Anchors Away:
Field Experiments on Anchoring of Economic Valuations

Jonathan E. Alevy, Craig E. Landry, and John A. List*

July 2008

Abstract

One of the pillars within an entrenched branch of research psychology is the view that preferences are constructed during the value elicitation process. Experiments on anchoring effects yield perhaps the strongest evidence to support the constructed preference hypothesis, since random and clearly uninformative cues are shown to have an important effect on individual economic values. While scholars commonly generalize such effects to markets, the conditions under which such biases meaningfully influence market outcomes have not been specified. This paper takes on this challenge using two distinct field experiments. First, it explores the impact of anchors on consumer valuations in a well-functioning, naturally occurring marketplace. Using a classic anchoring task in a field experiment, we find that while there is little evidence that experienced agents can be anchored, inexperienced consumers can be anchored. Using a complementary field experiment, we find that even in those markets populated solely by inexperienced consumers, distinct anchors have only transient effects on prices and quantities traded in a bilateral bargaining institution: aggregate market outcomes converge to the intersection of the supply and demand functionals after a few market periods.

JEL: C93 (Field Experiments), D11 (Microeconomic Theory)

Key words: anchoring, valuation, field experiment, experience

* Assistant Research Professor, Department Resource Economics, University of Nevada-Reno; Assistant Professor, Department of Economics, and Assistant Director, Center for Natural Hazards Research, East Carolina University; and Professor, Department of Economics, University of Chicago, and Research Fellow, National Bureau of Economics Research; respectively. The authors thank Alejandra Palma, Michael Price, seminar participants at Appalachian State University, University of Tennessee, and East Carolina University for helpful comments and suggestions.

1. Introduction

Take the last three digits of your social security number. Turn those numbers into a dollar value (i.e., if your numbers are 462 then they provide a value of $462). Consider whether you would be willing to pay that dollar amount for a first edition of JRR Tolkien's, The Hobbit. Now, how much would you actually pay for a first edition original copy?[1] A stylized result from laboratory experiments in economics and psychology is that a subject's answers to the latter valuation question are strongly influenced by the initial random offer price, whether for ordinary consumer products or exotic goods.[2] Based on the premise that the randomly determined value should contain no useful information, critics of neoclassical theory have argued that such findings refute the notion that decisionmakers' preferences are consistent and stable. If preferences are unduly labile and influenced by innocuous properties of circumstance, then no optimization principles may underlie even straightforward economic decisions.

Although considerable laboratory evidence consonant with dramatic anchoring effects has accumulated in the literature, a natural inclination for many economists is to discount these results on the grounds that they reflect poorly designed experiments (e.g. they lack sufficient incentives for meaningful response) or are merely the result of a

[1] For those interested Tolkien fans, only 1,500 copies of the first edition were printed. An Arizona buyer recently purchased a first edition copy for $65,000 from a New York bookseller. See http://www.abebooks.com/docs/10-anniversary/powers-10.shtml.

[2] There are a number of valuation exercises that show the effectiveness of an anchoring manipulation (see, e.g, Johnson and Schkade, 1989; Slovic, 1995; Bettman, Luce, and Payne, 1998; Green et al., 1998; Hoeffler and Ariely, 1999, Ariely, Loewenstein, and Prelec, 2003; Bateman et al. 2008). Taken together, these results suggest strong effects, even over goods that subjects have experience in consuming. Yet, anchoring results run much deeper than valuation exercises. For instance, anchoring has been found to be important in answers to factual questions (Tversky and Kahneman, 1974; Epley and Gilovich, 2006), the estimation of probabilities (Wright and Anderson, 1989), predictions of future performance (Switzer and Sniezek, 1991), social judgments (Chapman and Johnson, 2002) and legal decisions (Englich and Mussweiler, 2001), to name a few. It also represents a key component of theories of preference reversal (Schkade and Johnson, 1989) and the effect of ambiguity on probability judgments (Einhorn and Hogarth, 1985). The interested reader should see Epley (2004) for an excellent overview of the literature and the theories underlying how and why anchoring influences decisions.

mistake made by inexperienced consumers—the overall thought being that with appropriate incentives, even inexperienced consumers will learn through time, and their behavior will more closely match predictions from neoclassical models. Furthermore, even in those cases where inexperienced consumers constitute a considerable portion of the market, little is known about how such consumers influence equilibrium price and quantity outcomes.

This study provides some insight into these questions by conducting complementary field experiments within a well-functioning marketplace: the sportscard market.[3] In our first field experiment, we make use of an anchoring treatment that has been found in previous experiments to influence statements of value. In this experiment, we exogenously vary the type of good, using one that subjects expect to buy, sell, and trade in this market (an unopened package of sportscards) and another that is familiar, but that is unrelated to the market—a jar of peanuts. By examining behavior of both ordinary consumers and professional sports memorabilia dealers, we are also able to explore how the level of market experience relates to valuation decisions.

The data provide some unique insights. First, there is suggestive evidence that anchoring matters in the valuation exercise. Yet, the anchor only influences ordinary consumer valuations of the good that they did not expect to value when entering the market (the jar of peanuts). For example, whereas consumers were inordinately influenced by their randomly determined anchor in the peanut valuation treatment, they

[3] While we do not consider the sportscard marketplace particularly worthy of study in its own right, it is useful for our purposes for several reasons. First, it is a natural setting for an examination of preference structures since it provides a rich array of subjects making decisions in a familiar environment. Second, we can identify factors that arise endogenously, such as market experience or a person's role in the marketplace, and impose the remaining controls necessary to implement a clean experiment to explore whether these, or other, factors attenuate anchoring. Finally, when larger, or "more important" markets, are difficult or impossible to conduct experiments in with parallel control, manipulating smaller scale markets has value in that we can learn behavioral tendencies from at least one naturally-occurring setting.

were not influenced by the anchor when valuing the good they expected to buy, sell, and trade in the market (the unopened package of sportscards). We find that dealers were not influenced by the anchor for either peanuts or the unopened package of sportscards.

These results suggest that a segment of the population in a naturally-occurring market might be susceptible to common anchoring effects. Accordingly, these findings should influence the extensive literature that has concentrated on finding non-expected utility resolutions to paradoxes of choice, for example. From a policy perspective, these results merit serious consideration in several circles. For instance, the effects of anchoring have been of interest to practitioners of cost-benefit analysis for decades. In the non-market valuation setting, evidence of anchoring effects has become an important heuristic for evaluating the reliability of stated preference methods such as contingent valuation that are used to estimate the value of public goods (e.g., Kristrom, 1993; Holmes and Kramer, 1995; Herriges and Shogren, 1996; Green et al., 1998; Bateman et al., 2008).[4] Anchoring in this setting is consistent with the idea that the task of valuing non-market resources is hampered by consumers' unfamiliarity with such decision environments and the complexity of the exercise. Empirical results herein present some guidance into the underlying reasons for the observed anchoring, and provide hope that anchoring amongst even inexperienced consumers can be attenuated with the appropriate protocol.

