at the time of the decision (whether or not the alternate good was in the subject's possession at the time of the trade decision). Based on earlier findings (Table 1), it was expected that a significant lack of trade would result despite giving half the subjects one good and half the other good, allowing little possession time before the trade decision was made, and leaving both goods with the subjects when the trade decision was made. Third, we examined the effect of response mode by making subjects' trade decision public (via raising one's hand) in some experiments and private (by filling in a form) in others. A formal hypothesis was not possible here because of the unpredictable effect of herding, but we planned to see if group size or other factors were related to herding behavior.

⁸ We are aware of only one prior attempt to examine the role of indifference as a cause of the exchange asymmetry, the early study by Knetsch (1992) (number 2, Table 1) where he offered subjects a 5 cent inducement to trade. Knetsch found that the inducement had no effect on the outcome, as seen by comparing experiments 1 and 2 of Table 1, but the possibility remains that 5 cents was too small to make a difference.

Additionally, we examine the possibility that preference indifference or imprecision may contribute to the lack of trade. We assume that if the items differ enough in utility, then trade will happen. That is, an individual will trade when $U_X - U_E$ is large enough to allow a decision to trade. To further examine the possibility that preference indifference could contribute to the observed lack of trade, in two of our experiments we gathered data on preference difference, which we take to be a measure of $U_X - U_E$. We hypothesize that the incidence of trade will vary positively with this difference.

3.2. Methods

We ran 11 experiments, four at Charles Sturt University in New South Wales, Australia, and seven at Colorado State University in the USA, involving nearly 1300 subjects. The experiments are summarized in Table 2. In all experiments, some subjects were endowed with good A and then given the opportunity to trade that good for good B, and vice versa. The presence of an exchange asymmetry was detected, as in Table 1, as a significant difference ($p < 0.05$) in the proportion of subjects across groups ending up with one of the goods, based on a one-tailed z-test of equality of proportions.

The experiments can be divided into two sets, the first six, and the remaining five. The first set of experiments used the original Knetsch (1989) procedure of having subjects raise their hands to trade. The experiments of this set proceeded as follows. Avoiding the word “gift,” the experimenter said “Each of you is being given a (good). . . . The (good) is identical to ones sold in nearby stores, and is yours to keep.” The Charles Sturt experiments, but not the Colorado State experiments, then administered a questionnaire covering a topic unrelated to the trading experiment. In all six experiments, the experimenter then handed each subject a slip of paper with “TRADE” on it, placed an ample supply of the alternate good on the table in the front of the room, held up one of those goods, and said “Now each of you has the opportunity to exchange your (good) for a (other good) exactly like this one.”

What differ among the experiments of the first set are the goods used, the manner of assigning the endowed goods, and the length of time between receiving the endowed good and deciding about trading. Regarding goods, the first two experiments used two different high-quality chocolates, the second two experiments used a pen and an auto dashboard shade, and the third two used a university coffee mug and a bag of individually wrapped dark chocolates. Regarding the manner of assigning the endowed goods, in the first, third, and fifth experiments the experimenter handed the same good to each subject in the room, but in the other three experiments the experimenter alternated, giving the first subject good A, the next subject good B, the third good A, etc., with no suggestion that any thought went into who received what. Alternating between goods avoids signals to subjects in the room that the endowed good is preferable to the alternate good, and lessens (though perhaps does not completely eliminate) subjects’ worries that by trading their good they might offend the experimenter (a concern raised by Plott and Zeiler). Finally, regarding time in possession, the first four experiments included a questionnaire but the last two did not. The purpose of the questionnaire was to let some time pass with the subject in possession of the endowed good, to potentially increase the sense of attachment to the good.

