American Economic Review 101 (April 2011): 1012–1028
http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.2.1012

The Willingness to Pay—Willingness to Accept Gap, the "Endowment Effect," Subject Misconceptions, and Experimental Procedures for Eliciting Valuations: Reply

By Charles R. Plott and Kathryn Zeiler*

The purpose of Plott and Zeiler (2005)—henceforth, PZ—was to investigate whether previously published experiments using consumption goods such as mugs and candy bars to measure gaps between willingness to pay (WTP) and willingness-to-accept (WTA) support endowment effect theory (EET). Our results demonstrate that the gap for commodities can be turned on and off by implementing procedures designed to control for subject misconceptions about the value elicitation procedures.

Following experiments traditionally used to demonstrate the endowment effect, we used mug valuations to test EET. We used lottery rounds only to provide our subjects with paid practice using the value elicitation device prior to employing that device to elicit subjects' mug valuations. In a footnote, we report evidence of contamination in the lottery data that rendered it inappropriate for our purposes (PZ 2005, fn. 15). Our footnote summarized the details of misconceptions reported in a data supplement provided to all who request our lottery data. The supplement was not referenced in our paper, so we make it available as an online Appendix to our Reply.[1] Andrea Isoni, Graham Loomes, and Robert Sugden (2011)—henceforth, ILS—use experimental procedures similar to ours and observe, just as we did, no gap in mug valuations.

ILS claim that our 2005 paper is misleading and has misled researchers. They are concerned that our paper produces, and has been interpreted as producing, a set of procedures sufficient to remove all gaps, including gaps in lotteries. To justify their concern they focus on the wording of our abstract and overlook the context of paragraphs from which they quote sentences to support their thesis. The result is what we consider to be a misleading picture of the content of our paper and the facts that we report, namely that mug gaps disappear after we implement controls for misconceptions and that none of our data provides support for EET. We want to emphasize that our focus was on EET and not on more general theories of preference formation, reference effects and decision processes that have emerged in the literature more recently and might explain our results.[2] In Section I, we demonstrate that we did not make broad claims about our procedures.

* Plott: Division of the Humanities and Social Sciences, California Institute of Technology, M/C 228-77, Pasadena, CA 91125 (e-mail: cplott@hss.caltech.edu); Zeiler: Law Center, Georgetown University, 600 New Jersey Ave. NW, Washington, DC 20001 (e-mail: zeiler@law.georgetown.edu). Thanks to workshop participants at Temple University and University of Texas-Austin Law Schools and the Georgetown University McDonough School of Business.

[1] The online Appendix can be downloaded at *http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.2.1012*.

[2] We draw the reader's attention to two important details related to what has been established. (i) EET is an application of more general theories (prospect theory and reference-dependence theory) crafted to explain a specific, experimentally defined phenomenon dealing with commodities. According to the theoretical literature at the time, EET applies only if the reference point is some sure amount, as ILS note (ILS, fn. 5). The literature of the time can be interpreted as claiming that EET makes no prediction when the reference point is set by a lottery. Although

ILS make three additional claims. First, they claim that EET "appears to be a loosely related family of theories …." (ILS 2011, p. 994). From their immediately subsequent discussion, one might infer that testing EET is a futile endeavor. In Section II we demonstrate that EET is well specified and makes clear predictions given our experimental environment. We also emphasize that ILS's and our mug data do not support EET. When controls for misconceptions about the elicitation device are implemented in mug experiments, the valuation gap disappears.

Second, rather than focus as we do on EET, ILS substitute their own, much different focus: to determine whether our procedures generally eliminate gaps. ILS recognize that our procedures contaminated the lottery data and attempt to adjust our procedures to control for *all* contamination that concerned us. Their understanding of the sources of contamination, however, is much narrower than ours. In Section III, we demonstrate that, despite their attempt to implement a complete set of controls, ILS's lottery data are contaminated in the ways we described in our paper (PZ 2005, fn. 15) and in our data supplement. A closer look reveals that ILS's lottery data contain obvious footprints of contamination and that they were too quick to dismiss our concerns as a mere "ex post conjecture" (ILS 2011, p. 1009). Misconceptions are evident in the substantial number of irrational valuations reported by subjects. Furthermore, misconceptions about random devices appear to cause a shifting in subject beliefs about lottery outcomes that is correlated with selling (WTA) and buying (WTP) roles. This suggests that subjects perceive the lottery for which WTA is elicited as different from the lottery for which WTP is elicited. This phenomenon can be interpreted as a failure to measure a gap arising from the valuation of the "same good," the only type of gap of interest for testing EET, which depends on the shape of a preference relation.

Third, despite ILS's claim that they "are not concerned with testing any particular theoretical account of WTP-WTA gaps," (ILS 2011, p. 995) they take up a number of explanations in their conclusion section. In Section IV we discuss the two most prominent alternative conjectures ILS offer to explain our mug data. The first—the house money conjecture—cannot be tested properly using ILS's data and is inconsistent with patterns of data found in the literature. The second ILS theory is grounded in the impact of procedures, including emphasis on the role of buying and selling and placement of the good, and focuses on how such procedures produce perceptions of loss. This theory is problematic because the observations do not rule out enhancement effects. Section V offers concluding remarks and makes clear that our analysis of the lottery data is consistent with our observations related to the nature of different goods (PZ 2005, p. 531).

Daniel Kahneman, Jack L. Knetsch, and Richard H. Thaler (1990, p. 1343)—henceforth, KKT—briefly mention "risky prospects" in their discussion section, Amos Tversky and Kahneman (1991), which KKT (1990) cite as a foundational model of loss aversion, state that their theory applies to riskless choice. After we produced our results, others developed theories that might be applied to explain gaps in lottery environments (Sugden 2003; Botond Kőszegi and Matthew Rabin 2006). Our experiments, however, were not designed to test these alternative theories, which were not available at the time we were writing, nor do our experiments test theories of decision processes such as the theory posited by Gerd Gigerenzer and Peter M. Todd (2008).

(ii) We identified a set of procedures that remove gaps in the specific mug experiments cited in the literature as direct confirmations of EET, but there may well be other procedures that remove gaps or influence their existence.