While these behaviors might have import for survey based approaches to elicit non-market values, whether, and to what extent, these behavioral tendencies influence

[4] Non-market anchoring effects have also received attention in the broader literature on survey design. For example, efforts to reduce non-response to sensitive numeric questions, such as income, have made use of bracketed ranges of incomes, and there is evidence that these bracket values can anchor responses (Hurd 1999).

market equilibria is largely unknown. Extant theory suggests that factors such as the composition of marginal and inframarginal traders, the trading institution, and other market particulars, might have significance in the transference of the anchoring result to markets. To lend insights into this question, we design a second field experiment that makes use of a stronger anchoring treatment: in a decentralized bargaining market, we vary information concerning previous transaction prices, which serve as the focal source of market uncertainty. We find evidence that the anchor has some influence on early market transactions, but that the effect is transient. Even in those cases where the market is populated entirely by consumers who can be anchored, quantities and prices approach the intersection of the supply and demand curves after a few rounds of market play.

Taken together, the results in this paper suggest that when constant feedback mechanisms are in place and participants are able to receive signals of value and adjust their behavior accordingly, anchoring does not play a sharp role in bilateral market outcomes. In other instances, such as contingent valuation exercises commonly performed by government agencies, anchoring can have an important effect. Hence, the analyst should be aware of such effects and consider the properties of the situation when executing cost-benefit analysis.

The remainder of our study proceeds as follows. Section 2 summarizes the experimental design and the empirical results for the valuation experiment. Section 3 describes the experimental design and results for the bilateral market experiment. Section 4 discusses the relevance of our findings, and section 5 concludes.

2. Experimental Design and Results: Valuation Experiment

Our first field experiment was conducted at sportscard tradeshows in Virginia, USA, where we set up booths similar to those of professional dealers in the sportscard market. Our subjects include both ordinary consumers (nondealers) and professional sports memorabilia dealers. Non-dealer subjects are those participants who voluntarily approached the experimenter's booth and whose presence in the marketplace indicates an existing interest in sports memorabilia. Dealer subjects were approached at their own booths prior to the opening of the show and asked to participate in the study. Informed consent was obtained from all participants before introducing them to the protocol.

Table 1 presents the 2×2 design that we employed in our first experiment; treatments varied by type of subject (dealer or non-dealer) and type of good. The first good is an unopened pack of Upper Deck NFL cards, recently released to the market. As with any unopened pack of collectible cards, the value of the contents are uncertain, since different player's cards have different values. The pack we used had an additional element that emphasized the lottery-like aspect of the unopened deck. These card packs had a small probability (approximately 2%) of containing a special trading card with a swatch of fabric from a player's jersey worn during an actual NFL game. The market value for such cards depends upon the player and year, but in general is not well established at the time of pack release. During our experiment, one subject was able to sell a "jersey card" for $15—approximately three times the value of his own estimate of the original pack's value—to a card dealer. By introducing greater uncertainty, the choice of these collectible cards as an object of valuation was intended to give the anchoring protocol its best chance to succeed with a common market good.

Our second good is a large jar of unsalted, shelled peanuts, which was chosen as a common good for which consumers may have an established value. Only the dealers in the market might anticipate trading this good at the sportscard show, however. Thus we denote it as the "unexpected" good for ordinary consumers.[5] The recent theoretical exercise of Kőszegi and Rabin (2007) that examines mistakes in implementing preferences provides the underpinnings for why such expectations might play an important role in observed behavior. One interpretation of their work is that behavior will be more anomalous in situations that present themselves as surprises.

In implementing the protocol, data was collected by a monitor working one-on-one with each subject, who was endowed with one, and only one, of the two goods. The endowed good was rotated randomly based on the time the non-dealer approached the table, but we intentionally oversampled consumers receiving peanuts, since preliminary evidence showed that the variances were largest in this case, and our theory suggests that we were more likely to find considerable effects valuing this good. After receiving the good, subjects were told that they would be asked two questions about selling the good back to the experimenter, after which a coin flip would determine which response was binding. It was emphasized that they would keep either the good or cash, and that their answers to the questions and the random process would determine the outcome.

The anchoring protocol was initiated by asking the subjects to write the last three digits of their social security number on the provided questionnaire (see the Appendix for a copy of the questionnaire.) Subjects were asked whether they would sell the good back

[5] We note that dealers entering the market likely expect to be offered a trade with just about anything in this market. For instance, one of the coauthors was once offered a pair of "personally worn" Marilyn Monroe panty hose in trade for a Ken Griffey Jr. rookie card. He politely declined, but now regrets that decision (under new DNA testing procedures in the market these panties could now be tested for authenticity; if they were authentic, which we expect, they would now be worth several thousand dollars—much higher than what a Ken Griffey Jr. rookie card currently fetches (~$100)).

for the price derived from the last three digits of their social security number. For example, if the last three digits of their social security number were 123, their associated question was: would you accept $1.23 to sell the good back to us? Of course, this offer price was clearly uninformative, having been derived from a number that was known only to the subjects. Their dichotomous response was recorded.

Next, we elicited willingness-to-accept (WTA) compensation for the endowed good via an open-ended question. The elicitation of individual WTA made use of the BDM mechanism (Becker, DeGroot, and Marschak, 1964). Our BDM protocol utilized a bag of paper slips, upon each of which was a price. Prices ranged from $0.00 - $10.00 in 25 cent increments. It was explained to the subjects that we wanted to know the *minimum* compensation that they required for parting with the good with which they had been endowed.

Since we were not interested in testing the incentive properties of the BDM mechanism, our protocol included an explanation that the optimal strategy was to offer one's true minimum acceptable level of compensation. After recording their offer, a coin was flipped to determine which choice—the dichotomous choice or the open-ended BDM response—would be executed. If the dichotomous choice question was selected, subjects who answered "no" kept the good, and those who said "yes" sold the good for the SSN value rounded up to the nearest quarter of a dollar. If the open-ended question was executed through the BDM mechanism, a bid price was drawn randomly from the bag. If the bid was greater than or equal to the subject's offer, they were paid the bid amount and the good was returned to the experimenter; otherwise they received no payment and kept

the good. In all cases, subjects were asked to fill out a short survey before the account was settled.

Our approach is similar to the classic anchoring examples used in the psychology literature (see the citations provided above). The reason this method works is debated (for a recent review, see Chapman and Johnson 2002), but one popular explanation is due to Tversky and Kahneman (1974, p. 1128), who argue: "people make estimates by starting from an initial value that is adjusted to yield a final answer . . . adjustments are typically insufficient." Consistent with this idea of anchoring and insufficient adjustment, Jacowitz and Kahneman (1995) suggest that judges who are first asked if a target value is higher or lower than a given anchor adjust their estimates in the appropriate direction until an acceptable value is found.

Experimental Results

Summary statistics are presented in Table 2, at the individual level by treatment. Forty-two percent of our subjects were sportscard dealers, and 65 percent were randomly endowed with the jar of peanuts. The WTA (*offer*) and random anchor (*soc*) both varied widely, between \$0 - \$10 and \$0.09 - \$9.99, respectively. Average experience with the sportscard market (*mktyrs*) was 15 years; our sample consisted mainly of men.

Before we begin with the results summary, we should note that overall, 19 percent of subjects provided inconsistent responses to the two valuation queries. The inconsistencies were exhibited by subjects who stated a minimum WTA less (greater) than the DC offer that they had initially refused (accepted). For both subject groups, the majority of inconsistencies (74 percent) were found in the peanut treatment, and those

exhibiting inconsistencies had less market experience than those who were consistent.[6] Approximately 3 percent provided a minimum WTA exactly equal to their anchors. The remaining 78 percent provided consistent responses in which WTA was different from the anchoring value. For completeness, we present the results with and without the inconsistent responders in the sample.

