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Exchange Asymmetries Incorrectly Interpreted as Evidence of Endowment Effect Theory and Prospect Theory?

By CHARLES R. PLOTT AND KATHRYN ZEILER*

Jack L. Knetsch (1989) reported an important discovery. Using a simple experiment, he demonstrated the existence of asymmetries in exchange behavior. More precisely, when he followed a specific set of procedures to endow subjects with mugs and provided each subject an opportunity to exchange the endowed mug for a candy bar, he found that very few subjects gave up the endowed mug. By contrast, when he endowed a different group of subjects with candy bars using the same set of procedures, very few gave up the candy bar in exchange for a mug. While Knetsch, and many of those who followed him, interpreted the asymmetry as evidence of a special shape of preferences related to loss aversion (Knetsch 1989, 1277), our results demonstrate that observed asymmetries should be attributed instead to well-established alternative economic theories that influence choices through the experimental procedures employed.

Knetsch's initial intuitions have been expanded in a large and growing literature claiming that observed exchange asymmetries support "endowment effect theory"—an application of prospect theory positing that loss aversion associated with an endowment leads to asymmetries in valuations and exchange behavior. We use the term "endowment effect theory" rather than "endowment effect" to avoid the confusion over terminology that has emerged in the literature. From the beginning (i.e., Richard H. Thaler 1980), the label "endowment effect" has been used commonly to refer to observed symmetries. Using this label to refer to the observed phenomenon is problematic because it suggests a particular

theory as an explanation for asymmetries. To say that an observed phenomenon demonstrates an "endowment effect" does not simply denote that an asymmetry was observed; rather, use of the label implies that a very special form of preferences causes the asymmetry. We use "endowment effect theory" to distinguish the theoretical explanation from the observed phenomenon, which we refer to in this narrow context as an "exchange asymmetry." Specifically, endowment effect theory posits that individuals perceive parting with an endowed good as a loss that is greater than a potential gain from acquiring another good of otherwise equal value (Thaler 1980, 44). In turn, this interpretation generates support for a specific theory of choice behavior called prospect theory, of which loss aversion is a major component.¹

More broadly, endowment effect theory has been advanced to explain two completely different classes of phenomena that have seemed to defy explanation by any classical economic theory. First, some have argued that endowment effect theory explains observed gaps between "willingness to pay" (WTP) and "willingness to accept" (WTA). When asked to report the most one would be willing to pay for an item, the recorded amount tends to be lower than when the same person is asked to report the minimum amount he would be willing to accept to give up the item if owned. This observed phenomenon sparked dozens of articles over a period of 25 years. In recent years, however, several scholars have claimed that endowment effect theory explains observed gaps. In a review of the literature, Plott and Zeiler (2005) identify patterns consistent with the possible influence of experimental procedures. The experimental procedures employed were based on special methods to

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¹ Many have referred to endowment effect theory (or prospect theory) as the leading explanation for observed "endowment effects." See, e.g., Daphne R. Raban and Sheizaf Rafaeli (2003); John A. List (2006); Christine M. Jolls (forthcoming).

measure marginal rates of substitution. Plott and Zeiler (2005) posited an explanation centered on subject misconceptions stemming from the preference elicitation method, and ran additional experiments that implemented the union of commonly used controls to reduce misconceptions. When procedures were used to eliminate alternative explanations, the gap disappeared. The data support the conclusion that observed WTA-WTP gaps are caused by subject misconceptions resulting from the use of special mechanisms required to elicit valuations. The results suggest that endowment effect theory cannot explain data from that class of experiments.

Knetsch's (1989) results sparked a second body of evidence lending support to endowment effect theory. The simplicity of his experiments avoids rate of substitution measurement and thus avoids the complex elicitation procedures that Plott and Zeiler document as a possible source of subject misconceptions that lead to WTP-WTA gaps. For this reason, the Plott and Zeiler explanation of WTP-WTA gaps—subject misconceptions—does not appear to apply to the results derived from Knetsch's simple design.² Furthermore, the simplicity of Knetsch's design creates an impression that only endowment effect theory can account for observed asymmetries. Consequently, investigations of asymmetries have shifted away from the methods by which choices are elicited and toward a particular theory of preferences and conjectures about how the nature of different goods influences choices. The results from our study suggest that such diversions are premature.

The purpose of this study is to test an alternative explanation for observed asymmetries against endowment effect theory. Specifically,

² The procedures used in the Knetsch exchange experiment are completely different from the procedures used in WTP-WTA experiments. When subjects are asked to report WTP and WTA, the gap appears to be related to the use of the Becker-DeGroot-Marschak (1964) method of eliciting preferences (see Plott and Zeiler 2005). Misconceptions related to the method through which incentives are expressed and the role of the random mechanism demonstrated by Plott and Zeiler (2005) have little or nothing to do with the exchange procedure used in the Knetsch experiments. Thus, aside from the fact that experimental procedures are important in both cases, explanations for the WTP-WTA gap are unrelated to explanations for exchange asymmetries reported by Knetsch.

the alternative theory posits that asymmetries reflect the operation of classical preference theory together with well-established variations of classical theory operating through specific experimental procedures. For example, signaling theories suggest that experimenter choice of which good to endow might influence choices if subjects interpret the experimenter's choice as a signal of relative quality. Theories of other-regarding preferences suggest that asymmetries of choice unrelated to the value of the goods might occur if subjects feel obliged to avoid rejecting a good perceived as a gift from the experimenter. In addition, information aggregation and cascade theories suggest that the public nature of choice revelation allows for the possibility that choices depend on other subjects' choices. To control for these alternative explanations, we alter the procedures to rule out as many procedurally based theories as possible.

Given the nature of laboratory methodology, one might wonder whether our results are useful in analyzing behavior in field environments or whether the experimental investigation should be moved to a field environment. Laboratory procedures are well suited for our purposes and, in fact, are likely required, given the nature of the inquiry.³ Because endowment effect theory

³ We caution the reader to be aware of common misunderstandings regarding the purposes of experimental research that lead to unhelpful views about the usefulness of laboratory experiments. When choosing an experimental environment (e.g., lab versus field experiments), the purpose of the experiment becomes important. Two different purposes, parameter estimation and theory testing, call for different environments. If the question posed concerns measurement of a parameter, then the field could be the appropriate environment simply because the field might be the only environment in which the parameter confidently resides. For example, the field is appropriate if the intent is to measure the elasticity of market demand for a specified commodity or damages owed due to monopoly power or price-fixing schemes. A second and much different purpose of experiments, theory testing, must be recognized as a substantially different experimental activity. Relative to parameter measurement, theory testing is more naturally directed toward laboratory environments, especially in the case of very general theories, such as endowment effect theory. Theory testing requires that predictions of competing theories be clearly separated so the theory that best accounts for observed phenomena can be identified convincingly. Thus, depending on the theories, theory testing might require implementation of controls and replications under different sets of controls that are unimplementable in the field or have little resemblance to any field environment.

posits a particular preference relation, testing it requires controls and measurements difficult or impossible to implement in the field. Indeed, the results reported by Knetsch are noteworthy precisely because he demonstrated the phenomenon in the laboratory in the absence of confounding variables present in naturally occurring environments. By contrast, asymmetric choices observed in the field can result from a number of variables unrelated to a “kink” in the utility function at a reference point as postulated by endowment effect theory. In particular, value placed on an entitlement can be substantially influenced by the process through which entitlement was acquired. For example, one might hesitate to trade a trophy awarded at the end of a competition for a physically identical trophy. In addition, the ability to thoroughly inspect a good in one’s possession can be a source of information about its properties that leads to an adjustment in valuation. Some of these potential influences are impossible to observe in the field, and any attempt to control them almost certainly will introduce the same procedural difficulties inherent in the laboratory. The absence of proper controls and the alternative theories thereby injected render unconvincing inferences drawn from observed patterns of data.