I. We Did Not Claim that Our Procedures Will Eliminate Gaps in All Contexts and that Gaps were Eliminated in Lottery Rounds

The main claim of PZ (2005) is that the mug experiments that are widely advertised as supporting EET do not. When procedures are implemented to control for subject misconceptions about the elicitation device, no WTA-WTP gap is observed, contrary to the prediction of EET. ILS interpret our paper as having made additional claims, but the text of our paper and our data supplement, which we sent to ILS upon request early on,[3] both make clear that these additional claims cannot be attributed to us. The supplement clearly stated that we observed gaps in the lottery data. In addition, it, together with the text of our paper, identified footprints of misconceptions, which we can see now provide the key to a deeper understanding of what the lottery data can teach us about valuation elicitation.

ILS's statement of their perceived problem with our study mischaracterizes the nature of our procedures. In their abstract, they assert " … other data from the same study, not published in that paper, exhibit a significant and persistent disparity when the same experimental procedures are applied to lotteries" (ILS 2011, online abstract). This statement is inaccurate in a major (albeit subtle) way. The lottery data, in fact, were not produced using "the same experimental procedures." These data were produced during practice sessions implemented to remove misconceptions about the elicitation device used to elicit mug valuations, which eventually would be used to test EET. Our procedures call for substantial paid practice prior to collecting data for analysis and testing purposes. The lottery rounds were not preceded by practice.

In this section we clarify two important points in an effort to alleviate ILS's concern that readers will (and have) misinterpreted our conclusions, and, most important, to make sure readers understand what is learned from PZ (2005). First, readers of the full text of PZ (2005) will find that we did not claim the procedures are sufficient to control for misconceptions related to lotteries. Second, readers cannot reasonably conclude that we looked for but found no gaps in the lottery data.

A. *PZ (2005) Do Not Claim that the Procedures Remove All Gaps*

ILS state that PZ (2005) "has been widely cited as providing experimental support for the hypothesis that the PZ elicitation procedure eliminates WTP-WTA disparities *in general*" (ILS 2011, p. 993). This claim rests on two sentences in our abstract, which might mislead the reader if read in isolation. The sentences in the PZ abstract read, "Experiments were conducted using both lotteries and mugs, goods frequently used in endowment effect experiments. Using the modified procedures, we observe no gap between WTA and WTP" (PZ 2005, p. 530). The abstract should have read, "Experiments were conducted using lotteries as training tools. Endowment effect theory was tested using mugs, goods traditionally used for the development and testing of endowment effect theory."

[3] The data supplement includes all lottery data and an explanation of the problems we discovered when we analyzed them. We summarized the problems in the 2005 paper.

When PZ (2005) is taken as a whole, it is clear we did not claim that the procedures are sufficient to control for misconceptions in all contexts. In fact, we went to great lengths to caution against such a broad interpretation of our results. In several other parts of the paper, we explicitly state that we make no claims about having discovered a set of procedures that remove all sources of misconceptions (e.g., "...we have neither a general theory of what might constitute misconceptions nor a set of operational definitions characterizing them. Constructing a full set of procedures to control for them could be very difficult" (PZ 2005, p. 543); " … the concept of misconceptions has not been operationalized formally and certainly not quantified. In fact, its meaning changes from one experimental environment to another, and from experimental study to experimental study. Consequently, a theory of misconceptions has not been developed" (PZ 2005, p. 531)). We included these statements in the paper specifically to ensure that readers would not interpret claims made about the procedures too broadly. As a general matter, we encourage readers to develop impressions based on our full text.[4]

B. *PZ* (*2005*) *Did Not Claim that Gaps were Eliminated in Lottery Rounds*

While ILS assert that readers of PZ (2005) believe that we looked for and found no gaps in the lottery data, a closer look demonstrates that readers who read parts of the paper not quoted by ILS along with ILS's quotes are very unlikely to draw such a conclusion. While ILS focus on the text in our abstract and concluding section to claim that readers will interpret us as claiming that we eliminated gaps in the lottery rounds (ILS 2011, p. 993), in other parts of the paper that focus on the results themselves, we make it clear that we did not include the lottery rounds in our analysis. Our footnote 15 states that the lottery data collected during training rounds were not used (and should not be used) to test EET. We described features of the data suggesting that they should not be used to test EET and would be a challenge to *any preference-based theory*.

Finally, the studies referenced by ILS as examples of misinterpretations seem not to misinterpret our claims, contrary to ILS's suggestions. None of the quotes mentions lottery rounds, and some explicitly mention mug rounds. The quotes simply summarize our claim that observed gaps cannot be used to support EET, and the data suggest misconceptions are key.

II. ILS's Mug Data Reject Endowment Effect Theory

Before we address ILS's main concern, it is important to remind the reader about our study's major claims and purposes and to emphasize that ILS's mug round data

[4] As a warning to ILS readers, we note here that ILS sometimes quote from context, omitting key qualifying phrases. For example, while attempting to establish that we made broad claims about the removal of gaps in general, ILS include only part of the following quote: "The 'primary conclusion' they derive from the data they report is that 'observed WTP-WTA gaps do not reflect a fundamental feature of human preferences' …" (ILS 2011, p. 992). "*That is, endowment effect theory does not seem to explain observed gaps*" (PZ 2005, p. 542). ILS omit the last italicized sentence thereby substantially changing the meaning of our text. This sentence, along with many other statements in our paper, make it clear that we properly focus on experiments using commodities that allow for clean tests of EET. The full quote clearly indicates that we made no claim about all WTP-WTA gaps in all circumstances and unrelated to EET tests.

unambiguously reject EET. While ILS claim not to be concerned with explaining observed gaps[5] (or at least with testing EET[6]), the purpose of our study clearly is to test EET.[7] We find that gaps observed in KKT (1990) mug experiments are not due to a kink at the endowment as posited by EET. When the experiment is controlled for subject misconceptions about the elicitation device, the valuation gap disappears. ILS conduct experiments with mugs using our procedures. Their mug data confirm our basic conclusion.

Our study was designed specifically to focus on particular experiments developed to demonstrate EET at work—the case of mug experiments, where all alternative explanations seem to be ruled out. Data from both PZ (2005) and ILS demonstrate that experiments advanced in the literature as support for EET (i.e., experiments asking subjects to state valuations for goods such as candy and mugs) fail to produce gaps when controls designed to address misconceptions about the elicitation device are implemented. Thus, ILS contribute to the literature by replicating (using modified procedures) the key PZ (2005) result, adding to the data from other experiments that challenge EET as it was originally developed and applied.