Perusal of the data provides a first result:

> Result 1: *There is little evidence in the aggregate data that anchoring affects economic valuations.*

Preliminary evidence for this result can be found in Table 3. To begin, we use a simple null hypothesis that the open-ended valuation responses (*offer*) are independent of the random anchor (*soc*). Following Ariely, Loewenstein, and Prelec (2003), we split the aggregate sample by median social security number. Row 1 in Table 3 contains the pooled data summary. In this case, for the entire sample, those with high social security numbers place a sell value of $4.41 on average, whereas those with a low social security number place a sell value of $4.17 on the good. A similar data pattern is observed in the data set that excludes inconsistent subjects: a selling price of $4.46 versus $4.24. While the data tendencies are directionally in accord with the anchoring hypothesis, a Mann-Whitney test reveals that the difference is not statistically significant for either the overall sample ($p = 0.32$), or the restricted data ($p = 0.35$).

To provide additional test of the null hypothesis, we regress *offer* on the social security value and control variables that include age, education, gender and income.

[6] A Mann-Whitney test of differences in market experience, measured in market years, across groups of consistent and inconsistent responders yielded p=.083. Further, roughly three-fourths of the inconsistent responders made offers to sell at a price greater than their social security number when they had previously agreed to sell at that price. The attempt to sell at a price higher than the anchor value suggests that the inconsistent subjects misunderstood the properties of the BDM mechanism, and believed they were in a bargaining situation, despite our urging to report their true value.

Model 1 in Table 4 provides empirical estimates for *all* data and the subset of *consistent* responders.[7] Evidence of the ability to anchor market participants would arise from a positive coefficient on *Soc* that is both economically and statistically significant. As we can see, the unconditional results summarized in Table 3 are supported by parametric regression. Coefficients are small in magnitude and have p-values of .73 and .82 for *all* and *consistent* responders, respectively. Thus, even after controlling for individual specific observables, we find that the offer is not unduly influenced by the random anchor for the pooled data.

Clearly, however, data aggregation could be masking important heterogeneities. Upon parsing the data at a finer level, we find our next result:

> Result 2A: *There is some empirical evidence suggesting that non-dealers are influenced by the random anchor when valuing the unexpected good.*
>
> Result 2B: *There is little empirical evidence supporting the claim that dealers can be anchored for either good.*

Table 3 provides the first pieces of evidence to support Result 2. First, examining the data by subject pool, our non-parametric tests yield evidence that the anchor has a modest effect on values for non-dealers. Inspection at the level of treatments makes clear that the result is associated with their valuation of peanuts: in this case, we find a marginally significant effect of the social security number among the *consistent* responders at the $p = 0.09$ level. Yet, as Table 3 indicates, there is little evidence of anchoring for non-dealers valuing the sportscard pack or for dealers valuing either good. In fact, among the

[7] In our regressions, we exclude two influential observations from both non-dealer models. These individuals refuse to sell at anchor values greater than $9.00 but then make offers of fifty cents or less, indicating confusion with or inattention to the protocol. Standard errors are calculated using the White sandwich estimator (White, 1980) since the Breusch-Pagan test detects heteroscedasticity (Breusch and Pagan, 1979).

professional dealers, not only is there no evidence of anchoring, but the mean offers are somewhat greater for those with social security anchors below the median value than for those above. Figure 1 presents the results graphically for each of the four treatments.

An interesting data pattern pertains to why we find little evidence of anchoring for dealers, but some evidence of anchoring for non-dealers. To explore this finding in more detail, we pool the dealer and non-dealer data and consider the effect of market experience. Given that the sample sizes get quite thin at low levels of market experience, we do not formalize a result on market experience, but we discover some interesting tendencies in the data. For instance, for those 14 subjects who have one year or less experience in the market, we find considerable evidence of anchoring. Again splitting the sample around the median SSN, we find that the Mann-Whitney test yields a statistically significant difference ($p = 0.083$) for new market participants that is consistent with the existence of anchored responses. Among this group, the mean offer for those above (below) the median SSN is 6.00 (4.49), a difference of $1.50, or 33%. Furthermore, removing the individual who refused to sell at the SSN price of $9.19 and then offered $0.50 when the BDM protocol was used yields an anchoring effect significant at the level of $p = 0.015$ for the thirteen remaining subjects. Finally, we find no statistically significant differences among the more experienced subjects.

Model 2 in Table 4 provides regression results that support Result 2A and 2B and provide additional insights on market experience. The specification includes indicator variables for the *dealer* subject pool, the peanut treatment (*nuts*) and for new market participatns (*new* - indicating subjects with one year or less experience in the sportscard

markets). Interactions of treatment and experience variables with the social security anchor are also included as well as the demographic controls used in Model 1.

As in the pooled results, the coefficient on *soc* is not statistically significant supporting the nonparametric tests for the non-dealers when valuing the sportscards. The same results hold for the linear combinations of *soc* when interacted with treatment indicator variables.[8] The fact that the *soc* variable in combination with *soc*×*nuts* is not significant (p = 0.51 (*all*), p = 0.64 (*consistent*)) detracts from the robustness of Result 2A regarding the anchoring of peanut valuations by non-dealers.

With regard to market experience, however, the nonparametric results are supported by parametric regression. The *soc* and *soc*×*new* coefficients are jointly significant for both *all* and *consistent* respondents.[9] The coefficient can be interpreted in the context of an incomplete adjustment in the anchoring and adjustment model of Tversky and Kahneman (1974). Their model suggests that people first consider the value of the anchor and then move, though incompletely, toward what would be their unanchored response. Since a coefficient of zero implies complete adjustment, the measured coefficient of 0.536, in the model with all respondents, implies that 1-.536 = .464 is the magnitude of the adjustment towards the true value.[10] While the caveat regarding the small sample remains, the result does suggest that this is an area in which additional research is warranted.

---

[8] Consistent with the fact that dealer valuations are somewhat higher for those with SSN below the median, there is what appears to be a negative anchoring effect associated with the *soc* + *soc*×*dealer* coefficient. Given the one-sided nature of our hypothesis we believe this is an artifact and do not believe we have discovered a new phenomenon of economic significance. The positive coefficient on *soc*×*dealer*×*nuts* restricts this artifact to the dealers in the sportscard treatment.

[9] For *all* (*consistent*) respondents the magnitude and significance of *soc* for *new* market participants s 0.536 and p = 0.005 , (0.565 and p = 0.004).

[10] For all the other treatments the insignificant coefficients imply complete adjustment from the anchored value.

3. Experimental Design and Results: Bilateral Market Experiment

Whether, and to what extent, the anchoring affects observed above influence the operation of markets is an open issue that undoubtedly depends critically on the market institution. For example, making use of the Walrasian tatonnement mechanism, Becker (1962) proved that several fundamental features of economics, such as correctly sloped supply and demand schedules, may result even when agents are irrational, serving to sufficiently relax the utility-maximizing assumption inherent in economic modeling. Similarly, using zero-intelligence traders, Gode and Sunder (1993) illustrate that the efficiency of the double-auctions institution derives largely from its structure rather than from individual learning.