The experiments of the second set all used the same goods, a university coffee mug and a bag of individually-wrapped dark chocolates, and all asked subjects to privately indicate whether or not they wished to trade by writing “keep” or “trade” on individual slips of paper they had been given. The procedure and wording was otherwise very similar to the last two experiments of the first set. Experiments 7 and 9 were similar to experiment 5 in that only one good was endowed, with a sufficient quantity of the alternate good remaining with the experimenter at the front of the room. Experiments 8 and 10 were similar to experiment 6 in that the assignment of endowed goods alternated so that about half the subjects in the room received good A and the others received good B, with additional quantities of the goods up front with the experimenter.⁹

The final (11th) experiment is identical to the 10th one except that the subjects had both goods in front of them when they made their decision (in this respect this experiment is similar experiments 5, 10 and 17–20, Table 1). The experiment proceeded as follows: (1) every subject was given either a bag of chocolates or a coffee mug in alternating order so that about half had each item; (2) subjects were informed “Now that you all have an item we want you to know that we have enough of each item for everyone to have the item they want. We will now hand out the alternative items so you can all get a good look at them;” (3) subjects were given a bag of chocolates or a coffee mug so that each participant had one of each good; and (4) subjects privately wrote “keep” or “trade” on their blank slips of paper. This treatment avoids a transaction cost involved in trading, but it may tend to dilute any sense of endowment. In particular, the statement “for everyone to have the item they want” suggests a choice, in contrast to the decision to be made, which was to keep or trade.

A final manipulation is that a questionnaire was administered to each subject of experiments 9 and 10 approximately one week before the trading experiment. The questionnaire asked subjects to “rate your personal preference for, or liking of,” each of 35 items using a scale ranging from 1 (little/no preference) to 7 (strong preference).¹⁰ The items were 16 snacks (e.g., beef jerky, trail mix), ten accessories (e.g., mini-flashlight, refillable water bottle), and nine bottled drinks (e.g., water, green tea). The experimenter held each item up individually. Among these 35 items, which were all on a table at the front of the

⁹ For the experiments that used two endowed goods in the same room, the following instruction was used: “You are each going to be given either a CSU coffee mug similar to those found at the bookstore and around town or a 9.5-ounce bag of Dove dark chocolates similar to those available at most grocery stores and shopping centers. This item is yours to keep but we ask that you do not use it or open it until you have . . . left the study.”

¹⁰ Ratings are an efficient way to obtain comparative judgments (Brock and Jones, 1968). Although deciding about trading is quite different from rating a set of goods, the two tasks do share the fact that they both rely on comparative judgments.

Table 2
Our experiments.

Experiment	Location and year	Goods (A, B)	No. of goods endowed ^a	Where the goods were ^b	Passage of time after endowment ^c	Indication of subject's decision	Number initially endowed with goods A, B ^d	Final allocation of good A (%) ^e	Test of proportions p value ^f	Average % trading ^g
1	C Sturt U 2005	Maltesers, Toblerone	One	S, E	Yes	Raise hand to trade	152, 144	64, 39	<0.001	38
2	C Sturt U 2005	Maltesers, Toblerone	Two	S, E	Yes	Raise hand to trade	40, 32	60, 44	0.085	42
3	C Sturt U 2005	Pen, shade	One	S, E	Yes	Raise hand to trade	126, 88	90, 53	<0.001	32
4	C Sturt U 2005	Pen, shade	Two	S, E	Yes	Raise hand to trade	34, 37	74, 43	0.005	35
5	Colo St U 2008	Mug, chocolate	One	S, E	No	Raise hand to trade	82, 82	73, 55	0.015	41
6	Colo St U 2008	Mug, chocolate	Two	S, E	No	Raise hand to trade	40, 41	58, 29	0.010	36
7	Colo St U 2008	Mug, chocolate	One	S, E	No	Form: keep or trade	52, 56	75, 36	<0.001	31
8	Colo St U 2008	Mug, chocolate	Two	S, E	No	Form: keep or trade	29, 27	83, 52	0.013	35
9	Colo St U 2009	Mug, chocolate	One	S, E	No	Form: keep or trade	56, 56	77, 18	<0.001	21
10	Colo St U 2009	Mug, chocolate	Two	S, E	No	Form: keep or trade	52, 49	63, 37	0.007	37
11	Colo St U 2009	Mug, chocolate	Two	S, S	No	Form: keep or trade	25, 22	52, 41	0.447	44

^a Number of different goods initially given to subjects in the room: One = all subjects get the same good; Two = some subjects get good A, others good B. A separate column for "How goods allocated?" (see Table 1) was not needed here because it would be redundant with the "No. of goods endowed" column.