ILS avoid a discussion of the relationship between their data and EET by claiming that "PZ do not set out EET as a specific formal theory" (ILS 2011, p. 994). They argue "[a]s used by PZ, 'EET' appears to refer to a loosely related family of theories of reference-dependent preferences which has evolved and diverged over time …." (ILS 2011, p. 994), concluding that it is impossible to determine whether data support or reject the theory. They justify their avoidance of the issue by arguing that "[s]ince EET is not a sharply defined concept, engagement with those issues would be an unhelpful distraction from the point of our paper" (ILS 2011, p. 995).

This characterization of EET and our study is inaccurate. Contrary to ILS's claim, EET is a precisely stated theory with unambiguous predictions that apply generally.[8] KKT (2008, Figure 2) provide a clear formulation of EET derived directly from Tversky and Kahneman (1991, pp. 1046–7). The figure displays an indifference curve with a kink at the endowment, indicating that the marginal valuation for a reduction of the quantity of a good exceeds the marginal valuation for an increase in the good. In other words, the prediction that WTA exceeds WTP is deduced directly from Tversky and Kahneman's (1991) axioms.[9] While ILS claim that EET generates no unambiguous predictions,[10] the developers and appliers of the theory harbor no

[5] "[W]e are not concerned with testing *any* particular theoretical account of WTP-WTA gaps" (ILS 2011, p. 995).

[6] "[W]e are *not* concerned with PZ's interpretation of what they call EET. Nor are we concerned with whether PZ's or our own data are consistent with EET" (ILS 2011, p. 995). Later in the paper, ILS design experiments to test a house money effect explanation and other explanations for gaps in mug rounds.

[7] We clearly state in our title, abstract, and throughout the paper that our experiment is designed specifically to test EET.

[8] Our paper refers to two of many specific descriptions of the purported link between valuation gaps and prospect theory (PZ 2005, p. 531).

[9] Note that EET is a theory of preferences as opposed to a theory of a decision process as developed by Gigerenzer and Todd (2008) or a hybrid of preference and process as postulated by the discovered preference hypothesis (Plott 1996). When describing the experiments with mugs, KKT (1990, p. 1346) are clear that the shape of the indifference curves and the role of the endowment are important features of the theory: "To conclude, the evidence reported here offers no support for the contention that observations of loss aversion and the consequential evaluation disparities are artifacts; nor should they be interpreted as mistakes likely to be eliminated by experience, training, or 'market discipline.' Instead, the findings support an alternative view of endowment effects and loss aversion as fundamental characteristics of preferences."

[10] KKT (1990) do mention some contexts in which EET might not predict a gap (e.g., goods held for resale and induced-value tokens), but none of these contexts is relevant for our mug experiment.

such hesitations. For example, Knetsch, Fang-Fang Tang, and Thaler (2001, p. 257) conclude, "The endowment effect and loss aversion have been among the most robust findings of the psychology of decision making. People commonly value losses much more than commensurate gains independent of transactions costs, income effects or wealth constraints." KKT (1990, p. 1345) also express confidence in the reliability with which the theory can be applied, holding that its consequences can be observed in property rights acquired by historic accident or fortuitous circumstances, government licenses, landing rights, transferable pollution permits, divisions, plants, product lines, etc. Those applying EET in legal contexts have made similar claims.[11]

EET has been applied widely in economics and other fields, and gaps observed in the laboratory are used as evidence of the reliability of the theory as it is applied in the field. The applications assume that a kink exists at the endowment as opposed to other contextual reference points. Our experiment design was meant specifically to test whether the laboratory evidence conventionally used to support EET (i.e., mug experiments) would hold up when misconceptions about the elicitation device were controlled. Our data, ILS's data, and others' data reject EET when put to that test.

III. Lottery Data Contamination

The purpose of our lottery rounds was to provide our subjects with paid practice to help them understand the properties of the Gordon M. Becker–Morris H. DeGroot–Jacob Marschak (1964)—henceforth, BDM—mechanism and the meaning and consequences of expressing bids and asks in the BDM context.[12] During the practice rounds, we elicited valuations for four certainty lotteries in which the outcome was a small amount of money for certain,[13] in addition to two small-stakes, noncertainty lotteries and eight larger stakes, noncertainty lotteries, the outcomes of which were determined randomly. While we were curious whether the training rounds produced useful data, even though they were preceded by no training, our analysis of the data revealed patterns that suggest an appropriate experimental design and control were so lacking that we had no hope of being able to include a convincing analysis of the data.

ILS attempt to develop a set of experimental procedures that remove "all contamination" from lottery choices that we identified in our paper. Indeed, ILS define as their major purpose to determine whether a WTP-WTA gap remains in the

[11] "Researchers in behavioral decision theory have developed a growing line of evidence that people appear to value a good that they own much more than an identical good that they do not own …. Researchers have used several different procedures to demonstrate the endowment effect" (Jeffery J. Rachlinski and Forest Jourden 1998, p. 1551). Rachlinski and Jourden go on to argue that endowment effect theory explains why litigants expend resources to appeal judicial decisions that strip them of property rights even in cases in which the court awards damages to compensate for the loss. Similarly, Peter H. Huang (2004) suggests that valuation disparities impact settlement behavior of litigants. Law reviews house hundreds of similar applications of endowment effect theory to law.

[12] Given the role of the training rounds, lotteries appealed to us. Using lotteries for training was more efficient than consumer goods because we did not have to have a large number of different commodities on hand for each training decision of each session.

[13] Given ILS's inferences from a paragraph taken from our instructions ("This passage seems to be advising subjects to use the small-stakes lottery tasks as practices for the large-stakes lottery tasks and possibly (in the case of treatments 1 and 3) also for the mug task …." (ILS 2011, p. 998)), we worry that readers will make the unsupported assumption that we concluded that the four degenerate and two other small-stakes lottery rounds were sufficient for training. ILS's interpretation does not reflect our intentions. Our goal was simply to motivate subjects to pay attention during the small-stakes rounds.

lottery rounds after controlling for all sources of contamination.[14] They describe their experiment as an "uncontaminated replication of PZ's experiment" (ILS 2011, p. 995). In our view, however, it does not control for contamination that we identified and discussed. ILS rejected our analysis of misconceptions as ad hoc theorizing. Unfortunately they did not check their data for the footprints we identified, and it turns out that the ILS experiments contain the same evidence of contamination that we identified in our lottery data.