In this section, we garner insights into the effects of anchoring in markets by exploring market outcomes in multi-lateral bargaining contexts. Our market treatments are similar to Chamberlin (1948), as extended recently to naturally occurring markets by List (2004), List and Price (2006), and List and Millimet (2008), of which the design description closely follows. To maintain consistency with the valuation experiment above, we continue experimentation in the sportscard marketplace. In many ways, the sportscard market resembles many early-organized markets, as consumers mill around the marketplace, haggling and bargaining with dealers, who have their merchandise prominently displayed on tables.

In our bilateral market sessions, each participant's experience typically followed four steps: (1) consideration of the invitation to participate in an experiment, (2) learning the market rules, (3) actual market participation, and (4) conclusion of the experiment

and exit interview. In Step 1, before the market opened, a monitor approached dealers at the sportscard show and inquired about their interest in participating in an experiment that would take about 45 minutes. Since most dealers are accompanied by at least one other employee, it was not difficult to obtain their agreement after it was explained to them that they could earn money during the experiment. Non-dealers were recruited from people milling around the marketplace.

Upon agreement of the prerequisite number of dealers (sellers) and non-dealers to participate, monitors thoroughly explained the experiment rules in Step 2. The experimental instructions were standard, and borrowed from Davis and Holt (1993, pp. 47-55) with the necessary adjustments. Before continuing, a few key aspects of the experimental design should be highlighted. First, all individuals were informed that they would receive a $10 participation payment upon completion of the experiment. In addition, following Smith (1964), to ensure that marketers would engage in a transaction at their reservation prices, we provided a $0.05 commission for each executed trade for both buyers and sellers.

Second, the non-dealers were informed that the experiment consisted of five periods and that they would be buyers in the experiment. In each of the five periods, we used Smith's (1976) induced value mechanism by providing each buyer with a "buyer's card" containing a number – known only to that buyer – representing the maximum price that he or she would be willing to pay for <u>one</u> unit of the commodity. Dealers were informed that they would be sellers in the market and, in each of the five periods, that each would be given a "seller's card" containing three sequential numbers – known only to that seller – representing the minimum price that he or she would be willing to sell up

to three units. Importantly, both buyers and sellers were informed that this information was strictly private and that reservation values would change each period. They were also informed about the number of buyers and sellers in the market (explained more fully below) and informed that agents may have different values.

Third, the monitor explained how earnings (beyond the participation and commission payments) would be determined. The difference between the contract price and the maximum reservation price determined the market earnings of buyers; the difference between the contract price and the minimum reservation price determined sellers' earnings. Several examples illustrated the irrationality associated with buying (selling) the commodity above (below) the induced value.

Fourth, the homogeneous commodities used in the experiment were 1982 *Topps* Ben Oglivie baseball cards, upon which decorative moustaches had been drawn, thereby rendering the cards valueless outside of the experiment. Consequently, the assignment given to buyers was clear: enter the marketplace and purchase the Oglivie "moustache" card for as little as possible. Likewise, the task confronting sellers was equally as clear, and an everyday occurrence: sell the Oglivie "moustache" card for as much as possible. The cards and participating dealers were clearly marked to ensure buyers had no trouble finding the commodity of interest. Finally, buyers and sellers engaged in two five-minute practice periods to gain experience with the market.

In Step 3, subjects participated in the bilateral market. Each market session consisted of five market periods, each lasting five minutes. After each five-minute period, a monitor privately gathered the buyers and gave each a new buyer's card; a different monitor privately gave each seller a new seller's card. Note that throughout the

market process careful attention was paid to prohibit discussions between sellers (or buyers) that could induce collusive outcomes. Much like the early writers in this area, we wanted to give neoclassical theory its best chance to succeed. Step 4 concluded the experiment, where subjects were paid their earnings in private.

We follow this simple procedure in each of three treatments. Treatment 1 is the baseline, which represents a similar treatment to those summarized in List (2004). In this case, we include 12 (4) buyers (sellers) who are inexperienced (have varied experience) in the sportscard market. The buyers have unitary demand whereas the sellers have up to 3 items they can sell. Figure 2 presents buyer and seller induced values, which are taken from Davis and Holt (1993, pp. 14-15). In Figure 2, each step represents a distinct induced value that was given to buyers (demand curve) and sellers (supply curve). The extreme point of the intersection of the buyer and supplier rent areas in Figure 2 yields $37 in rents per period, which occurs at the static price/quantity of *Price* = $13 – $14 and *Quantity* = 7.

Treatments 2 and 3, which are the novelty of this experiment, augment Treatment 1 by announcing a previous price that was realized in past experiments. This price is announced to all experimental participants in the following form: "in a previous experiment identical to this one, the first transaction occurred at a price of $X." Previous literature (e.g., Simonson and Drolet, 2004) suggests that once the decision to buy (or sell) has been taken, value judgments "are most susceptible to influence by anchors relating to market prices." Indeed, summarizing the results from four experiments, Simonson and Drolet (2004) support this reasoning and highlight the importance of the source of uncertainty as a moderator of susceptibility to anchoring effects. Thus, given

that our buyers and sellers have certainly taken the step to be buyers or sellers of their good, anchoring the source of uncertainty is important.

In Treatment 2 we announce only one price realization (either a high or a low price), and this announcement takes place directly before market period 1 commences. No extra information is given beyond that given in Treatment 1 from that point onwards. When announcing a high (low) price, we use the second step on the aggregate demand (supply) function: \$18 (\$9). Due to symmetry, this price is \$4 from the equilibrium price boundary of \$14 (\$13).

In Treatment 3, we announce a distinct high or low price directly before each of the five market periods commences. The high price signal is drawn randomly from integers on the uniform distribution [15, 18]; the low price signal is also randomly drawn from integers on a uniform distribution, but over [9, 12]. Accordingly, putting these two treatments together, we can explore both short and long run effects of price anchors. Our usage of announced random anchors as previous market outcomes is directly at the heart of the source of uncertainty in these markets mentioned above. By appropriately choosing plausible realized prices (taken from our previous experiment to avoid deception), we give anchoring its best shot because this announced price might contain important information pertaining to the underlying equilibrium price (indeed, by rewarding the entire source of price variation to anchoring we overestimate the power of anchoring). Alternatively, by following the literature and using the same induced value schedules across all five market periods, our tests represent a demanding one for anchoring.

Experimental Results

Table 5 contains summary statistics for the experimental data. We gathered data from 3 baseline sessions, 6 Treatment 2 sessions (3 high signal and 3 low signal), and 6 Treatment 3 sessions (3 high signal and 3 low signal). Given that there are sixteen unique subjects in each session, our entire design includes data drawn from 240 subjects. Entries in Table 5 provide summary price (quantity) data in the top (bottom) panel. A first insight is that the baseline treatment yields results that suggest the predictive power of supply and demand functionals. This result is in line with previous research, and points to the power of the simple situation of supply and demand curves.[11] Perusal of the data summary for the various treatments yields a first formal comparative static finding:

Result 3: *Price and quantity realizations in bilateral trading markets are influenced by anchors, but the effect is transient*

A first piece of evidence to support Result 3 is that prices realized for the first market trade are crucially linked to the anchor. Whereas the average price in the high anchor Treatment 2 is \$17.70, the average price is only \$9.50 in the low anchor Treatment 2. These differences are statistically significant at conventional levels using a Wilcoxon signed rank test. While the average price differences for the low and high anchor Treatment 2 remain in the first few periods, by period 3 the prices have reasonably converged. Any remaining price differences are small in periods 4 and 5 of Treatment 2. These data patterns suggest that the initial anchor does not have important long-run effects on prices.