This is not to say that field experiments are impossible or cannot be helpful. On the contrary, imaginative experiments on asymmetric choices have been conducted in partial field environments (e.g., List 2004). While these experiments provide insights about theory robustness, they do not serve well as tests of competing theories. The problem is that observed asymmetries cannot be attributed convincingly to endowment effect theory, given the existence of competing alternative theories related to uncontrolled field variables.⁴ Testing endowment effect theory

against classical preference theory in the field with all controls needed to make a convincing case, while at the same time maintaining an unperturbed field environment, would add complexities that likely would make it impossible to identify the theory at work due to the various extraneous forces triggered by entitlement creation. Fortunately, laboratory experimental procedures can be structured to test the predictions of competing theories more easily, given the ability to control the environment. By peeling away the previously unrecognized complexities and using several subtle variations of controls and replications that would be very difficult if not impossible to implement in the field, we are able to identify the theory that better explains observed exchange asymmetries.

The paper is organized as follows. Section I provides background, including a recap of the design of, and results from, exchange experiments,

losses (List 2004, 624). The results reported in the present paper suggest other possible explanations for observed asymmetries in his experiments. For example List’s procedures allow for experimenter involvement in the choice of the endowed good, which, according to our results, can signal relative quality. Thus, our results, together with List’s results, can be interpreted as suggesting that behavior was driven by the combination of experience with trading together with reactions to subtle signals of relative quality. If experienced subjects are more confident in their own abilities to assess quality (no matter what the good), they might rely less on signals to update their beliefs about the quality of goods. In addition, a host of alternative theories can be derived from the fact that inexperience is known to operate along several different channels (e.g., see Plott 1996), any one of which could lead to asymmetric choices. While List controlled several variables we identified as important, others left uncontrolled are potential explanations of observed choices.

One can formulate other candidate theories, based on various features of List’s experiments, to explain his observations. For example, to understand the actual effect of experience on the propensity to resist giving up endowments, it seems important to rule out selection effects. It could be that those with higher levels of confidence in their own ability to judge the quality of goods trade more relative to those who have lower confidence levels. Therefore, the act of trading might do little to change the perception of the good in the eyes of the owner. Conducting experiments in the field makes it difficult, if not impossible, to control for selection effects. In the lab, however, subjects can be randomly assigned to different groups, some of whom gain experience during the experiment and some of whom do not. In general, the lab offers virtually unlimited opportunities to control for a multitude of variables with the goal of identifying the theory that best explains the data.

⁴ For example, List (2003, 2004) finds that choice asymmetries differ across subject pools, and attributes this difference to variation in experience. In particular, he posits two theories, which depend on whether subjects are choosing between unique goods or “everyday consumable goods.” In the case of unique goods, he theorizes that “experienced subjects are more certain of their preferences” and thus “[l]esser-experienced agents may keep their endowed good simply to avoid making embarrassing mistakes” (List 2004, 617). In the case of everyday consumable goods, he suggests that experience makes the subject more likely to view traded endowments as opportunity costs rather than

and provides evidence of the proliferation of the interpretation of exchange asymmetries as support for endowment effect theory in both the economics and law literatures. The prevalence of this particular interpretation prompted our investigation. Section II discusses particular procedures that might allow the operation of a variety of preference theories, which could explain observed exchange asymmetries. Endowment effect theory predicts that the presence of an endowment will result in reluctance to exchange, and therefore we should observe an exchange asymmetry in each of our five treatments. On the other hand, if procedure-driven theories such as signaling theory, other-regarding preferences, and information aggregation and cascade theory explain observed asymmetries, then differences in experimental procedures will produce divergent results, even when the presence of an endowment is held constant across designs. Section III discusses the basic design features of our experiments and their results. We start with a set of baseline procedures, which reliably produces exchange asymmetries. We then assess the influence of a collection of controls without establishing the independent influence of any particular control. Additional experiments initiate an investigation of the effects and interdependencies of individual controls. The main result is that exchange asymmetries are not robust to changes in the procedures used to observe choices. Presence of an endowment is the one feature held constant across all experiments; therefore, our results reject the claim that endowment effect theory explains observed asymmetries. Finally, Part IV concludes that our results, in combination with results from Plott and Zeiler (2005), strongly suggest that classical preference theories finding influence through procedures, as opposed to the structure of preferences as postulated by prospect theory, explain observed exchange asymmetries. As such, our results call into question proposed legal policies that rely on interpretations of standard “endowment effect” experiment results grounded in endowment effect theory.

I. Background

Knetsch (1989) was the first to report results from exchange experiments to directly test the

reversibility of indifference curves. The experiments involved two groups of subjects. Each subject in the first group was given a mug and then asked to complete a questionnaire.⁵ Following the questionnaire, the subjects were shown candy bars and told that they could each have one in exchange for the mug. The subjects were instructed to hold up a piece of paper with the word “trade” written on it if the candy bar was preferred to the endowed mug. To reduce transaction costs, the experimenter immediately executed all desired trades by taking candy bars to the subjects wishing to exchange. Using a second group of subjects, the same experiment was performed, except that each subject in this group was endowed with a candy bar and given an option to trade it for a mug.

The results were quite striking. Of the 76 subjects endowed with mugs, 89 percent chose to keep the mug. The possibility that subjects simply preferred the mugs to the candy bars was ruled out by the fact that, of the 87 subjects endowed with candy bars, 90 percent chose to keep the endowed candy bar rather than exchange it for a mug. From these results, Knetsch concluded that subjects’ choices depended on their endowments. He suggested that the dramatic asymmetry resulted from subjects “[weighing] the loss of giving up their initial or reference entitlement far more heavily than the foregone gains of not obtaining the alternative entitlement” (Knetsch 1989, 1279). In other words, he interpreted the observed behavior as resulting from loss aversion.⁶

Other researchers have obtained similar results using Knetsch’s design. William T. Harbaugh, Kate Krause, and Lise Vesterlund (2001) used simple exchange experiments with children to test whether market experience affects reluctance to trade, and found that observed exchange asymmetries were independent of market experience levels. In addition,

⁵ The questionnaire supposedly was devised as an instrument to allow the subjects a chance to “experience” entitlement of the endowed goods for a period of time, on the theory that such time would allow them to understand they were entitled to the endowed good.