Relative to our concept of what constitutes lottery data contamination, ILS use a narrower definition. ILS use the term "contamination" to describe specific procedures related to the order of the lottery rounds and the personal communications between subjects and the experimenter during the practice rounds.[15] By contrast, our footnote 15 develops the foundation for our analysis—"The lottery round data … are contaminated by a design that was developed only for training and not for purposes of measuring a gap" (PZ 2005, fn. 15). Our paper attempts to make clear that we designed the lottery rounds with the single purpose of providing subjects with paid practice with the elicitation device that would subsequently be used to elicit mug valuations to test EET. We neglected all other aspects of training necessary for subjects to understand lotteries. Indeed, for the lottery rounds we made no attempt to apply the revealed theory method, which was central to our paper.[16]

Our footnote 15 continues with a list of both sources and hints of subject misconceptions related to the elicitation device, the nature of randomization and the concept of probability, all of which bear on lottery valuations. The list summarizes the misconceptions analysis contained in a data supplement that we made available to everyone interested in the lottery data, including ILS. When we conducted our study, our view was that the data were not useful for testing EET and possibly not useful for testing any other theory of preference; thus, encouraged by the editor to save space, we saw no need to mention the lottery data analysis in our paper beyond the brief summary that we included. Of course, ILS are correct in assuming that if one wishes to study lottery valuations then special effort must be made to examine subject-experimenter interactions, otherwise one cannot be sure exactly what experiment was conducted. Contamination, however, is a much broader concept than ILS assume. Looking over our lottery data again, together with ILS's data, brings the problems we detected and reported into clearer focus.

Our data supplement reports gaps in the lottery data. It also reveals footprints of misconceptions about random devices that we identified by analyzing the lottery data in light of subject debriefs that provided hints about where to look. More specifically, we reported evidence of irrational valuations, evidence that subjects' subjective probabilities might be influenced by the valuation measurement and inexplicable risk preference patterns. We find the same footprints of misconceptions in

[14] "In mounting our study, our primary objective was to apply PZ's elicitation procedure to both mugs and lotteries while ensuring that none of the paid tasks was contaminated. (ILS 2011, p. 994).

[15] "All WTA rounds were conducted prior to the WTP rounds," and "Mistake corrections, public answers to questions and other procedures were also employed continuously …" (PZ 2005, fn. 15). ILS interpret our footnote 15 to mean that if these two sources of contamination are properly controlled (or otherwise explained away), the lottery data will be useful for determining whether gaps are linked to misconceptions.

[16] A first cut at applying revealed theory methodology to lottery rounds can be found in a separate online Appendix available at *http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.2.1012*.

TABLE 1—PROPORTION OF IRRATIONAL VALUATIONS FOR CERTAINTY LOTTERIES

	Lottery #1	Lottery #2	Lottery #4	Lottery #5
Certain payoff	20 cents/pence	30 cents/pence	30 cents/pence	40 cents/pence
ILS	28 percent	24 percent	19 percent	22 percent
PZ (2005)	3 percent	3 percent	3 percent	5 percent

the ILS lottery data, and closer study reveals patterns in the data that challenge ILS's enterprise of using lottery valuations to measure WTP-WTA gaps.

While we presented no general theory of misconceptions, the existence of footprints of misconceptions about the random device implies that misconceptions cannot be ruled out as an explanation of patterns observed in the lottery data. In the remainder of this section, we provide a preliminary analysis of our conjectures related to the lottery data. The data suggest that our training procedures are inappropriate for eliciting lottery valuations. These are the footprints of contamination that our footnote 15 and data supplement report.

A. *Irrational Choices*

The natural places to look for evidence of misconceptions are those in which the indicators are uncontroversial and clear. For this reason, we first examine subject valuations of the four lotteries with certain payoffs. Under the BDM procedure, the dominant strategy is to value certainty lotteries at the certain value. Presumably, any irrational choices expressed in these lotteries reflect misconceptions, misunderstanding or confusion on the part of the subjects.

A close look at ILS's lottery data reveals substantial irrationality, both in general and relative to our lottery data. As ILS explain, their subjects' and fixed offers were restricted to multiples of £ 0.05. Thus, the only rational valuations for a lottery with a certain payoff of x are x and $(x + 0.05)$ for sellers (WTA) and x and $(x - 0.05)$ for buyers (WTP). Table 1 reports the proportion of irrational valuations reported by ILS's subjects and our subjects for the four lotteries with certain outcomes. For example, in Lottery #1, 28 percent of ILS's subjects failed to state a rational value for a lottery that paid 20 pence with certainty. On average, in each certainty lottery round, roughly 23 percent of ILS's subjects exhibited irrationality in this seemingly simple task. The irrationality displayed by ILS's subjects is approximately seven times higher than that displayed by our subjects; roughly 3.4 percent of our subjects' valuations, on average per round, were irrational. ILS suggest that their modifications of our procedures removed all contamination, but their data demonstrate that they did not.

The difference between ILS's and our data is striking, and we can only speculate about the reasons. As we explained in our paper, we detected what appeared to us to be a substantial lack of understanding of BDM during the early training rounds and used hands-on methods to correct it. ILS, following our footnote, viewed the hands-on procedures as a possible source of contamination from an experiment design perspective and employed computerized instructions that involved different methods. The levels of irrational behavior exhibited in ILS's data, however, suggest that either our training procedures did at least some work to reduce some misconceptions or the procedures they used generated additional misconceptions.

Irrational valuations of noncertainty lotteries are similarly signaled by valuations that fall outside the support of a lottery value's probability distribution (when payoffs are positive). If a lottery has a zero probability of paying more than x and a zero probability of paying less than y, then valuations above x or below y violate rationality postulates. The ILS noncertainty lottery data exhibit very little irrationality according to this definition, and thus stand in sharp contrast to their certainty lottery data discussed above. Indeed, the change in behavior between certainty and noncertainty lotteries is dramatic. On average, in the noncertainty lottery rounds, 3.6 percent of ILS's subjects reported irrational valuations, as defined in the subsection above, whereas 22 percent exhibited irrational behavior in lottery #5, the last certainty lottery round. While further investigation is required to determine the factors driving this phenomenon, the sudden change in behavior—substantial irrationality in a simple task to almost no irrationality in a more complex task—is another footprint of misconceptions. In the following section, we provide a more detailed examination of the nature of the misconceptions suggested by both sets of data.