[11] We also gathered data in a treatment that used an anchor of \$13.50, the midpoint of the equilibrium prediction of the supply and demand curve intersection. These data did not significantly differ from the baseline treatment data, suggesting that this market can yield efficient outcomes with or without anchors present.

Treatment 3 data provide a different test in that agents receive a fresh signal at the beginning of each market period. Similar to the Treatment 2 data, in this case we again find that in the early periods the signal (high or low) influences prices. For example, the initial trade is $16.30 ($13.30) in the high (low) treatment, and the first few periods show that prices in the high anchor treatment are above those observed in the low anchor treatment. Yet, the signals lose their power in the latter periods, where we find that little difference in prices exists across the high and low anchor treatments. Interestingly, in a regression model that uses the observed price as the regressand, and the signal, market period, and the signal and market period interacted as regressors, we find that in the early periods the signal has a significant influence, but by period 3 the signal no longer has an influence on the market transaction prices. This result suggests that even in the short run anchors do not have considerable influence for those agents who are experienced with the market fundamentals.

Such transient effects are also found when examining quantities traded in the market. In this case, however, there are no observed differences across the high and low price signal treatments: in each instance the market is stifled by the anchor in the early periods. This is due to one side of the market holding out for unrealistic prices, due to the random price signal. Yet, this too wanes, as by the fourth period the expected market quantity is realized in all treatments.

4. Discussion

Over the past three decades, a large amount of experimental evidence has amassed that suggests agents construct their preferences during the evaluation process.

Our current study extends the investigation of anchoring—one of the modalities through which preferences may be constructed—to a field environment. Overall, our approach highlights that using field experiments and/or "special" markets (like those for sportscards) to focus on deep questions that are hard to take on with observational field data, or in markets that are more important *per se*, represents a useful first attempt in the field to learn about fundamental tenets of human behavior.

We investigate anchoring with two complementary field experiments. In our first experiment, we employ an anchoring protocol that has been found in previous experiments to influence statements of economic value (Johnson and Schkade, 1989; Slovic, 1995; Bettman, Luce, and Payne, 1998; Green et al., 1998; Hoeffler and Ariely, 1999, Ariely, Loewenstein, and Prelec, 2003; Bateman et al. 2008). We find evidence that anchoring matters in the valuation exercise. Yet, the anchor only influences ordinary consumer valuations of the good that they did not expect to trade in this particular market setting. Ordinary consumers were not influenced by the anchor when valuing the good they expected to buy, sell, and trade when entering the market. We find that dealers were not influenced by the anchor for either class of goods. That the most intense traders, sportscard dealers, are immune to anchoring for either of the goods provides some limited evidence that the stability of preferences is transferable across domains, for similar trading decisions.

Our results differ from those of Ariely, Lowenstein, and Prelec (2003). Their experiment utilized common consumer products (wines, computer peripherals, books, and chocolates) and salient incentives, but in a laboratory setting with MBA students. Their results support the anchoring hypothesis—they found willingness to pay for these

commodities was correlated with the random anchor. We employ the same protocol (albeit using willingness to accept compensation rather than willingness to pay) in the field, with subjects that self select into the particular market environment. Our subjects have a demonstrable interest in one of the commodities used in our experiment, in contrast to the subjects of Ariely, Lowenstein, and Prelec, and we find no evidence of anchoring for this good. Our other commodity is not one that our subjects would expect to trade at a sportscard show, yet we find only weak evidence of anchoring for this good.

Despite the lack of response to anchoring in aggregate, we do observe suggestive evidence of an anchoring effect among subjects with one year or less experience in the sportscard market. The fact that anchors are effective in this group raises interesting questions for preference formation and price equilibration. Our results are consistent with the discovered preference hypothesis (DPH) of Plott (1996) (Braga and Starmer, 2005). DPH posits that agents in new and unfamiliar decision-making environments may be confused about their best course of action given the alternatives. Observed behavior under these circumstances may appear anomalous and at odds with neoclassical decision theory. With the ability to make repeated choices and receive feedback on the consequences of those actions, however, the behavior of these agents may evolve to more closely resemble that prescribed by normative microeconomic theory. Such an operative mechanism could give rise to the pattern of results we find.

The limited evidence of preference instability for "unexpected" goods and for inexperienced traders notwithstanding, the method by which anchors might influence market outcomes more generally remains an important issue. Previous literature (e.g., Simonson and Drolet, 2004) suggests that when market participants are focused on

trading in goods, their value judgments are susceptible to signals of market price, leading to a situation in which expressed values are shaped by price rather than prices reflecting value. Along these lines, we designed the second field experiment to explore the effect of anchoring in a decentralized bargaining market. We view this as an initial step in exploring anchoring in more realistic trading environments with salient incentives. Our first treatment replicates the standard bilateral market trading experiment (Chamberlin, 1948; Davis and Holt 1993), while treatments two and three introduce anchors before trading is allowed to take place. Rather than employ random price signals, we explicitly identify anchors as initial price realizations from previously-conducted but otherwise similar bilateral trading experiments. In the spirit of Simonson and Drolet's results, the anchor is potentially informative from a trader's perspective. The anchors were chosen to provide either a high or low signal of value. Treatment two employs one initial anchor (high or low) before trading commences, allowing for an examination of the anchor's long run influence on trading decisions. Treatment three allows for an examination of short run effects by offering a new anchor (drawn randomly from a high or low distribution) before each market period.

Results of our bilateral trading experiments suggest that price and quantity realizations are influenced by anchors, but the effect is transient. Prices realizations for the first market trade in both treatments two and three are significantly influenced by the anchor. In each treatment, however, price and quantity realizations converge to neoclassical predictions by the third round. Thus, potentially informative anchors appear to have little influence on aggregate market behavior in a bilateral trading experiment in either the short run, when a new signal is offered up before each trading period, or the

long run when multiple rounds of trading occur after exposure to an anchor. Overall, our results provide evidence that anchoring effects are not persistent in markets with repeated opportunities to engage in exchange within a common, static trading regime.

The use of induced values in our second experiment focuses subject uncertainty on lack of experience with the particular market, rather than the formation of preferences. Future research could integrate our two experiments by exploring the role of commodity type and level of experience on bilateral trading with homegrown values. Such an examination could provide additional insight into the effect of anchors or other anomalies on market outcomes.

5. Conclusions

Many of the standard results of welfare economics—such as the interpretation of market surplus measures, the Pareto Efficiency of perfectly competitive market outcomes, and the rationing and allocative functions of market prices—are predicated on the notion of durable and meaningful consumer preferences. An individual demand schedule should reflect maximum willingness to pay for units of a commodity, *ceteris paribus*. The assumption that preferences are stable has immense normative significance since the correspondence between observed demand and durable preference is at the heart of the application of microeconomic theory to welfare analysis and public policy. The extent to which firm decisions might be influenced by random signals is a topic which has received less attention in the economics, business, and psychology literature (Rothschild 1973; Sterman 1989; Schoemaker 1990), though nonetheless important.