⁶ In similar experiments designed to test the assumption of transitivity of preferences, Knetsch (1992, 1995) obtained nearly identical results. Knetsch (1995) interprets these results as support for loss aversion and prospect theory as well.

List (2003, 2004) reports results from exchange experiments also designed to study whether market experience affects exchange asymmetries. He found that subjects with market experience tend not to display exchange asymmetries. For those without market experience, however, he observed a significant asymmetry in choices. Finally, Eric van Dijk and Daan van Knippenberg (1998) conducted exchange experiments to test the effects of the comparability of consumer goods on the reluctance to trade. Subjects were “rewarded” with a bottle of wine (half one kind and half another) in exchange for participating in the study. Subjects were then allowed to trade with one another. The results suggest that subjects were reluctant to trade in general and were more reluctant to trade when they perceived substantial differences between the endowed good and the alternate good.

The results from these simple exchange experiments have been interpreted by many as support for endowment effect theory, loss aversion, and/or prospect theory. Thaler (1980) interprets observed asymmetries from a variety of settings as resulting from prospect theory and loss aversion. In particular, Thaler posits that the endowment sets the reference point and that selling moves one in the direction of a loss and buying in the direction of a gain. To avoid losses, individuals state high values when asked how much they would be willing to accept to give up the endowment. Later Daniel Kahneman, Knetsch, and Thaler (1990) summarize the results reported by Knetsch (1989) and interpret the data as support for endowment effect theory (see also Kahneman, Knetsch, and Thaler 1991, and Thaler 1992). They then argue that the data support prospect theory.⁷

This interpretation of exchange experiment results has bled into legal scholarship as well. In separate experimental literature reviews, Russell Korobkin (2003) and Jeffrey J. Rachlinski and Forest Jourden (1998) refer to Knetsch’s (1989)

results as evidence of the existence of an endowment effect. Jeffrey E. Stake (1995, 2001), Ian Ayres and Fredrick E. Vars (1998), Henry E. Smith (2000), and M. Gregg Bloche (2003) also use Knetsch’s results as evidence of the existence of an endowment effect (i.e., evidence for endowment effect theory) and/or support for loss aversion and prospect theory.⁸ More importantly, a number of legal commentators advance policy prescriptions that are, in part, responses to implications of endowment effect theory.⁹

It is important to note that several theories have been posited to explain WTP-WTA gaps and exchange asymmetries, and these theories should not be confused with endowment effect theory. As described, endowment effect theory holds that the utility function includes a “kink,” which leads to differing evaluations of gains and losses. That is, the gain from acquiring a good is less than the loss from giving up that *same* good. Endowment effect theory should not be confused with theories about the potential role of ownership in the creation of features of goods that hold special values, such as sentimental value, emotional attachment, familiarity, etc. These alternative theories posit that such sources of value are reflected in offers to sell (see, e.g., Cass R. Sunstein 1986, 1151; Thomas F. Cotter 1997, 62; Cynthia R. Farina and Rachlinski 2002, 605; and Lee Anne Fennell 2005, fn. 107). That is, these theories describe ownership as being associated with phenomena that transform the features of goods so that the good to be given up is not the same as the good that was acquired. These form what one might call “attachment theory,” which focuses on features that give special value to specific goods that, except for such features, are identical to alternative goods. It is important to note that such explanations are not the subject of endowment

⁸ Other legal scholars have challenged the existence and stability of the endowment effect. See e.g., Christopher Curran and Paul H. Rubin (1995) and Jennifer Arlen (1998).

⁹ See, e.g., Rachlinski and Jourden (1998) (arguing that injunctive remedies, as opposed to damages remedies, impede trade because injunctions are perceived as endowments, and right holders are resistant to giving up rights to which they are entitled due to the endowment effect) and Stake (2001) (arguing that the theory of loss aversion “provides a strong framework” for maintaining legal rules related to adverse possession).

⁷ Werner Guth, Jan P. Krahnem, and Christian Rieck (1997), Gwendolyn C. Morrison (1998), Gretchen Chapman (1998), Nira Liberman et al. (1999), Leaf Van Boven, David Dunning, and George Loewenstein (2000), Stephen Wu (2001), Van Boven, Dunning, and Loewenstein (2003), Gail Tom (2004), and Steffen Huck, Georg Kirchsteiger, and Jorg Oechssler (2005) also invoke Knetsch’s results as evidence of endowment effect theory, loss aversion, and/or prospect theory.

effect theory, which theorizes that the good and all features of the good are the same when buying and selling, but the kink in the utility function at a reference point creates an asymmetry in choices due to loss aversion.

II. Experimental Procedures under Investigation

In this section, we elaborate on our conjectures about why the experimental procedures, as opposed to endowment effect theory, are responsible for observed asymmetries.

A. Method and Language Used to Endow Subjects

The specific method used to determine which good to endow and the language used to convey the method of determination might trigger procedure-driven explanations for observed asymmetries. Specifically, whether subjects are told that the endowed good is chosen through some random process or whether they perceive it as being chosen by the experimenter might influence subject choices over goods. Both signaling theory and theories of other-regarding preferences suggest the importance of this seemingly innocuous procedure.

While the method used to determine and explain the endowment might influence subject choices in myriad ways unrelated to loss aversion, we offer two specific possibilities. First, the language typically used to convey the nature of the endowment might cause subjects to perceive the endowed good as a gift from the experimenter. For example, imagine that the experimenter, after distributing the endowed good, “X,” announces, “X is yours. You own X. I am giving X to you.”¹⁰ Subjects might perceive this language as indicating that X is a gift from the experimenter, even though the experimenter might simply intend to convey that subjects now

own X. Given this perception, subjects might hesitate to trade the “gift” for the alternate good in deference to the experimenter. In other words, rather than choosing between a simple mug and a simple pen, each subject is choosing between a mug, which was a gift from the experimenter, and a pen.¹¹

Second, subjects might perceive the method used to determine the good chosen for the endowment as containing information. A subject who perceives the experimenter as exercising some judgment in determining which good to endow reasonably might view the experimenter as having special information about the relative value of the goods. The choice of the endowed good might serve as a signal about the relative value of the goods.¹² By engaging in particular actions, the experimenter might unintentionally reveal what subjects perceive as information about the relative value of the endowment. A subject who is indifferent between the endowed good and alternate good, or has not thought much about the relative value of the goods, might base choices on a perceived signal and do so instinctively.

To test these alternative explanations, we employed two different procedures for determining which good to endow. The first procedure focuses on language we suspect encourages the subjects to perceive the endowment as a gift. When the endowed goods were distributed, the experimenter announced, “I’m giving you X. It is a gift. You own it. It is yours.” The second procedure was designed to convey that the determination of the endowment resulted from a random process—a process through which the experimenter exercised no judgment. Subjects were told that the endowed good was determined by a coin flip before the start of the experiment.¹³ If exchange asymmetries result

¹¹ We thank Richard Lazarus for pointing this out as a potentially important variable. Gertrud M. Fremling and Richard A. Posner (1999, 27) focus on a need for subject anonymity to avoid any public display of choices and any associated public signal of personal traits or attitudes.