B. *Boundary Valuations*

All ILS and PZ lotteries take the same form. Each results in a payout of x with probability $P(x)$ and a payout of y with probability $1 - P(x)$. The preference elicitation procedures are designed to elicit the subject's lottery value, V. Under such conditions, standard models of risky choice, including the model ILS adopt, hold that $V = x$ if and only if $P(x) = 1$, and $V = y$ if and only if $P(x) = 0$. This property is true regardless of the level of risk aversion.[17] Thus a valuation at the boundary of a lottery payoff support suggests an extreme belief over the outcome probabilities. Of course this model assumes that possible lottery outcomes are continuous and that utility functions are smooth. If these mathematical properties are viewed only as approximations, boundary valuations imply beliefs that are inconsistent with the objective probabilities of the lottery value supports. In other words, under such simplifying assumptions, the existence of boundary valuations is evidence that subjects have misconceptions about randomness. We hasten to add that an individual choice model assuming a kink in the preference relation at a possible reference point might be consistent with the lottery data (although in some cases, it might require extreme levels of loss aversion). Thus, the discussion of this section must be considered as tentative conjectures.

To investigate footprints of misconceptions in the noncertainty large-stakes lottery rounds, we focus on "boundary valuations." We define boundary valuations as valuations that lie exactly on the bounds of the support or irrationally outside the bounds. For example, ILS lottery #13 (a buying task) paid £3.5 with probability 0.5 and £1.5 with probability 0.5; thus, all valuations at £3.5 or above are counted as "at or above the upper bound of the value support," and in this case 5 (5 percent) of ILS's subjects reported such valuations. All valuations at £1.5 or below are counted as "at or below the lower bound of the value support," and in this case 15 (15 percent) of ILS's subjects reported such valuations. Even though valuations exactly on the

[17] ILS 2011 reference constant absolute risk aversion as a measure of attitudes toward risk.

Table 2—Boundary Valuations for Uncertain Large-Stake Lotteries

	ILS		PZ (2005)	
	WTA boundary valuations	WTP boundary valuations	WTA boundary valuations	WTP boundary valuations
	Lotteries 7, 8, 9, 10	Lotteries 11, 12, 13, 14	Lotteries 7, 8 10[a]	Lotteries 11,12,14[a]
Valuations at or above the upper bound of the value support	7.3 percent (29)	3.8 percent (15)	7.7 percent (17)	3.2 percent (7)
Valuations at or below the lower bound of the value support	1.8 percent (7)	14.0 percent (56)	0.9 percent (2)	5.0 percent (11)

Note: ILS $N = 400$ (100 subjects, each valuing four lotteries); PZ (2005) $N = 222$ (74 subjects, each valuing three lotteries).

[a] PZ (2005) lotteries 9 and 13 have a negative lower bound.

bounds are not strictly irrational as defined in the previous subsection, these valuations are sufficiently extreme to challenge reasonable assumptions related to belief formation. Table 2 displays the frequencies of boundary valuations for large-stakes, noncertainty lotteries. In both experiments, subjects valued large-stakes lotteries as sellers (WTA) in rounds 7–10 and as buyers (WTP) in rounds 11–14.

Several features of the data are instructive. First, ILS subjects more frequently reported boundary valuations. On average each round, 13.4 percent of ILS's subjects reported boundary valuations; 8.2 percent of our subjects, on average across rounds, reported such valuations (counting only lotteries with nonnegative outcomes—lotteries #9 and #13 had a negative lower bound in our experiment).

While the ILS boundary valuations are more frequent than ours, both are suspiciously large. Given the experimental environment, these subjects do not exhibit sophisticated beliefs over lottery outcomes and are acting as if they know the lottery outcome with certainty, a phenomenon that we reported in our paper and in the data supplement.

C. *"Boundary Valuation Asymmetries" and a "Question-Influenced Beliefs Conjecture"*

A closer look at Table 2 reveals that subject beliefs appear to be directly influenced by whether they are asked to state their WTA or WTP. That is, the boundary valuations suggest a tendency for these subjects to believe that the high payoff will occur with certainty when they are asked for a selling price (WTA) and that the low payoff will occur with certainty when they are asked how much they would pay (WTP). We reported this phenomenon in our data supplement but considered it evidence only of contamination as opposed to a phenomenon that might be worthy of independent study. A quick search through the literature reveals that others have reported closely related phenomena elsewhere (e.g., Jane L. Risen and Thomas Gilovich 2007).

A "boundary valuation asymmetry" or, more generally, the support for a question-influenced beliefs conjecture appears as the asymmetric tendency for subjects to be more likely to report boundary valuations at or above the upper bound in selling (WTA) rounds and at or below the lower bound in buying (WTP) rounds. For example, when asked to report WTA, 7.3 percent of ILS's subjects reported a value at or above the upper bound while 1.8 percent of their subjects choose at or below the lower bound, a ratio of just over 4 to 1. When their subjects were asked to report WTP, however, the ratio of upper to lower bound choices reverses to roughly 1 to 4 (3.8 percent versus 14.0 percent). Our subjects exhibited the same tendency: for WTA, the ratio of upper to lower bound values was 8.5 to 1, and for WTP it fell to 1 to 1.6.[18]

Simply put, the data support a question-influenced beliefs conjecture. Specifically, the conjecture holds that when reporting valuations as sellers, subjects are more likely to believe that the lottery outcome will be the higher payoff. Conversely, when subjects report valuations as buyers, they are more likely to believe that the lottery outcome will be the lower payoff.

The question-influenced beliefs conjecture has implications for the issues of interest to ILS. It implies that observed WTP-WTA gaps might be due to a difference in the beliefs about outcomes that is influenced by the preference elicitation question. This suggests that observed gaps could be due to an uncontrolled, systematic shifting of beliefs as opposed to the shape of the preference relation EET assumes. The conjecture also has implications for the use of lotteries in the measurement of WTP-WTA gaps. If misconceptions about the random device cannot be controlled, and in particular, if the systematic violation of sophisticated beliefs cannot be avoided, then there is an issue about the use of lotteries to measure WTP-WTA gaps, which assume that individuals value the same good as buyer and seller. If the roles of seller and buyer trigger a change in subjective probabilities for the same lottery, then from the subject's point of view the lottery valued from the buying perspective is not the same as the lottery valued from the selling perspective. Unless subjects view the good as the same good from both perspectives, or unless the standard theory can be adjusted to deal with this phenomenon, testing EET or even measuring a valuation gap using lotteries becomes difficult if not impossible.