^b Location of endowed good, alternate good, at time of decision (S = with subject, E = with experimenter).

^c Was a questionnaire administered after the endowed good was distributed and before the alternate good was presented?

^d Initial number of good A owners, good B owners.

^e Percent of good A owners who kept good A, good B owners who traded for good A.

^f Significance level (p value) of the z -test of equality of proportions, one-sided. $H_0: p_1 = p_2$; $H_a: p_1 > p_2$; p_1 = the proportion of good A owners who kept good A, p_2 = the proportion of good B owners who traded good B for good A.

^g The simple average of the trading rates of the two groups of subjects.

Table 3

Logistic regression model across experiments 1–10.

Variable ^a	β	S.E.	Sig.	^b
DISTRIBUTION	−0.855	0.698	0.221	
RESPONSE MODE	−3.202	1.172	0.006	**
TIME	−2.620	1.173	0.026	*
GROUP SIZE	−0.143	0.057	0.012	*
DISTRIBUTION by RESPONSE MODE	0.773	0.376	0.040	*
DISTRIBUTION by TIME	0.597	0.415	0.150	
DISTRIBUTION by GROUP SIZE	0.032	0.031	0.306	
GROUP SIZE by RESPONSE MODE	0.118	0.057	0.040	*
GROUP SIZE by TIME	0.098	0.057	0.086	
Constant	2.575	1.181	0.029	*

^a Dependent variable: TRADE (0 = no, 1 = yes); DISTRIBUTION (0 = one good, 1 = two goods); RESPONSE MODE (0 = raise hand, 1 = form); TIME (0 = short time, 1 = longer time); GROUP SIZE (size of the group in which the subject participated). Number of cases: 1285. Percent of cases correctly predicted: 66%. −2 Log Likelihood: 1629. Cox and Snell's R^2 : 0.018. Gender was not included in the final model because it was found to have essentially no effect on TRADE.

^b * significant at 5% level, ** significant at 1% level.

room, were the university coffee mug and the bag of chocolates to later be used in the trading experiment. The preference questionnaire was administered to determine the extent to which subjects were indifferent between the two target items, and to see if non-indifferent subjects' ultimate decisions (about which good to leave the room with) coincided with their prior preferences. The questionnaire was administered one week prior, instead of immediately prior, to the trading experiment to avoid any influence of the rating task on the subsequent trading task; it is unlikely that subjects would have remembered their ratings of the two target goods.

Because a seven-point rating scale does not allow for very precise distinctions, a zero rating difference may not indicate true indifference. Further, preferences could change over time.¹¹ However, by and large the rating difference is assumed to indicate $U_X - U_E$. Thus we hypothesize that the incidence of trade will vary positively with $R_X - R_E$, where R indicates a rating; that is, trading will be most likely when $R_X - R_E$ is strongly positive and least likely when $R_X - R_E$ is strongly negative.

3.3. Results

We used the first set of experiments (numbers 1–6, Table 2) to examine three questions: when subjects indicate their decision publically, is the exchange asymmetry invariant to (1) the goods used, (2) whether all subjects in the room received the same good or half of the subjects received good A and the other half good B (a dichotomous variable we call DISTRIBUTION), and (3) whether or not a questionnaire was administered after the endowed good was handed out (a dichotomous variable we call TIME). In five of the six experiments the exchange asymmetry was significant at $p \leq 0.05$ based on the z-test of equality of independent proportions (in the sixth experiment the asymmetry was significant only at $p = 0.085$) (Table 2). These results are consistent with those of earlier studies that used several different pairs of goods, with Chapman (1998) and Harbaugh et al. (2001) who found that avoiding signaling by endowing two different goods still yielded a significant lack of trade, and with prior experiments that did not include a time-consuming questionnaire yet found a significant lack of trade.