¹² That subjects gather information by observing the actions of others is well established (see, e.g., Plott 2000).

¹³ Specifically, subjects were told, “Before the start of the experiment, a coin was flipped to determine which good, the mug or the pen, to distribute. It came up heads, which means that we start with the mugs (or pens). The subjects in the other room will start with the pens (or mugs).”

¹⁰ While the exact language used by experimenters in previous experiments is often not reported, the description of the experiments suggests that subjects perceived the endowed good as a gift. For example, Knetsch (1989) indicates that “76 were given a coffee mug,” and that “87 participants in the second group were offered an opportunity to make the opposite trade of giving up a candy bar, which had been given to them initially ...” (emphasis added) (Knetsch 1989, 1278).

from the involvement of the experimenter in determining the endowment, we should observe an asymmetry when employing the first procedure but not the second. If, instead, loss aversion explains observed asymmetries, we should observe an asymmetry regardless of the procedure employed.

B. *Suggestions of Relative Value*

When conducting exchange experiments, experimenters have tended to incorporate procedures to establish a “reference point,” in this case, entitlement vis-à-vis the endowed good, prior to asking them to choose one of the goods. For example, subjects often are asked to fill out questionnaires while the endowed good is in front of them. Presumably, this is thought to provide subjects enough time to comprehend the endowed good as an entitlement.

A close examination of the procedures, however, reveals that experimenters, intent on emphasizing entitlement, possibly establish more than a reference point by inadvertently signaling that the endowed good is more valuable than the alternate good. For example, emphasizing ownership (e.g., “This good is yours. You own it. It belongs to you.”) in an attempt to ensure that subjects comprehend the entitlement might signal to the subjects that it is more valuable than the alternate good. Alternatively, this sort of language might be interpreted as a signal from the experimenter that the “correct” choice is to keep the endowed good.

To test whether this feature of the procedures might lead to exchange asymmetries, we altered the standard procedures (i.e., those likely employed by Knetsch (1989)) in two ways. First, when we distributed the endowments, we simply said, “These X’s are yours.” This is in contrast to emphasizing entitlement by saying, “I’m giving you X. It is a gift. You own it. It is yours.” Second, the forms subjects used to communicate their choices simply instructed, “Please circle the item you wish to take home with you.” The form included three choices: “X,” “Y,” and “I DON’T CARE.” This is in contrast to stating the choices as “I want to keep my X,” and “I want to trade my X for a Y.” If exchange asymmetries are caused by experimental procedures that suggest relative value, then modifying such procedures potentially will have an impact on

choices. On the other hand, if endowment effect theory explains exchange asymmetries, then these subtle changes in language should not affect choices, and asymmetries will persist.

C. *Location of Endowed Good at Time of Choice*

Where the endowed good is located at the time subjects make their choices might influence their decisions. In standard exchange experiments, the experimenter places the endowed good immediately in front of the subjects. As with other procedures analyzed previously, it could be that subjects perceive the position of the endowed good relative to the alternate good as a signal of relative value.

To control for this feature of the procedures, we altered the standard procedures in a simple way. After presenting subjects with the endowed good and allowing them to inspect it while completing the questionnaires, we replaced the endowed good with the alternate good (reminding subjects that they maintained entitlement to the endowed good even though it was not sitting in front of them). If exchange asymmetries are caused by signals of relative value produced by the location of the goods, then removing this sort of signal will influence exchange asymmetries (or result in reverse asymmetries). On the other hand, if endowment effect theory explains exchange asymmetries, then this change in procedures should not affect choices, and asymmetries will persist.

We constructed an additional treatment (referred to as the “transaction costs test”) to investigate a second conjecture about how the location of the endowed good might influence choices. Specifically, if a subject is indifferent between the endowed good and the alternate good, even very slight transaction costs (e.g., requiring a subject to raise his hand if he wishes to trade or to take any sort of action to initiate a trade such as the physical exchange of the endowment for the alternate good) might encourage him to keep the good within reach. That slight transaction costs can influence choices when subjects are near indifferent is well known (Plott and Smith 1978). Indeed, Chapman (1998) speculated that some of her results were due to reluctance to trade because subjects were “truly indifferent between the two

items and trading involved transaction costs.” Many experimenters (e.g., Knetsch 1989) have incorporated procedures to eliminate (or at least reduce) transaction costs in an attempt to rule them out as a direct cause of observed asymmetries (e.g., hand delivering the alternate good to subjects). Complete removal has proved difficult, however. Few, if any, have been able to conclude definitively that they are not an important driver of observed asymmetries.

We took a different approach to investigate whether transaction costs explain observed asymmetries. We used the standard procedures except that we asked subjects to make choices while the alternate good, rather than the endowed good, was within their reach.¹⁴ If subjects are indifferent between the goods, and transaction costs make subjects reluctant to trade, we should observe a “reverse” asymmetry when subjects make choices while the alternate good is in front of them. Put simply, transaction costs will result in subjects keeping the alternate good rather than trading back for the endowed good. On the other hand, if endowment effect theory accounts for observed asymmetries, we should observe subjects trading to retrieve their endowed goods.¹⁵

D. Public Revelation of Choices

Signals as sources of information about value can enter through yet another aspect of the procedures typically employed in studies that report exchange asymmetries. In most reported exchange experiments, including Knetsch’s (1989) experiment, subjects are asked to raise their hands if they wish to trade the endowed good for the alternate good. The resulting public revelation of choices might cause a “cascade” of sorts; those who are contemplating choosing one good might observe, as hands begin to go up or stay down, that most others seem to prefer one good over the other. Public revelation of choices might trigger cascades if subjects view other

subjects’ decisions as signals about the goods’ relative value. That subjects interpret the choices of others as signals of value is well established (Lisa R. Anderson and Charles A. Holt 1997; Angela A. Hung and Plott 2001; Martin Barner, Francesco Feri, and Plott 2005).

To control the influence of public revelation of choices on behavior, we used forms to allow subjects to report their choices privately. This ensures that subjects’ choices are independent of other subjects’ choices. If exchange asymmetries are due to the public nature of choice elicitation, then allowing subjects to communicate their decisions privately should eliminate them. On the other hand, if endowment effect theory explains asymmetries, then the elicitation mechanism should not significantly influence the results.

Section III reports results from five exchange experiments that implement various sets of the procedures discussed in Section II.

III. Experiment Designs and Results

The experimental design reflects the study’s primary purpose: to investigate the possibility that classical preference theory working through certain procedures, as opposed to endowment effect theory, accounts for observed exchange asymmetries. To test this conjecture, we alter the design in an attempt to rule out procedure-driven explanations. This is accomplished by a demonstration that asymmetries can be made to appear or disappear in a given subject pool by altering procedures while leaving the predictions of endowment effect theory intact. That is, the test calls for a demonstration of a change in behavior suggested by other well-established, competing theories when endowment theory predicts no change in behavior.