We believe that our results should have some import for economic theorists, applied economists, and practitioners. For example, the study raises questions about how experience transfers across markets, including to new products. In this light, theoretical work that extends the research of Kőszegi and Rabin (2007) to consider market equilibria would be particularly valuable. We trust that future research to further our understanding of the behavior of new market participants and their impact on market outcomes will be of great interest. Lastly, continued exploration of whether, and to what extent, inexperience with valuation of public goods and unfamiliarity with standard valuation protocol affect subjects' responses to survey questions should yield fundamental insights into assessing consumer preferences for public goods as well as provide guidance to scholars engaged in cost-benefit analysis.

References

Ariely, D., G. Loewenstein, and D. Prelec. 2003. "Coherent Arbitrariness": Stable Demand Curves without Stable Preferences. *Quarterly Journal of Economics* 118(1): 73-105.

Bateman, I. J., D. Burgess, W. G. Hutchinson, and D. I. Matthews. 2008. Learning Design Contingent Valuation (LDCV): NOAA Guidelines, Preference Learning, and Coherent Arbitrarienss. *Journal of Environmental Economics and Management* 55: 127-41.

Becker, G. S. 1962. Irrational Behavior and Economic Theory. *Journal of Political Economy* 70: 1-13.

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

Bettman, J.R., M.F. Luce, and J.W. Payne. 1998. Constructive Consumer Choice Processes. *The Journal of Consumer Research* 25(3): 187-217.

Braga, J. and C. Starmer. 2005. Preference Anomalies, Preference Elicitation and the Discovered Preference Hypothesis. *Environmental and Resource Economics* 32(1): 55-89.

Breusch, T. S. and A. R. Pagan 1979. A Simple Test for Heteroscedasticity and Random Coefficient Variation. *Econometrica* 47(5): 1287-94

Chamberlin, E. H. 1948. An Experimental Imperfect Market. *Journal of Political Economy* 56: 95-108.

Chapman, G. B., and E. J. Johnson. 2002. Incorporating the Irrelevant: Anchors in Judgments of Belief and Value. *Heuristics and Biases: The Psychology of Intuitive Judgment.* Eds. T. Gilovich, D. Griffin, and D. Kahneman, 120-38. Cambridge, UK: Cambridge University Press.

Davis, D. D. and C. A. Holt. 1993. *Experimental Economics* Princeton University Press: Princeton, NJ.

Einhorn, H. J., R. M. Hogarth. "Ambiguity and Uncertainty in Prorbabilistic Inference." *Psychological Review*, 1985, (92), 433-461.

Englich, B. and T. Mussweiler. 2001. Sentencing under Uncertainty: Anchoring Effects in the Courtroom. *Journal of Applied Social Psychology* 31(7): 1535-51.

Epley, N. 2004. A Tale of Tuned Decks? Anchoring as Adjustment and Anchoring as Activation. *The Blackwell Handbook of Judgment and Decision Making*. Eds. D.J.

Koehler & N. Harvey, Oxford, UK: Blackwell Publishers.

Epley, N. and T. Gilovich. 2006. The Anchoring and Adjustment Heuristic: Why the Adjustments Are Insufficient. *Psychological Science* 17(4):311-8.

Gode, D. K. and S. Sunder. 1993. Allocative Efficiency of Markets with Zero-Intelligence Traders: Market as a Partial Substitute for Individual Rationality. *Journal of Political Economy* 101(1): 119-37.

Green, D., K. E. Jacowitz , D. Kahneman, and D. McFadden. 1998. Referendum Contingent Valuation, Anchoring, and Willingness to Pay for Public Goods. *Resource and Energy Economics* 20: 85-116.

Herriges, J. A., and J. F. Shogren. 1996. Starting Point Bias in Dichotomous Choice Valuation with Follow-up Questioning. *Journal of Environmental Economics and Management* 30: 112–31.

Hoeffler, S., and D. Ariely. 1999. Constructing Stable Preferences: A Look Into Dimensions of Experience and Their Impact on Preference Stability. *Journal of Consumer Psychology* 8, no. 2: 113-39.

Holmes, T. P. and R. A. Kramer. 1995. An Independent Sample Test of Yea-Saying and Starting Point Bias in Dichotomous-Choice Contingent Valuation. *Journal of Environmental Economics and Management* 29: 121-132.

Hurd, M. 1999. Anchoring and Acquiescence Bias in Measuring Assets in Household Surveys. *Journal of Risk and Uncertainty* 19, no. 1-3: 111-36.

Johnson, E. J. and D. A. Schkade. 1989. Bias in Utility Assessment: Further Evidence and Explanations. *Management Science* 35(4):406-24.

Kőszegi, B. and M. Rabin. 2007. Revealed Mistakes and Revealed Preferences. U.C. Berkley working paper.

Kriström, B. 1993. Comparing Continuous and Discrete Contingent Valuation Questions. *Environmental and Resource Economics* 3: 63-71.

List, J. A. 2004. The Nature and Extent of Discrimination in the Marketplace: Evidence From the Field. *Quarterly Journal of Economics* 119(1): 49-89.

List and Price (2006)

List and Millimet (2008)

Plott, C. R. 1996. Rational Individual Behavior in Markets and Social Choice Processes: The Discovered Preference Hypothesis. *Rational Foundations of Economic*

Behavior. Eds. K. Arrow, E. Colombatto, M. Perleman and C. Schmidt, London, UK: Macmillan and St. Martin's Publishers.

Rothschild, M. 1973. Models of Market Organization with Imperfect Information: A Survey *Journal of Political Economy* 81(6): 1283-1308.

Schkade, D. A., E. G. Johnson. "Cognitive Processes in Preference Reversals." *Organizational Behavior and Human Decision Processes*, 1989, *44* (2), 203-231.

Schoemaker, P. J. H. 1990. Strategy, Complexity and Economic Rent *Management Science* 36(10): 1178-92.

Simonson, I., A. Drolet. "Anchoring Effects on Consumers' Willingness-to-Pay and Willingness-to-Accept." *Journal of Consumer Research*, 2004, *31* 681-90.

Slovic, P. 1995. The Construction of Preference. *American Psychologist* 50(5): 364-71.

Smith, V. L. 1964. The Effect of Market Organization on Competitive Equilibrium. *Quarterly Journal of Economics* 78: 181-201.

Smith, V. L. 1976. Experimental Economics: Induced Value Theory. *American Economic Review Paper and Proceedings* 66: 274-79.

Smith, V. L. 1982. Microeconomic Systems as an Experimental Science. *American Economic Review* 72(5): 923-55.

Sterman, J. D. 1989. Modeling Managerial Behavior: Misperceptions of Feedback in a Dynamic Decision Making Experiment. *Management Science* 35(3): 321-39.

Strack, F., and T. Mussweiler. 1997. Explaining the Enigmatic Anchoring Effect: Mechanisms of Selective Accessibility. *Journal of Personality and Social Psychology* 73: 437-46.