In the second set of experiments (numbers 7–11, Table 2) we held the goods constant and asked the subjects to indicate their decision privately. Ignoring for now the preference survey administered prior to experiments 9 and 10, the first four experiments again examined the question of whether the exchange asymmetry is invariant to how the endowed goods are distributed. A significant lack of trade ($p < 0.05$) was found in all four experiments (Table 2).

Combining the data from experiments 1–10 allows us to test the significance of DISTRIBUTION (one good or two), RESPONSE MODE (public or private trading decision), and TIME (short or long). In addition, we tested for the significance of GROUP SIZE (the number of subjects in the room), a covariate about which we had not formed a hypothesis. A logistic regression on the dependent variable TRADE (if the subject traded or not) shows that three of four simple effects (RESPONSE MODE, TIME, and GROUP SIZE) and two of five possible two-way interactions are significant (Table 3). The negative coefficient on RESPONSE MODE indicates that the trading rate was higher if subjects responded publically (raised their hand to trade, trading rate of 0.37) than if subjects responded privately (filled in a form, 0.30). The negative sign for TIME is expected, as it indicates that the trading rate was greater if less time passed between receiving the endowed good and deciding whether or not to trade.¹² The negative sign for GROUP SIZE indicates that the trading rate is greater for smaller groups.

¹¹ Although preferences expressed one week prior are not a perfect indication of current preferences, there is no reason to expect that the time span would systematically increase or decrease preference differences. However, there is the possibility that subjects' personal situations could have changed. For example, a subject could have been craving chocolate when responding to the questionnaire but not so during the experiment.

¹² This finding about time of possession is in line with results reported by Strahilevitz and Loewenstein (1998). However, our finding must be qualified because TIME also represents the location of the experiments. Although there were no systematic differences in procedure between the two locations (other than the presence/absence of the time-consuming questionnaire), it is possible that some unidentified effect reflecting location influenced this finding.

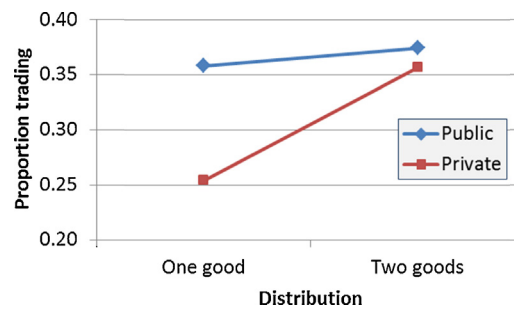


Fig. 1. Trading rate as a function of RESPONSE MODE (public response by raising hand or private by using a form) and DISTRIBUTION (one or two goods endowed among subjects in the room). Trading rates computed by averaging across all individual subjects in the respective conditions ($N = 1285$).

Table 4
Comparison of preference ratings with trading decisions.

Rating point difference	Number of subjects	Consistency of trading decision with preferences			Decision of subjects deciding contrary to preferences	
		Number consistent	Number contrary	Percent consistent	Kept	Traded
0	34 ^a					
1	60	38	22	63	17	5
2	41	30	11	73	7	4
3	30	23	7	77	6	1
4	24	18	6	75	4	2
5	11	9	2	82	2	0
6	10	9	1	90	1	0
Total	210	127	49	72	37	12

^a Of the indifferent subjects, 7 (21%) traded and 27 (79%) did not.

The first of the two significant interactions, DISTRIBUTION by RESPONSE MODE, has a positive sign. This interaction indicates that the difference in trading rate when using a public (raising hands) as opposed to a private (using a form) RESPONSE MODE diminishes when two goods, rather than just one, were distributed among subjects in the room (Fig. 1). We note that herding would not affect private responses and is less likely when two goods (as opposed to just one) are distributed among the assembled subjects because with two goods it is more difficult to quickly receive a clear signal about the preferences of other subjects in the room. Thus, we would expect herding to principally play a role in the Public, One Good case, which is what we see in the results. Of course, herding could operate in either direction, leading subjects to trade or not to trade. For the result obtained here to have been influenced by herding, the effect of herding in the Public, One Good case would be that herding led to more trading than would otherwise occur.