Multiple competing theories such as signaling or other-regarding preferences can work to influence choices through different procedures; therefore, we first test the alternative explanation by controlling simultaneously for all competing theories. If changes in procedures do not result in changes in choices, we can conclude that the data support endowment effect theory. On the other hand, if the results suggest that procedures influence choices, as is the case in the experiments reported here, we can begin to explore the impact of subsets of procedures to establish

¹⁴ See Section IIIB for specific procedures employed in the transaction cost treatment.

¹⁵ Of course, it could be that transaction costs and endowment effect theory, in combination with one another, make subjects reluctant to trade. If this is the case, then our results might provide evidence only about which effect overpowers the other in particular contexts.

robustness of the initial result and to initiate a deeper investigation of the channels through which the procedural influences work.

Section IIIA reports results from two treatments, which taken together demonstrate that controls affect choices. Endowment effect theory predicts we will observe an exchange asymmetry in both treatments studied given that entitlement is present in both. We observed an asymmetry, however, only in the treatment void of any experimental controls to rule out alternative explanations related to procedures. From this, we conclude that the classical preference theories, and not endowment effect theory, account for observed exchange asymmetries.

Section IIIB reports a set of results that tests robustness of our initial results and moves us a step toward understanding the effects of individual procedures and possible interactions between them. The analysis provides insights into not only which procedures (or combinations of procedures) seem to be driving differences in elicited choices, but also the alternative theories that might be at work. While endowment effect theory predicts asymmetries in all three treatments, we observe asymmetries in only one of the three treatments (when differences are measured at the 5 percent level). Table 1 summarizes the features of each design. Table 2 provides details regarding each session conducted, including the date, person(s) who ran the experiment, the subject pool, and the goods used. We also summarize the results for each session individually in Table 2. We drew our subjects from pools of Caltech students and Georgetown law students.

A. *Classical Preference Theories Explain Observed Exchange Asymmetries*

We conducted two treatments to test for whether classical preference theories affect choices through the collection of procedures used to endow subjects and elicit their choices. The baseline treatment contains none of the controls we identified as important. The second treatment incorporates a full set of controls meant to remove the influence of the alternative theories mentioned above. The data support the conjecture that classical preference theories, and not endowment effect theory, explain observed exchange asymmetries. Specifically,

we observed a significant exchange asymmetry when no controls are employed. When we control for all competing theories, however, the asymmetry disappears, contrary to what endowment effect theory would predict.

A Baseline.—We first attempted to replicate the asymmetric exchange phenomenon that has been widely reported in the literature. We refer to this treatment as the “baseline” because it includes all procedures the alternative explanations predict would lead to an observed asymmetry. We employ the baseline procedures to demonstrate that we can observe asymmetries in our subject pools. This set of procedures serves as a backdrop against which we assess alternative procedures.

We distributed mugs branded with university insignia to the subjects (Caltech mugs to Caltech students and Georgetown mugs to Georgetown students) and informed the subjects that they owned the mugs by announcing, “I’m giving you the mug. It is a gift. You own it. It is yours.”¹⁶ While the endowed goods were located in front of the subjects, we allowed them approximately three minutes to fill out questionnaires.¹⁷ After they completed the questionnaires, we allowed them to pass a few pens around the room so they could inspect them.¹⁸ Once each subject had an opportunity to inspect a pen, the experimenter instructed, “Please raise your hand if you want to keep the mug, the thing you own, rather than trading it for a pen.”¹⁹ The experimenter then walked around the room to make any necessary

¹⁶ We obtained the mugs from campus bookstores for roughly \$5 apiece. We removed the price tags from all goods prior to conducting the sessions.

¹⁷ Appendix A contains a typical questionnaire. Subjects’ answers to the questions were irrelevant to our study. We employed the questionnaires so that our results would be comparable to previously reported results. Each session lasted for less than ten minutes and subjects possessed the endowed good for about three to five minutes before making their choices. This is roughly the same amount of time provided in other experiments of this kind.

¹⁸ We obtained the pens, labeled with the name of the subjects’ university, from campus bookstores for approximately \$5 apiece. In one session, we used candy bars that were not labeled with the name of the university but cost roughly the same as the other goods.

¹⁹ The standard procedures ask subjects to raise their hands if they want to *trade* the endowed good for the alternate good. It seems reasonable to assume that a majority

TABLE 1—DESIGN FEATURES AND CONTINUUM OF RESULTS
(From no asymmetry to statistically significant asymmetry)

	Full set of procedural controls	Loss emphasis test	Standard procedures	Transaction costs test	Baseline procedures
Endowed good immediately in front of subject at time of choice	NO	YES	YES	NO	YES
Experimenter chose and gave OR randomly assigned which good to endow	RANDOMLY ASSIGNED	RANDOMLY ASSIGNED	EXPERIMENTER CHOSE AND GAVE	EXPERIMENTER CHOSE AND GAVE	EXPERIMENTER CHOSE AND GAVE
Experimenter purposefully and repeatedly emphasized ownership	NO	NO	YES	YES	YES
Choices made by public show of hands OR use of private forms	FORMS	FORMS	HANDS (TRADE ENDOWED GOOD)	FORMS	HANDS (KEEP ENDOWED GOOD)
(# mug owners, # pen owners)	(69, 70)	(44, 43)	(44, 52)	(53, 48)	(64, 65)
(# mug owners who chose mugs, # pen owners who chose mugs)	(37, 47)	(36, 32)	(34, 32)	(38, 24)	(54, 18)
(Percent mug owners who chose mugs, percent pen owners who chose mugs)	(54 percent, 67 percent) diff = -13 percent	(82 percent, 74 percent) diff = 8 percent	(77 percent, 62 percent) diff = 15 percent	(72 percent, 50 percent) diff = 22 percent	(84 percent, 28 percent) diff = 56 percent
Result [†]	$p = 0.94^{\ddagger}$	$p = 0.18$	$p = 0.06$	$p = 0.01$	$p = 0.00$

Notes. Overall, we observed a general preference for the mug. Of the 618 subjects that participated (including pilots), 398 (or 64 percent) chose mugs. This is statistically significantly greater than 50 percent ($p = 0.00$). This general mug preference, however, does not affect our results as we measured asymmetries by comparing the percentage of mug owners who chose mugs and the percentage of alternate good owners who chose mugs. This measurement controls for the overall mug preference.

[†] Results are from two-sample tests of equality of proportions (null hypothesis: proportions are equal; alternate hypothesis: percent mug owners who chose mugs > percent of pen owners who chose mugs).

[‡] If we use an alternate hypothesis of H_a : percent of mugs owners who chose mugs < percent of pen owners who chose mugs, the p value is equal to 0.06. This (weakly) supports a hypothesis that a “reverse” asymmetry exists.

exchanges. We conducted additional sessions using an identical design, except that subjects were endowed with pens instead of mugs.