Switzer, F. S., and J. A.Sniezek, 1991. Judgment Processes in Motivation: Anchoring and Adjustment Effects on Judgment and Behavior. *Organizational Behavior and Human Decision Processes* 49: 208-29.

Tversky, A., and D. Kahneman. 1974. Judgment under Uncertainty: Heuristics and Biases. *Science* 185: 1124-31.

White, H. 1980. A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica* 48: 817–30.

Wright, W. F. and U. Anderson. 1989. Effects of Situation Familiarity and Financial Incentives on Use of the Anchoring and Adjustment Heuristic for Probability Assessment. *Organizational Behavior and Human Decision Processes* 44(1): 68-82.

Table 1 Experimental Design and Sample Size

	Non-Dealers	Dealers	Row Totals
Sportscards	Treatment 1 $n = 34$	Treatment 2 $n = 32$	$n = 66$
Peanuts	Treatment 3 $n = 75$	Treatment 4 $n = 46$	$n = 121$
Column Totals	$n = 109$	$n = 78$	$N = 187$

Table 2 Summary Statistics by Treatment

Sportscards

	T1 - Non-Dealers				T2 – Dealers			
Variabl	*Mean*	*St.Dev*	*Min*	*Max*	*Mean*	*St.Dev*	*Min*	*Max*
Offer	3.47	1.56	0	6.00	3.80	2.04	0.50	10
Soc	4.73	2.92	0.09	9.50	5.05	2.94	0.26	9.99
Sell	0.62	0.49	0	1	0.72	0.46	0	1
Education	3.85	1.46	2	6	4.09	1.61	2	6
Age	35.97	12.17	18	70	49.19	13.30	19	74
Gender	0.15	0.36	0	1	0.09	00.30	0	1
Income	61.30	33.80	5.00	125.00	66.98	41.85	5.00	125.00
Mktyrs	13.06	7.89	0	35	19.23	15.83	1	68

Peanuts

	T3 - Non-Dealers				T4 - Dealers			
Variable	*Mean*	*St.Dev*	*Min*	*Max*	*Mean*	*St.Dev*	*Min*	*Max*
Offer	4.18	2.11	0	10	5.43	2.32	1.5	10
Soc	4.62	2.63	0.23	9.47	5.03	3.20	.35	9.99
Sell	0.62	0.49	0	1	0.59	0.50	0	1
Education	4.03	1.61	2	6	4.00	1.72	0	6s
Age	41.20	14.13	19	70	46.33	13.92	19	68
Gender	0.13	0.34	0	1	0.09	0.29	0	1
Income	56.41	35.07	5.00	125.00	62.56	36.44	5.00	125.00
Mktyrs	14.24	10.07	0	50	16.9	11.3	1	50

Notes: Income in thousands of dollars

Income for each respondent is estimated as the median value of the indicated income bracket in the survey.

Table 3: Pooled Data and by Treatment

		All Respondents				Consistent Respondents			
Treatment	*Median Split*	*Mean SSN*	*Mean Offer*	*N*	*P Value*	*Mean SSN*	*Mean Offer*	*N*	*P Value*
Pooled	High	7.25	4.41	92	0.32	7.46	4.46	75	0.35
	Low	2.34	4.17	91		2.53	4.24	75	
Cards	High	7.19	3.58	32	0.82	7.56	3.68	28	0.47
	Low	2.40	3.88	32		2.50	3.80	28	
Nuts	High	7.18	4.99	60	0.11	7.40	5.00	47	0.13
	Low	2.24	4.46	59		2.54	4.44	47	
Nondealers	High	6.81	4.33	53	0.23	7.06	4.44	45	0.09
	Low	2.24	3.86	52		2.35	3.78	44	
Dealers	High	7.70	4.74	39	1.00	7.97	4.23	31	0.90
	Low	2.38	4.78	39		2.71	5.20	30	
Treatment 1	High	6.96	3.69	16	0.96	7.31	3.78	14	0.59
Nondealers/Cards	Low	2.12	3.64	16		2.14	3.54	14	
Treatment 2	High	7.42	3.62	16	0.67	7.81	3.59	14	0.77
Dealers/Cards	Low	2.69	3.97	16		2.85	4.06	14	
Treatment 3	High	6.74	4.54	37	0.30	6.94	4.73	31	0.09
Nondealers/Nuts	Low	2.30	4.02	36		2.44	3.89	30	
Treatment 4	High	7.85	5.36	23	0.87	8.07	4.99	17	0.82
Dealers/Nuts	Low	2.21	5.50	23		2.62	5.94	16	

Notes:

Table 4 OLS Estimates – Anchoring Experiment

Dependent Variable	**Model 1**		**Model 2**	
Offer	All	Consistent	All	Consistent
Soc	0.022	0.016	0.109	0.102
	(0.057)	(0.061)	(0.080)	(0.084)
New			-0.440	0.508
			(0.860)	(1.010)
Soc x New			0.427**	0.448**
			(0.177)	(0.184)
Dealer			1.145*	1.485**
			(0.614)	(0.729)
Soc x Dealer			-0.331**	-0.378***
			(0.133)	(0.141)
Nuts			0.538	0.647
			(0.528)	(0.611)
Soc x Nuts			-0.031	-0.055
			(0.114)	(0.124)
Soc x Dealer x Nuts			0.258**	0.279**
			(0.124)	(0.132)
Education	0.156	0.188	0.232**	0.299**
	(0.101)	(0.121)	(0.099)	(0.115)
Age	0.031**	0.030***	0.035***	0.033**
	(0.011)	(0.012)	(0.011)	(0.013)
Gender	0.161	0.194	0.339	0.416
	(0.552)	(0.095)	(0.462)	(0.503)
Income	-0.137*	-0.194**	-0.169**	-0.237**
	(0.082)	(0.095)	(0.081)	(0.092)
Constant	2.878	3.081	1.622*	1.640*
	(0.879)	(0.961)	(0.901)	(0.975)
N	172	143	172	143
R^2	0.060	0.063	0.184	0.199
F	2.30	1.95	2.78	2.58
Prob>F	0.090	0.047	0.002	0.004

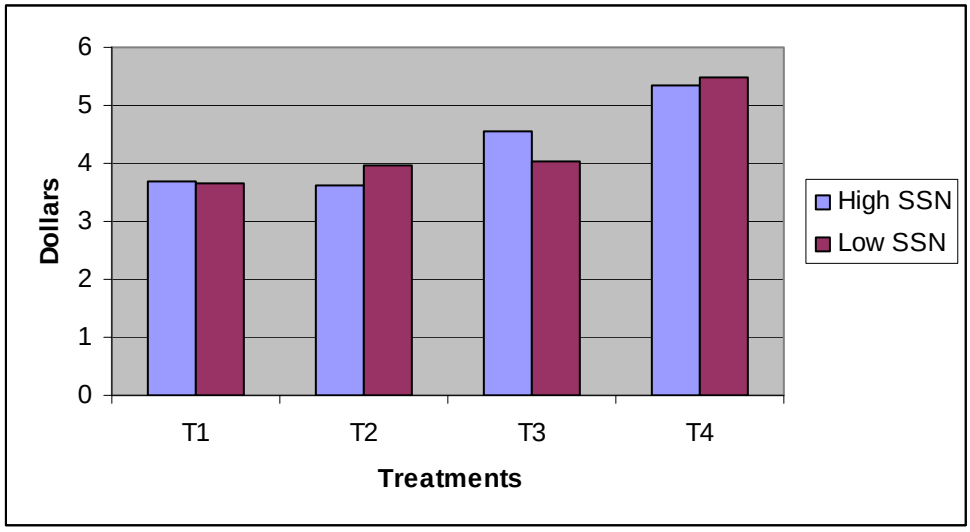
Notes: Standard errors are in parentheses beneath the coefficients. Statistical significance is indicated by: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5 Bilateral Trade Experiment