Interpreting the GROUP SIZE by RESPONSE MODE interaction is more challenging. The simple effect for group size is negative ($\beta = -0.1397$), whereas the interaction is positive ($\beta = 0.1140$). Interpreting the two coefficients jointly indicates that when subjects respond privately the effect of group size on trading is negligible ($\beta_{\text{GROUP SIZE|PRIVATE TRADING}} = -0.1397 + 0.114 = -0.0257$). However, for public trading, when the response mode term equals zero, there is a relatively large negative effect of group size on trading ($\beta_{\text{GROUP SIZE|PUBLIC TRADING}} = -0.1397$). This suggests first that trading is much more sensitive to group size when trading is public, and second that when trading is public, trading is more likely when group size is small. One implication of this interpretation is that when subjects respond publically, peer or herd effects (e.g., pressure to trade) are more intense with smaller groups than with larger groups.

In contrast to the earlier experiments, in the last experiment (experiment 11, Table 2) all subjects had both goods close at hand when deciding whether to keep the endowed good or trade it for the alternate one (in all other experiments the alternate goods were with the experimenter at the front of the room when the keep/trade decision was made). This procedural alteration eliminated any difference in transaction cost between keeping and trading the endowed good. The alteration resulted in an increase in trading (44% traded) and no exchange asymmetry ($p = 0.45$). A remaining question is whether the alteration also lessened the sense of endowment, a topic addressed in Section 4.

Returning to experiments 9 and 10 (Table 2), we now examine the correspondence of subjects' trading decisions to their earlier preference ratings. The results from these two experiments are combined, as the results from separate analyses are very similar. A total of 210 of the 213 subjects completed the preference task. It seems reasonable to posit that subjects who rated the two goods identically did not show a clear preference for one good over the other. In fact, 34 subjects (16%) rated the two target goods identically (Table 4), and 29%, 20%, 14%, 11%, 5%, and 5% of the subjects gave ratings that differed by one, two, three, four, five, and six points, respectively. Thus, strong preferences for one good over the other were relatively uncommon.

As might be expected given the number of subjects indicating small preference differences between the two goods, a substantial number of subjects later decided contrary to their expressed preferences. Overall, 49 (28%) of the 176 subjects who expressed a difference in preference decided on a good contrary to that difference. Interestingly, not all contrary decisions were at the low end of the preference difference scale, although the percent of contrary decisions dropped as preference difference increased, from 37% (22 of 60) at a rating difference of 1 to 10% (1 of 10) at a rating difference of 6 (Table 4).

Also of interest is whether subjects who decided contrary to their prior preferences kept or traded the endowed good. As seen in Table 4, keeping a good contrary to prior preferences was consistently more common than trading contrary. Thirty-seven subjects (21% of the 176 subjects with a preference rating difference) kept the endowed good contrary to their prior preferences but only 12 (7%) traded contrary; the difference in proportions is highly significant (binomial test, $p < 0.001$). Thus, subjects were three times more likely to keep their good contrary to prior preferences than to trade contrary. Further, of the 34 subjects whose ratings indicated indifference, only 7 (21%) traded their endowed good. Thus, indifferent subjects were four times more likely to keep their good than to trade it.

4. Discussion

4.1. Effect of experimental procedures

The extreme lack of trade observed in Knetsch's (1989, 1992) two early experiments has not been replicated. Most of the more recent experiments occurred in more controlled settings than those first used by Knetsch, which took place in classrooms among students who regularly met for class. It is possible that the classroom setting used by Knetsch discouraged trading. For example, herding (in this case leading to a lack of trading) may be more likely if subjects know each other. However, this is mere conjecture.