We collected data from 129 Georgetown law students and Caltech students. We endowed 64 subjects with mugs and 65 with pens. Eighty-four percent of the mug owners chose mugs and 28 percent of the pen owners chose mugs. A two-sample, one-tailed test of equality of proportions

supports a rejection of the null hypothesis that the percentages are identical in favor of the alternative hypothesis that the percentage of mug owners who chose mugs is greater than the percentage of pen owners who chose mugs ($p = 0.00$). In other words, these data reveal a statistically significant exchange asymmetry.

Full Set of Controls.—We refer to the second treatment as the “full set of controls” because we designed the procedures to remove all avenues of procedural influence listed in Section II. This treatment tests for the existence of procedural

with raised hands is easier to detect than a majority that remains with hands down.

TABLE 2—SUMMARY OF SESSIONS

	Date	Experimenter	Subject pool	Number of mug owners / Number who chose mug	Number of alternative good* owners / Number who chose mug
Full set of procedural controls	Jul 14 04	Zeiler	Gtown JD	8 / 5 [1]** (63%)	9 / 5 [1] (56%)
	Jul 15 04	Zeiler	Gtown JD	6 / 4 [1] (67%)	7 / 4 [1] (57%)
	Jul 26 04	RAs	Gtown JD	10 / 6 [1] (60%)	5 / 2 [1] (40%)
	Jul 27 04	RAs	Gtown JD	5 / 4 [1] (80%)	5 / 4 [0] (80%)
	Aug 03 04	RAs	Gtown JD	9 / 1 [1] (11%)	18 / 12 [1] (67%)
	Sept 20 04	Zeiler	Gtown JD	17 / 8 [3] (47%)	14 / 11 [1] (79%)
	June 21 05	Zeiler and RA	Gtown JD	4 / 2 [1] (50%)	N/A
	June 23 05	Zeiler and RA	Gtown JD	5 / 2 [0] (40%)	8 / 6 [2] (75%)
	June 27 05	Zeiler and RA	Gtown JD	5 / 5 [0] (100%)	4 / 3 [1] (75%)
			TOTAL	69 / 37 [9] (54%)	70 / 47 [8] (67%)
Loss emphasis test	Mar 30/Apr 3 06	Zeiler and RAs	Gtown JD	8 / 5 (63%)	14 / 10 (71%)
	June 12 & 14 06	Zeiler and RA	Gtown JD	12 / 10 (83%)	15 / 12 (80%)
	June 13 & 15 06	Zeiler and RA	Gtown JD	24 / 21 (88%)	14 / 10 (71%)
			TOTAL	44 / 36 (82%)	43 / 32 (74%)
Standard procedures	Sept 01 04	Zeiler and RA	Gtown JD	15 / 12 (80%)	30 / 18 (60%)
	April 14 05	Zeiler and RA	Gtown JD	9 / 6 (67%)	11 / 9 (82%)
	June 30 05	Zeiler and RAs	Gtown JD	20 / 16 (80%)	11 / 5 (45%)
			TOTAL	44 / 34 (77%)	52 / 32 (62%)
Transaction costs test	Jun 4 03	Zeiler	Caltech UG	28 / 19 (68%)	28 / 14 (50%)
	Jun 8 and 10 03	Zeiler	Gtown JD	13 / 10 (77%)	10 / 5 (50%)
	Jun 25 04	Zeiler	Gtown JD	12 / 9 (75%)	10 / 5 (50%)
			TOTAL	53 / 38 (72%)	48 / 24 (50%)
Baseline procedures	Aug 2004	Plott and RA	Caltech	9 / 7 (78%)	8 / 2 (25%)
	Aug 18 04	Plott	Caltech	7 / 5 (71%)	N/A
	Sept 9 04	Zeiler and RA	Gtown JD	17 / 14 (82%)	17 / 3 (18%)
	April 26 05	Zeiler and RA	Gtown JD	8 / 7 (88%)	15 / 6 (40%)
	June 17 05	Zeiler and RAs	Gtown JD	8 / 8 (100%)	7 / 2 (29%)
	June 29 05	Zeiler and RA	Gtown JD	8 / 7 (88%)	8 / 2 (25%)
	July 10 05	Zeiler and RA	Gtown JD	N/A	10 / 3 (30%)
	July 11 05	Zeiler and RA	Gtown JD	7 / 6 (86%)	N/A
			TOTAL	64 / 54 (84%)	65 / 18 (28%)

* In each session except for the first session conducted on June 4, 2004, we used pens as the alternate good. During the first session, we used candy bars.

** The numbers enclosed in square brackets indicate the number of subjects who reported indifference between the two goods by circling "DON'T CARE" on the decision record (see Appendix C). The numbers in parentheses indicate the percentage of subjects who chose mugs.

influences even though the exact nature of the influence might not be apparent.

We began these sessions by informing the subjects that mugs and pens would be used during the experiment. Subjects were then told that a coin was flipped before the start of the experiment to determine which good, the mug or the pen, would be distributed first. We then distributed mugs to the subjects and announced, "These mugs are yours." Next, we asked the subjects to complete the questionnaires. After

the questionnaires were completed, we took the mugs from the subjects, placed them at the front of the room and distributed pens to the subjects. After providing time to inspect the pens, we asked each subject to decide which good to keep and to indicate the decision on a form.²⁰ The forms simply indicated the options

²⁰ See Appendix C for a sample of the forms used during these sessions. We gave subjects an opportunity to indicate

and did not reemphasize the fact that subjects owned one of the goods. Once each subject completed a form, the experimenter walked around the room to collect them and make any necessary exchanges. We conducted additional sessions using an identical design, except that subjects owned pens rather than mugs.

In Table 1 we report the features of, and results from, this treatment. We collected data from 139 Georgetown law students, 69 endowed with mugs and 70 endowed with pens. Fifty-four percent of the mug owners chose mugs and 67 percent of the pen owners chose mugs. A two-sample, one-tailed test of equality of proportions supports the hypothesis that the percentages are identical ($p = 0.94$).²¹ In other words, we observed no exchange asymmetry under this set of procedures. Indeed, we observe a (somewhat weak) reverse asymmetry.²²

These results, taken together, support the conjecture that exchange asymmetries result from classical preference theories working through the experimental procedures and cannot be explained by endowment effect theory or prospect theory. The next section reports results from experiments designed to begin an exploration of specific procedural effects and how procedures interact with one another.

B. Sets of Procedures and Their Interactions

Classical preference theories suggest several mechanisms through which any specific procedure might influence choices. Thus, we turn our investigation to whether specific procedures exhibit influence over choices. Using key procedures as cornerstones, we begin an investigation into the effects of sets of specific procedures

allowing the influence of classical preference theories. This section reports the design features of these treatments and their results. Recall that endowment effect theory predicts asymmetries in each treatment. By contrast, we observe an asymmetry in only one treatment (when measured at the 5 percent level).

Transaction Costs Test.—The transaction costs test was designed to explore whether transaction costs created by procedures might explain observed asymmetries. That small transaction costs can create asymmetries in choices when individuals are nearly indifferent was established in the 1970s (Plott and Smith 1978).