We note that the analysis offered here is focused on the boundary choices, the only cases in which beliefs can be clearly measured and separated from preferences. In an online Appendix we provide additional statistics related to the sensitivity of the WTP-WTA gap to the removal of the boundary values.[19] Our conjecture is that the overall gap is influenced by the small set of valuations on or outside the boundaries. We include in the online Appendix some insights about the literature related to controlling beliefs and the set of procedures a thorough application of revealed theory methodology might produce. Our hope is that the methods suggested by the literature will provide tools for removing the unwanted influences we detected in the lottery data, which discouraged us from using it to test theories such as EET.

[18] Proportion of at-or-above the upper bound valuations in ILS's WTA rounds (7.3 percent) exceeded the proportion of at-or-above the upper bound valuations in ILS's WTP rounds (3.8 percent) ($p = 0.02$; one-tailed test). Similarly, 14 percent > 1.8 percent ($p = 0.00$); 7.7 percent > 3.2 percent ($p = 0.02$); and 5 percent > 0.9 percent ($p = 0.005$). Using proportions from each round, we also conducted one-tailed tests of equal proportions to test whether subjects are more likely to report valuations at or above the upper bound in WTA rounds relative to WTP rounds. Five of the eight tests using ILS's data resulted in a statistically significant difference (at the 5 percent level); four of the six tests using our data produced similar results.

[19] The online Appendix can be found at *http://www.aeaweb.org/articles.php?doi=10.1257/aer.101.2.1012*.

D. *Preference Consistency and Risk Preferences*

In footnote 15 (PZ 2005), we suggested problems related to subjects' ability to assign values to lotteries. In our data supplement, we were more explicit about the usefulness of the lottery data for testing theories such as EET. The footprints of misconceptions we reported included an analysis of risk preferences. Classical prospect theory holds that individual preferences are possibly risk averse in the gains and possibly risk seeking in the losses (Kahneman and Tversky 1979). Individuals, however, are postulated to have consistent (complete and transitive) risk preferences given an endowment (or reference point). That is, risk preferences will change as the endowment changes, but, given the endowment, risk preferences are of a classical type. Even neglecting the possibility that EET does not apply to lotteries, testing with data that violate these fundamental assumptions seems unhelpful.

The problem we saw in our data, and that we see in ILS's data, is that subjects appear to exhibit shifting preferences and shifting risk attitudes as expectations and experience evolve. While modern theory might be able to accommodate such instability, it presents a challenge to EET as it is traditionally developed and applied. Both ILS's and our subjects failed to exhibit stable risk preferences across rounds. For example, only 38 percent of ILS subjects consistently displayed risk aversion in all of the large-stakes buying rounds. Similarly, only 23 percent of our subjects consistently bid below the expected value of the lottery in every large-stakes buying round.[20] While such proportions suggest that a large number of subjects had risk seeking preferences, very few subjects consistently bid the expected value or bid above the expected value in all of the large-stakes buying rounds.[21] Thus, 53 percent of ILS's subjects and 64 percent of our subjects failed to exhibit consistent risk preferences in buying rounds. Such instability, the flopping back and forth between risk aversion and risk seeking behavior in WTP rounds, suggests a loss of experimental control and suggests that, without insights about the nature of the randomness, the data are of questionable value for gap measurement and for theory testing. The same instability is observed in selling rounds, with 70 percent of ILS's subjects and 72 percent of our subjects exhibiting inconsistent risk preference throughout those rounds.[22] A natural hypothesis is that this general pattern of instability is related to the question-influenced beliefs conjecture, but we mention it only to bring the possibility to the reader's attention since it is beyond the scope of this paper.

In conclusion, it is important to remember that EET rests on a theory of preferences characterized by a specific kink at the reference point set by the endowment. Thus, if EET is tested using any data for which the reference point is known or assumed to be known, and if the data fail to exhibit consistent preferences, the

[20] Risk aversion is commonly reported in experiments, so a lack of evidence of risk aversion (especially in buying rounds, as some versions of EET predict risk seeking behavior in selling rounds) signals a possible lack of experimental control. Of 400 large-stakes WTP valuations in ILS's study, 91 (23 percent) were *above* expected value. The results are starker if we include the small-stakes lotteries. In PZ's (2005) study, 63 (21 percent) of 296 large-stakes WTP valuations were above expected value. This calls control into question in both experiments.

[21] Three percent (8 percent) of ILS (PZ 2005) large-stakes lottery buyers bid the expected value each round. Six percent (5 percent) consistently bid above expected value in these buying rounds.

[22] Eleven percent (5 percent) of ILS (PZ 2005) large-stakes lottery owners offered below expected value each round. Thirteen percent (11 percent) consistently offered above expected value in these rounds. Six percent (12 percent) consistently offered the expected value in these rounds.

theory is rejected. The lottery data might tell a useful story if applied to modern extensions of prospect theory, especially those assuming the reference point is set by something other than the endowment. Pointing to the lottery data as either support for or rejection of EET, however, would not be credible given that major features of the data disconfirm the basic assumptions of EET.

IV. Explanations for Gaps: House Money Effect, Enhancement Effect Theory and Endowment Effect Theory

Despite ILS's claim that they are "not concerned with testing *any* particular theoretical account of WTP-WTA gaps" (ILS 2011, p. 995), they include expansive discussions of possible explanations. Specifically, they offer two conjectures to explain the mug results reported both by us and by them. Both conjectures are problematic. We discuss them in turn and note that their second theory is simply a version of a more general theory we call enhancement effect theory, which holds that the process of acquisition and experience of ownership can create special features of goods that have independent value that can be confused with a kinked preference relation.

A. *The House Money Effect*

The first conjecture is that the elimination of the gap in mug rounds is due to a house money effect triggered not by cash from lottery winnings but by the show-up fee paid in cash at the beginning of the experiment. While this alternative explanation is worth considering, we note that it does not work to explain the broader set of results found in the literature. It also provides additional evidence to reject EET.[23]

While ILS's results are suggestive, one should be hesitant to accept the house money effect explanation on the basis of this limited evidence. While ILS prefer not to formally test the conjecture, it could be tested in many ways, the most straightforward of which would be to replicate KKT's (1990) gap result and then add a show-up fee to the KKT (1990) procedures. The disappearance of the gap would serve as evidence of the house money effect conjecture. ILS, however, cannot perform this test because they have not produced a baseline using KKT's (1990) original procedures, which do not include a show-up fee but result in a gap. They claim that they performed a "controlled comparison between the PZ and KKT elicitation procedures" (ILS 2011, p. 1006), but they have not. Since ILS do not perform this experiment, the lack of a gap in their modified KKT (1990) experiment possibly is a result of a different subject pool or subtle differences between KKT's (1990) original procedures and ILS's modification of them. That is, it could be the case that KKT's (1990) results simply are not robust to slight changes in procedures, which would imply that EET does not explain gaps.