Perusal of Tables 1 and 2 suggests that the exchange asymmetry is a remarkably robust phenomenon. Although newer experiments have not reproduced the low trading rate of Knetsch's original experiments, a significant asymmetry nevertheless has resulted across several experimental manipulations including (1) employing various different pairs of goods; (2) using scripts that carefully avoid the suggestion that one good is preferred to the other; (3) making the keep/trade decision private; (4) making it clear that either of the goods could have been endowed to a given subject; (5) leaving both goods with the experimenter when subjects made their keep/trade decision; and (6) allowing little time to pass between endowment of the good and presentation of the opportunity to trade.

In contrast to the results of the bulk of studies, a significant exchange asymmetry was not found under three sets of conditions: when the subjects have ample trading experience or receive utility from the act of trading; when endowment is less clearly established such that the decision presented to subjects could be perceived as a choice between items; and when the exchange decisions were not final.

A choice between goods is very different from a decision of whether or not to trade a good one owns for one owned by someone else. Some experimental procedures may leave the nature of the subject's decision somewhat ambiguous. For example, consider the situation where both items, say a mug and a pen, are placed in front of the subject for inspection, the mug is then randomly selected and given to the subject ("the mug is yours"), and the subject is then asked whether he or she would like to trade the mug for the pen. To what extent does leaving both items with the subject and then randomly selecting one as belonging to the subject dull the sense of ownership? Or, to alter the procedure slightly, suppose the subject is asked, as in Plott and Zeiler (2007) "full set of controls", what good he or she would rather keep. To what extent does such a question suggest a choice between goods rather than a decision about trading? Similarly, in our experiment 11, to what extent does telling subjects that there were enough of each item so that each subject could "have the item they want" leave the impression that subjects are to choose, despite the fact that they were subsequently instructed to decide whether to keep or trade their item? We assert that the way the items are presented and the language used to describe the endowment and the decision to be made can all affect the *degree* to which subjects perceive they own an item and the *degree* to which they perceive their decision as involving trading, as opposed to choosing. One may imagine a continuum from the condition where endowment is very clear and trade is plainly the issue to the condition where endowment is not clearly felt and the decision to be made seems more like a choice. In other words, we are positing a continuum from where a subject's reference point is that of an owner to where the reference point is that of a chooser. The existence of a continuum, influenced by various aspects of the experimental procedure, would complicate the interpretation of the results of some experiments.

The effect on the trading rate of using a public process for subjects to record their trading decision, as opposed to a private process, is difficult to predict given the possibility of herding, which can conceivably lead subjects in either direction. Indeed, in the literature there is some suggestion of herds leading subjects not to trade (Knetsch, 1989, 1992) and to trade (Plott and Zeiler, 2007, "standard procedure"). Overall, the results from our study suggest that herd effects increased trading, but this need not occur in other studies. Of course, both herd effects and the group size effect can be avoided by keeping the trading decision private.

4.2. Adding preference indifference and imprecision to the list of possible causes of the exchange asymmetry

The observation that most subjects do not trade in experiments that clearly endow a good, ask for a keep/trade decision, and do not use special samples of subjects provides, by itself, little indication of the reason for the lack of trade. At least

four explanations have previously been offered. The first, proposed by [Knetsch \(1989\)](#) and based on [Kahneman and Tversky \(1979\)](#), is loss aversion, the notion that giving something up is of greater personal consequence than is obtaining it. This explanation turns on the importance of the reference point; once a person is endowed with an item, his or her reference point is that of an owner, which, it is argued, is quite different from that of a non-owner. Rather than dispassionately comparing two items (the one owned and the one that is offered in trade), people are thought to favor the owned item.

The second explanation, related to but formally separate from loss aversion, has been offered by [Bordalo et al. \(2012\)](#), who posit that there is a good feeling experienced in receiving the endowed good that is associated with the increase in utility that the endowment brings. There is no similar good feeling when an alternate good is offered in trade, such that without an otherwise strong preference for the alternate good the feeling associated with the endowed good, which is missing for the alternate good, determines the outcome. As opposed to loss aversion, this explanation might be called gain attraction.¹³

The third explanation is that while some people may like trading (e.g., List's traders and the 13% of [Engelmann and Hollard's \(2010\)](#) subjects who voluntarily traded most of the time), others, as Engelmann and Hollard propose, are biased against trading, and will avoid trade unless the gains from trade are sufficient to overcome their uneasiness. Although we are not convinced that Engelmann and Hollard's primary evidence in favor of this explanation, that of their "forced trade" experiment, did not result from an opposite bias, one in favor of trade, we expect that the results of their "voluntary trade" treatment, as well as of many other experiments, could plausibly be the result of a dislike of trading.