To test for the influence of transaction costs, we distributed mugs to the subjects and informed them that they owned the mugs. While the endowed good was located in front of them, subjects spent approximately three minutes filling out questionnaires. After they completed the questionnaires, we removed the endowed mugs and placed the mugs at the front of the room (reminding them that they still owned them) and passed around pens for their inspection. Each student then filled out a form to indicate whether he wanted to keep his mug or trade his mug for a pen (see Appendix B). Once the subjects completed the forms by choosing one of the options, the experimenter walked around the room to collect the forms and made any necessary exchanges. We conducted additional sessions using an identical design, except that subjects were endowed with pens instead of mugs.

We collected data from 101 Georgetown law students and Caltech students. We endowed 53 subjects with mugs and 48 with pens. Seventy-two percent of the mug owners chose mugs and 50 percent of the pen owners chose mugs. A two-sample, one-tailed test of equality of proportions supports a rejection of the null hypothesis that the percentages are identical in favor of the alternative hypothesis that the percentage of mug owners who chose mugs is greater than the percentage of pen owners who chose mugs ($p = 0.01$). In other words, these data reveal a statistically significant exchange asymmetry. This result suggests that any transaction costs

indifference between the goods by circling the option "DON'T CARE." None of the subjects asked which good would be received upon choosing this option. Each subject who chose "DON'T CARE" received the good in her possession at the time she completed the form. Only 17 of the 139 subjects reported indifference; therefore, the results from statistical tests of differences of proportions are virtually identical when we include and exclude these data.

²¹ We tested the null hypothesis of equality against an alternative hypothesis that the percentage of mug owners who chose mugs was greater than the percentage of pen owners who chose mugs.

²² When the alternative hypothesis is framed as "the percentage of pen owners who chose mugs is greater than the

percentage of mug owners who chose mugs," a two-sample test of equality of proportions results in a p -value of 0.06.

that might exist in the baseline do not account for the observed exchange asymmetry.

This result suggests that transaction costs do not drive observed exchange asymmetries. In addition, when combined with the results from the treatment employing the full set of controls, this result suggests that removing the influence of classical preference theories by modifying the standard procedures (e.g., removing the experimenter from the determination of the endowment, etc.) eliminated the exchange asymmetry under the full set of controls.²³

Standard Procedures.—We refer to this design as “standard” because of its close proximity to the procedures found in the literature. The procedures differ from the baseline only in terms of the meaning of raised hands during the choice phase of the experiment. In the baseline procedures, a subject’s raised hand signals the desire to keep the endowed good. In the standard procedures, a raised hand signals the desire to trade the endowed good for the alternate good.²⁴ This natural robustness check might also provide insight into what sorts of signals subjects send to one another when they make public choices, and how these signals interact with other procedures. Our suspicion was that the baseline procedures would produce stronger cascades and exacerbate the asymmetry. If the collection of procedures encourages subjects to trade, as suggested by the alternative theories, then many subjects will raise their hands to trade, leading to cascades of other subjects raising their hands to trade.

In Table 1 we report the features of, and results from, this treatment. We collected data from 96 Georgetown law students. We endowed 44 subjects with mugs and 52 with pens. Seventy-seven percent of the mug owners chose mugs and 62 percent of the pen owners chose

mugs. A two-sample, one-tailed test of equality of proportions (somewhat weakly) supports a rejection of the null hypothesis that the percentages are identical in favor of the alternative hypothesis that the percentage of mug owners who chose mugs is greater than the percentage of pen owners who chose pens ($p = 0.06$). In other words, we observed a (somewhat weak) exchange asymmetry. The fact that the asymmetry observed under the standard procedures is not as large as that under the baseline procedures suggests that the existence of subtle signals might have an effect, but the effect is not sufficient to eliminate the asymmetry.²⁵

Loss Emphasis.—This treatment addresses critiques of the “full set of procedural controls” treatment design and serves as another test of the robustness of our general results. Some have argued that the procedures embodied in the full set of controls might reduce the chance that subjects understand they actually are entitled to one of the goods, and therefore the full set of

²⁵ In a separate test, we investigated whether the order in which the experimenter presents the goods to the subjects matters (see Harrison et al. (2005) for a general discussion of how order effects tend to confound results). We presented subjects with the alternate good before presenting them with the endowed good. During these sessions, we distributed mugs to the subjects and informed them that the mugs did not belong to them but that they should inspect them because we would give them an opportunity to obtain one later in the session. We then asked the subjects to complete the questionnaires. After the questionnaires were completed, we removed the mugs, placing them at the front of the room, and distributed pens to the subjects. Once each subject possessed a pen, the experimenter announced, “These pens are yours.” Subjects then completed forms to indicate whether they wanted to keep the endowed good or trade it for the alternate good. The experimenter walked around the room to collect the forms and make any necessary exchanges. We conducted additional sessions using an identical design, except that the sessions started with pens rather than mugs.

We collected data from 50 Georgetown law students. We endowed 17 subjects with mugs and 33 with pens. Seventy-one percent of the mug owners chose mugs and 64 percent of the pen owners chose mugs. The proportions are not statistically significantly different ($p = 0.31$; power = 0.0675). This result differs from those obtained using the standard procedures; but the treatment changed both the public nature of the choice and the order of presentation. Since the effect is small relative to the “standard treatment,” but large relative to baseline, and because multiple variables are at work, we chose not to pursue this line of investigation. The question of order of presentation remains open.

²³ It also suggests that the results obtained when implementing the full set of controls are not driven by the fact that the endowed good is not in front of the subjects when they are choosing between the two goods.

²⁴ Of all the designs we study, the “standard procedures” design seems most similar to Knetsch’s (1989) procedures (hence the label). Knetsch (1989) does not include the exact language used to convey entitlement to the subjects. Therefore, our language (i.e., “I’m giving you the mug. It is a gift. You own it. It is yours.”) might not exactly match the language he used.

controls cannot test the influence of reference points on choices. In particular, some have suggested that lack of possession of the endowed good at the time of choice and the brief mention of entitlement (e.g., “The mug is yours.”) are insufficient to ensure that subjects understand they are entitled to the endowed good. In effect, they argue that specific features of the full set of controls make endowment effect theory irrelevant because, if entitlement is absent, then no loss is contemplated when choosing between the goods.

To address this conjecture we altered the “full set of controls” treatment in three ways: (a) when endowing one of the goods, rather than saying, “The mug is yours,” we said, “The mug is yours. You own it.”; (b) the subjects made choices while in possession of the endowed good; and (c) the forms reiterated entitlement to the endowed good (see Appendix B).²⁶ We altered the design in these ways to determine whether a lack of understanding of entitlement caused the null result in the “full set of controls” treatment, while being careful to eliminate possible experimenter signals of value.