Second, their explanation does not work well to explain the pattern of results reported in the literature. Experiments conducted by Stephanie Kovalchik et al.

[23] Of course, others might argue that, rather than rejecting EET, the evidence simply suggests that multiple effects exist and, in some cases, one effect will dominate the others. If this were the case, however, EET would need to be modified to take these other effects into account. This adaptation of the theory, however, would seriously compromise the effect's robustness and would call into question the myriad applications of EET. See Zeiler (2010) for numerous examples of applications in law.

(2005) employ the PZ (2005) procedures and do not observe a gap in mug valuations, yet they do not employ a show-up fee. This result is contrary to the predictions of the ILS conjecture that PZ's (2005) elimination of the gap is the result of a house money effect. In addition, the experiments performed by Alex Smith (2008) employ KKT's (1990) procedures but add a show-up fee. Contrary to the prediction of ILS's theory, Smith (2008) reports a valuation gap. Thus, the broader literature meets the ILS house money effect explanation of our mug gap with skepticism.

B. *Enhancement Effect Theory*

The second theory ILS advance to explain gaps observed in the lab is based on a blurring of EET with what we call "enhancement effect theory." Enhancements are generated through the process of endowment or the experience of ownership. Enhancements create value that is added to value that exists in the absence of the enhancement.[24] For example, if the experimenter announces, "the mug is a gift" when endowing subjects, the nature of the good in the eyes of nonowners and owners might differ (PZ 2007). Potential buyers are deciding about a mug, while potential sellers are deciding about a mug that was a gift from someone who will observe whether the "gift" is traded for something else. Sellers who benefit from signaling appreciation to the experimenter might be more likely to ask for more than they would if they were considering only the mug's consumption value.

Examples of valuation gaps taken from the field demonstrate the potential for blurring enhancements and endowments. In the field, ownership is often associated with enhancements that supplement, modify, and shape the value of a good for potential sellers. For example, a potential home seller values his home from the perspective of someone who has lived in it for some time. The independent value from enhancements that shape the seller's overall value could emerge from many unobservable sources or could manifest in observable ways (e.g., through the good memories that living there creates). Economic theory posits a number of sources of enhancements that might produce gaps, such as value associated with a trophy or the first dollar one earns, or values associated with information asymmetries (e.g., a risk-averse owner might place a higher value on the car she has been driving relative to an identical car that was previously owned by someone else simply because she is more certain about how her car impacts her utility). If we count unobserved beliefs as enhancements and if almost any behavior can be supported by some assumption about beliefs, it becomes difficult to separate EET from enhancement effect theory. Despite this difficulty, the question-influenced beliefs phenomenon we reported in Section III provides at least some opportunity for separation.

Without the ability to control for enhancement effect theory as an alternative explanation, we are unable to determine whether a kink in the preference relation at the reference point is causing gaps observed in complex field environments, or

[24] In a separate study, we explore environments in which features of the experiment design generate enhancements that might add value to endowed goods (PZ 2007). Specifically, we tested what we now call enhancement effect theory against EET and found support for enhancement effect theory. While we focused on features that enhance the value of endowments in an effort to explain exchange asymmetries, we recognize the possibility that features of the environment might also reduce the value of the endowed good, possibly leading to buyer valuations that exceed seller valuations.

whether ownership (for more than just a few minutes) has somehow changed the nature of the good. It is hard to imagine an example taken from the field and used as support for EET that could not also be explained by some imagined but unobserved enhancement effect, including beliefs and expectations.[25] Of course, this calls into question the falsifiability of enhancement effect theory when applied in the field. A similar problem, however, is a challenge to field applications of EET. Essentially any gap in the field also can be attributed to some unobserved reference point combined with loss aversion when in fact some enhancement or some systematically varying belief might be at work.

The problems of distinguishing EET from enhancement effect theory naturally spill into the laboratory. For example, ILS criticize our instructions as biasing the results against EET by using the word "offer" as opposed to words like "buy" and "sell." They assert that such language "reduc[es] the salience of the distinction between buying and selling tasks" (ILS 2011, p. 1007). They argue that, while our subjects are told that they own the good, "there is little else to flag up the difference between buying and selling, whereas other experiments draw more attention to this difference" (ILS 2011, p. 1007). Enhancement effect theory predicts, however, that subjects might perceive the experimenter's emphasis of roles as information about the value of the goods (PZ 2007). Emphasis on "buyer" and "seller" roles can also trigger strategic instincts that cause sellers to offer high amounts and buyers to offer low amounts (PZ 2005). Therefore, if we emphasize buying and selling and we observe a gap, we cannot be sure whether the result supports EET or enhancement effect theory (or reversion to basic instincts).[26] Similarly, ILS argue that placement of the good might impact the setting of the reference point, and that elimination of the gap in our experiment might be due to the fact that both sellers and buyers had mugs in front of them (ILS). Again, if we make such a change and observe a gap, enhancement effect theory cannot be ruled out because it suggests that placement of the goods might signal relative value (PZ 2007). Given that the theories are observationally equivalent in general settings, separation of them requires highly controlled laboratory settings.

V. Concluding Remarks

PZ (2005) draws a specific conclusion: EET does not explain the WTP-WTA gap observed in the classic mug experiments that were designed specifically to give the theory its best shot. ILS replicate this result, providing support for our central conclusion.

That EET does not explain gaps does not tell us what does explain them. Our analyses (PZ 2005, 2007) offer two conjectures, both of which are related to experiment procedures. The first posits that gaps are due to misconceptions related to

[25] David Genesove and Christopher Mayer (2001) is an example demonstrating the possibility of separating loss aversion (with the purchase price as the reference point) from enhancement effect theory in a field environment. Enhancement effect theory seems not to explain the results, depending on assumptions made about beliefs. We thank the editor for bringing the example to our attention.