The fourth explanation is that subjects shy away from trading to avoid an anticipated feeling of regret ([Ratan, 2014](#)). The evidence for this explanation is somewhat unclear, because allowing subjects to later reverse their decision removes the commitment that a trade decision entails. Removing the need to commit may lessen the sense of endowment and allow subjects to approach the decision as they would a choice between goods.

All four of these explanations are plausible, and within a group of subjects they may each play a role, depending on the proclivities of the individuals. But this is not the whole story, because preference indifference may also contribute to the experimentally observed lack of trade. Although people may, in the standard consumer choice model, be envisioned as able to precisely trade off one good for another, such precision of preference is probably often lacking, as argued by [Mandler \(2004\)](#) and also as recognized, for example, in the contingent valuation literature (e.g., [Evans et al., 2003](#); [Ready et al., 1995](#)) and in more general studies of binary choice behavior (e.g., [Brown et al., 2008](#)).

We found that subjects who assigned the same preference rating to both goods tended strongly to keep their endowed good (79% did so). It seems plausible that such subjects would tend not to trade even if they were unaffected by the four tendencies mentioned above because of the small effort that trading entails. That is, such subjects' default position is most likely to keep the endowed good. However, we cannot know to what extent indifference or preference imprecision was accompanied by one or more of those four tendencies. Indeed, preference indifference and preference imprecision open the door wide to other reasons for not trading, such as the four noted above, for the lack of a clear ranking of the goods provides the subject with no help in deciding whether to trade or not.

Further, our preference rating data contain additional evidence of reluctance to trade. Of the 49 subjects who decided contrary to their prior expressed preferences, three-fourths of those contrary decisions were to keep—not to trade—the endowed good. The prevalence of keeping contrary to preferences could result partially from situations where the small difference in preference did not warrant the bother of trading, or where the difference in preference was too small to allow a clear ordering of the goods. However, it leaves open the possibility, especially for the subjects with a large preference difference ([Table 4](#)), that loss aversion, gain attraction, regret avoidance, or anti-trade bias—or some combination of these tendencies—was a substantial motivator.

5. Conclusion

The exchange asymmetry is a robust experimental finding that stands up to various alterations in experimental procedure designed to avoid herding effects, an over-emphasis of endowment, and other potential biasing factors. In most cases when ownership of the good is still clear and the decision subjects make is keep/trade and final, a significant asymmetry resulted. The cause of the asymmetry, however, is difficult to pin down. We conclude that loss aversion, gain attraction, regret avoidance, anti-trade bias, preference indifference, and preference imprecision may all play a role in causing the exchange asymmetry typically found in trading experiments. Furthermore, these different tendencies may interact, which greatly complicates the task of determining the relative importance of each. Doing so remains a challenge to the research community.

The trading experiments offer an interesting example of the progression of science. The dramatic early results, presented by [Knetsch \(1989, 1992\)](#), showed an extreme lack of trade. These early experiments were run under what would now be considered rather loose experimental controls. Since about 2000 roughly 30 more trading experiments have been reported, all under more carefully controlled circumstances and all finding a less extreme lack of trade. Progress has been made and much has been learned, yet it remains difficult to predict what the trading rate will be in a given experiment. What seemed at first like the simplest of experiments turns out to be surprisingly complex.

¹³ Perhaps related to gain attraction is the ownership effect—that ownership creates an association between the item and the self that leads to an increase in its perceived value ([Morewedge et al., 2009](#)).

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at <http://dx.doi.org/10.1016/j.jebo.2015.09.006>.

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