In Table 1 we report the features of, and results from, this treatment. We collected data from 87 Georgetown law students. We endowed 44 subjects with mugs and 43 with pens. Eighty-two percent of the mug owners chose mugs and 74 percent of the pen owners chose mugs. A two-sample, one-tailed test of equality of proportions supports acceptance of the null hypothesis that the percentages are identical ($p = 0.18$). In other words, we did not observe an exchange asymmetry. That the result from this treatment is consistent with the result from the “full set of controls” treatment suggests that a lack of understanding of entitlement does not explain the absence of an exchange asymmetry under the full set of controls.

In sum, it is important to stress that, while procedures clearly have a strong influence on the asymmetry of choices, any inferences about how particular features of the procedures affect choices are only conjectures at this stage. Given

our results, we suspect that interaction effects exist between various procedures and the information contained in Table 1 is insufficient to understand what role each of them plays in influencing choices. One thing is clear, however: our results demonstrate that endowment effect theory cannot explain observed asymmetries.

IV. Discussion and Conclusions

Many have advanced observed exchange asymmetries as support for endowment effect theory and underlying prospect theory. These are very general theories about the nature of preferences, which, if accepted, have implications for applied economics in complex field settings. While we do challenge the general accuracy of endowment effect theory, we do not challenge prospect theory, which has been explored in different experiments. More specifically, we challenge the interpretation of exchange asymmetries as providing empirical support for either endowment effect theory or prospect theory. Knetsch’s (1989) discovery of asymmetries in exchange experiments is interesting and certainly should not be dismissed, but our results suggest that his discovery cannot be explained by endowment effect theory. The experiments we report, along with those reported by others, suggest that classical preference theories influencing choices through procedures used in the experiments account for the patterns of observed choices.

Well-established classical preference theories suggest the mechanisms through which the procedures influence choices to produce observed exchange asymmetries. Intuitions derived from them are based on two broad classes of variables. First, other-regarding preferences, specifically the regard subjects have for the experimenter, might influence choices in exchange experiments. These preferences are known to operate during experiments and, in the case of exchange experiments, could be activated by aspects of procedures that might ingratiate the subject vis-à-vis the experimenter (e.g., gift language). This theory is related to theories that hold that the context in which entitlement is granted generates an independent source of value and that value accounts for observed exchange asymmetries as opposed to loss aversion.

²⁶ In most sessions, the forms said, “I want to keep *my* mug,” and “I want to trade *my* mug for a pen.” In the sessions conducted on March 30, 2006, and April 3, 2006, however, we used forms that said, “I want to keep *the* mug,” and “I want to trade *the* mug for a pen.”

Second, signals built into the experimenter's actions and language choices (or the actions of others) in combination with the possibility of asymmetric information about the relative value of the goods might influence choices. Subjects might interpret experimenter emphasis on entitlement as indicating that the experimenter has information about the relative value of the goods that the subjects do not. Similarly, the choices of others might be interpreted as reflections of private information about the relative value of the goods. Such influences are well known in the information aggregation and cascade literature.

It is important to note that our results demonstrate much more than the possibility that arbitrary changes in procedures might influence subject choices. Classical preference theory is consistent with the pattern of results. The experimental economics literature has demonstrated that other-regarding preferences and various signals about the relative value of the goods can influence choices. By implementing procedures that collectively control for the various routes of influence classical preference theories suggest, a substantial difference in choice behavior is observed under the full set of controls (when we neutralize these influences) and the treatments implementing the standard and baseline procedures (when we do not control for these influences). Our results remain strong in robustness checks, which attempt to ensure that subjects understand their entitlement to the endowed goods while avoiding signals that might act as sources of information about relative value. The results from the transaction costs treatment suggest that the transaction costs variable does not explain a significant portion of the variation observed across treatments, even when classical preference theory suggests it might. We hasten to add that our understanding of procedural influences is incomplete but our general conclusion stands: endowment effect theory does not seem to explain observed exchange asymmetries.

While gaps occasionally are observed, our results demonstrate that observed gaps are inconsistent with endowment effect theory. Either no "endowment effect" of the sort predicted by prospect theory exists or the effect is sufficiently weak that other phenomena easily swamp it. If asymmetries of observed choices

are to be interpreted as strong evidence of the principles that operate when individuals make choices, then such asymmetries should be robust against subtle features of procedures and variations in the set of procedures used to conduct experiments.

In closing, we call the reader's attention to a common interpretation mistake. It is tempting to interpret our results as demonstrating that the endowment effect is context-dependent. Specifically, some have argued that our results demonstrate that, in some contexts, individuals operate under utility functions with shapes suggested by prospect theory, and, in others, they do not. Previous claims of this sort abound in the literature and represent a tendency to reconcile results that do not support endowment effect theory by pointing to various contextual features.²⁷ This "context-dependent" interpretation places a very awkward strain on endowment effect theory in an effort to find support for it. Consider a typical example of context-created value: a good received as a gift from a friend. Value is created by the context because, as a gift from a special individual, it is unique and thus valued differently from otherwise identical goods. Such values reflect special attributes of the good created by the context, and might be consistent with some form of attachment theory, but are unrelated to loss aversion, a key property of endowment effect theory. The strain results from the fact that one does not need to resort to a "kink" in a utility function to explain observed asymmetries in such contexts. Furthermore, such context-dependent theories destroy the generality and robustness of the theory, which is the source of power and relevance of any theory in applied work. Attempting to add to the theory various features of the context renders it impossible to reject.

²⁷ See, e.g., Jennifer Arlen, Matthew L. Spitzer, and Eric L. Talley (2002, 4) ("Existing evidence on the endowment effect suggests that it is pronounced in certain circumstances yet muted (or absent) in others. Much like other deviations from rational choice, the existence and magnitude of the endowment effect is context dependent."); Korobkin (2003, 1235) ("The broad array of experiments testing the endowment effect demonstrates that the effect is robust across different types of endowments, but it is not universally apparent nor equally striking across contexts.")

APPENDIX A

A Sample Questionnaire

1. The current U.S. Secretary of State is _____ .
How likely is it that your answer is correct? _____ percent
(enter likelihood between 0 percent and 100 percent)
2. Bucharest is the capital city of _____ .
How likely is it that your answer is correct? _____ percent
(enter likelihood between 0 percent and 100 percent)
3. The author of the novel *The Phantom of the Opera* is _____ .
How likely is it that your answer is correct? _____ percent
(enter likelihood between 0 percent and 100 percent)
4. The actor _____ plays the U.S. President on the TV series *The West Wing*.
How likely is it that your answer is correct? _____ percent
(enter likelihood between 0 percent and 100 percent)
5. What does the acronym NASA stand for? _____
How likely is it that your answer is correct? _____ percent
(enter likelihood between 0 percent and 100 percent)

APPENDIX B

Sample Transaction Record for Transaction Costs Test

Transaction Record*

- I want to keep my mug.
 I want to trade my mug for a pen.

*This form was used in sessions in which we endowed subjects with mugs. Subjects were asked to check one box. Similar forms, with obvious modification, were used in sessions in which we endowed subjects with pens.

APPENDIX C

Sample Decision Record for Sessions Using Full Set of Procedural Controls

Decision Record

Please circle the item you wish to take home with you.

MUG PEN

DON'T CARE

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