[26] We should add that the forms subjects used to record valuations mentioned buying or selling more than once immediately prior to the elicitation of the offer. For example, the forms used to elicit valuations from buyers read: "**Buyer** Record Sheet: The experimenter owns one Round X lottery ticket. I will offer to **buy** the lottery ticket for an amount equal to the maximum I am willing to pay for the ticket" (emphasis added).

the elicitation mechanism. The second advances enhancement effect theory as an explanation. Enhancement effect theory holds that gaps arise from enhancements predicted by economic theory and generated by the method of endowment and, perhaps, by the elicitation procedures. While it was not their purpose, ILS's results add to the evidence we present here that lottery gaps are related to misconceptions about randomness and beliefs about probabilities produced by preference elicitation procedures. Thus, the ILS experiments bring into focus a dimension of misconceptions that we posited and that, upon further exploration, have been found to raise questions about the appropriateness of interpreting observed lottery gaps as revealing preferences for the same item in the selling role and in the buying role.

PZ (2005) makes clear that we were curious about experimental procedures and associated misconceptions. Our observation related to the nature of goods was (and is) that others have focused on the properties of the endowed good (e.g., the difference between mugs and money or tokens for money) as an explanation for gaps, suggesting that the disparity in gap results that we highlighted in Table 1 in PZ (2005) is caused by the lack of comparability of different properties (Tversky and Kahneman 1991). The conclusion of PZ (2005) was that the mug experiments do not support theories based on the properties of goods formulated to explain the disparity of results across experiments. The claims we make here about the different nature of lotteries are unrelated to our previous claim. Here we argue that properties of goods, such as the randomness associated with lotteries, might carry an inherent possibility of misconceptions, which might lead to gaps. Indeed, our analysis of ILS's and our lottery data supports the view that misconceptions play a role in producing lottery gaps and that different goods might call for different controls for misconceptions. Just as we argued in PZ (2005), however, a full understanding and appropriate test of our conjecture will require an application of revealed theory methodology, which neither ILS nor we applied.

REFERENCES

Becker, Gordon M., Morris H. DeGroot, and Jacob Marschak. 1964. "Measuring Utility by a Single-Response Sequential Method." *Behavioral Science*, 9(3): 226–32.

Genesove, David, and Christopher Mayer. 2001. "Loss Aversion and Seller Behavior: Evidence from the Housing Market." *Quarterly Journal of Economics*, 116(4): 1233–60.

Gigerenzer, Gerd, and Peter M. Todd. 2008. "Rationality the Fast and Frugal Way: Introduction." In *Handbook of Experimental Economic Results*, ed. Charles R. Plott and Vernon L. Smith, 976–86. Amsterdam: Elsevier.

Huang, Peter H. 2004. "Lawsuit Abandonment Options in Possibly Frivolous Litigation Games." *The Review of Litigation*, 23(1): 47–115.

Isoni, Andrea, Graham Loomes, and Robert Sugden. 2011. "The Willingness to Pay—Willingness to Accept Gap, the 'Endowment Effect,' Subject Misconceptions, and Experimental Procedures for Eliciting Valuations: Comment." *American Economic Review*. 101(2): 991–1011.

Kahneman, Daniel, Jack L. Knetsch, and Richard H. Thaler. 1990. "Experimental Tests of the Endowment Effect and the Coase Theorem." *Journal of Political Economy*, 98(6): 1325–48.

Kahneman, Daniel, Jack L. Knetsch, and Richard H. Thaler. 2008. "The Endowment Effect: Evidence of Losses Valued More Than Gains." In *Handbook of Experimental Economic Results*, ed. Charles R. Plott and Vernon L. Smith, 939–48. Amsterdam: Elsevier.

Kahneman, Daniel, and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica*, 47(2): 263–91.

Knetsch, Jack L., Fang-Fang Tang, and Richard H. Thaler. 2001. "The Endowment Effect and Repeated Market Trials: Is the Vickrey Auction Demand Revealing?" *Experimental Economics*, 4(3): 257–69.

Kőszegi, **Botond**, **and Matthew Rabin.** 2006. “A Model of Reference-Dependent Preferences.” *Quarterly Journal of Economics*, 121(4): 1133–65.

Kovalchik, **Stephanie**, **Colin F. Camerer**, **David M. Grether**, **Charles R. Plott**, **and John M. Allman.** 2005. “Aging and Decision Making: A Comparison between Neurologically Healthy Elderly and Young Individuals.” *Journal of Economic Behavior and Organization*, 58(1): 79–94.

Plott, **Charles R.** 1996. “Rational Individual Behaviour in Markets and Social Choice Processes: The Discovered Preference Hypothesis.” In *The Rational Foundations of Economic Behaviour: Proceedings of the IEA Conference Held in Turin, Italy*, ed. K. J. Arrow, Enrico Colombatto, Mark Perlman, and Christian Schmidt, 225–50. New York: St. Martin’s Press.

Plott, **Charles R.**, **and Kathryn Zeiler.** 2005. “The Willingness to Pay-Willingness to Accept Gap, The ‘Endowment Effect,’ Subject Misconceptions, and Experimental Procedures for Eliciting Valuations.” *American Economic Review*, 95(3): 530–45.

Plott, **Charles R.**, **and Kathryn Zeiler.** 2007. “Exchange Asymmetries Incorrectly Interpreted as Evidence of Endowment Effect Theory and Prospect Theory?” *American Economic Review*, 97(4): 1449–66.

Rachlinski, **Jeffrey J.**, **and Forest Jourden.** 1998. “Remedies and the Psychology of Ownership.” *Vanderbilt Law Review*, 51(6): 1541–82.

Risen, **Jane L.**, **and Thomas Gilovich.** 2007. “Another Look at Why People Are Reluctant to Exchange Lottery Tickets.” *Journal of Personality and Social Psychology*, 93(1): 12–22.

Smith, **Alex.** 2008. “Lagged Beliefs and Reference Dependent Utility.” PhD diss. University of Arizona.

Sugden, **Robert.** 2003. “Reference-Dependent Subjective Expected Utility.” *Journal of Economic Theory*, 111(2): 172–91.

Tversky, **Amos**, **and Daniel Kahneman.** 1991. “Loss Aversion in Riskless Choice: A Reference-Dependent Model.” *Quarterly Journal of Economics*, 106(4): 1039–61.

Zeiler, **Kathryn.** 2010. “Willingness to Pay-Willingness to Accept Gaps: The Legal Implications of Recent Empirical Developments.” Unpublished.