	Baseline	Treatment 2		Treatment 3	
		High	*Low*	*High*	*Low*
Prices					
First Trade Price	13.7	17.7	9.5	16.3	13.3
	(4.0)	(0.6)	(0.7)	(1.5)	(4.2)
First Period Avg. Price	13.5	17.1	10.0	15.9	11.7
	(2.0)	(0.7)	(1.2)	(1.2)	(2.7)
Second Period Avg. Price	14.0	15.9	11.1	14.5	12.0
	(1.7)	(1.4)	(1.7)	(1.5)	(2.5)
Third Period Avg. Price	13.7	14.4	13.4	14.0	13.2
	(1.9)	(1.8)	(2.1)	(1.4)	(2.4)
Fourth Period Avg. Price	13.8	14.1	14.1	13.9	13.3
	(1.5)	(1.4)	(1.7)	(0.9)	(1.9)
Fifth Period Avg. Price	13.1	13.8	13.5	13.5	13.6
	(1.3)	(1.3)	(1.1)	(1.1)	(1.2)
Quantities					
First Period Avg. Quantity	7.3	2.3	1.7	3.3	3.0
	(0.6)	(1.2)	(1.5)	(2.5)	(2.0)
Second Period Avg. Quantity	8.0	4.3	3.7	5.0	4.3
	(1.0)	(1.2)	(1.5)	(2.6)	(1.5)
Third Period Avg. Quantity	7.0	6.7	5.3	6.0	6.0
	(0.0)	(0.6)	(1.5)	(1.7)	(1.0)
Fourth Period Avg. Quantity	8.0	7.3	7.0	7.7	6.7
	(1.0)	(0.6)	(1.0)	(1.5)	(1.5)
Fifth Period Avg. Quantity	7.3	6.7	7.3	7.3	7.0
	(0.6)	(0.6)	(0.6)	(0.6)	(1.0)

Notes: First trade price is the first executed transaction in the session. For the baseline this price is the average over the 3 sessions; for Treatments 2 and 3 this price is the average over the 5 sessions. The other figures represent the average of the session averages in each of the given periods. *High* and *Low* represent the high and low signal treatments. Standard deviations are in parentheses underneath the means.

Figure 1 WTA for Anchoring Protocol by Treatment: All Respondents



Notes: For each treatment the value off the offers split by median SSN are presented.
T1 – Nondealers/Cards, T2 – Dealer/Cards, T3 – Nondealers/Nuts, T4 – Dealers/Nuts

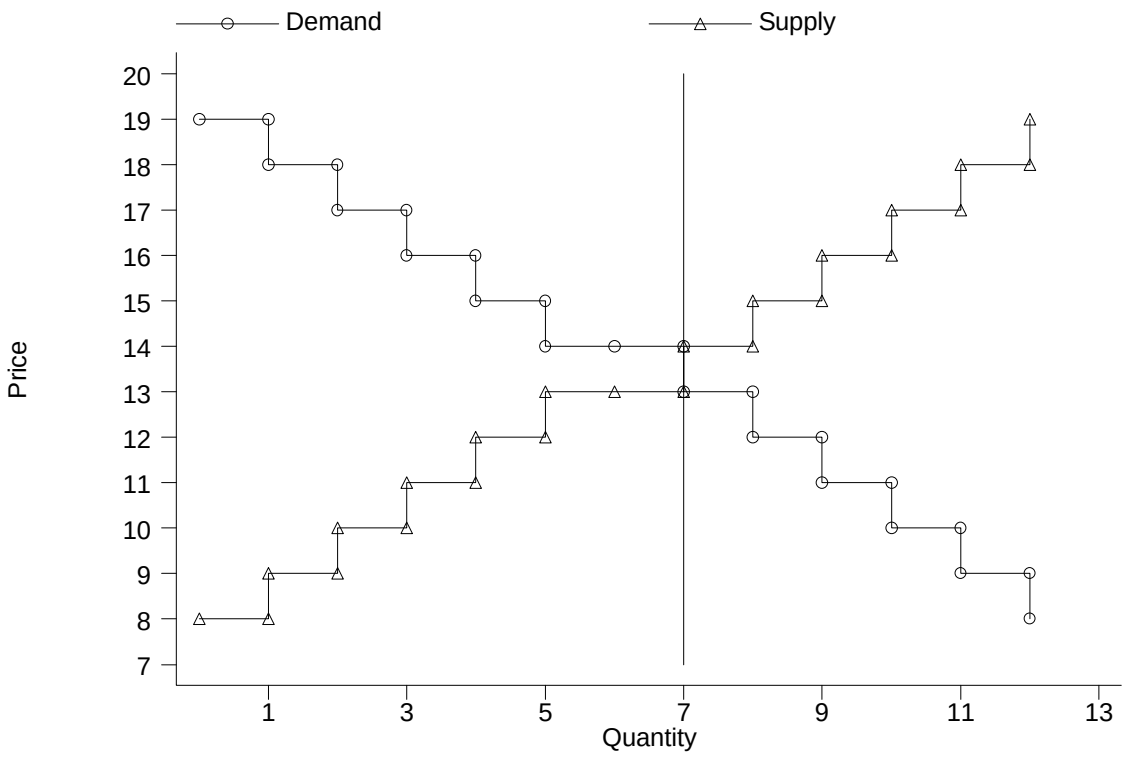


Figure 2. Buyer and seller induced values for market experiment

Appendix

Detailed procedures: anchored valuation question

In this experiment we will ask you three questions.
First what are the last three digits of your social security number?

Please write them here _____

You have been given good "X" and we will now ask you two questions about selling it. After answering the two questions, we will flip a coin and your answer to one of the questions will be carried out. If the coin turns up heads your answer to the first question is used and you will either keep the good or sell it based on your answer. If the coin turns up tails we will use the second question, and you will either keep the good or sell it depending on your answer to that question.

Question 1.
You have the opportunity to sell "X" back to us for $*S.SN*, the value of the last three digits of your social security number converted into dollars and cents.

Would you accept $*S.SN* to sell the good back to us? Yes No

For question 2 you will tell us the price at which you are willing to sell the good. Details of the procedure are on the next page.

Detailed instructions: BDM Individual Choice Elicitation Method

Welcome to Lister's Auctions. You have been given good "X" and have the opportunity to either keep it or sell it back for a price that will be determined in the following way.

I am holding a bag that contains 20 slips numbered 1 through 20. You are welcome to verify this. I am going to ask you to write on the offer sheet a price at which you are willing to sell X. If the number I draw from the bag, lets call it $A, is greater than or equal to the price you have written down you will receive $A and return the good to me. If $A is less than the price you have written on the offer sheet then you keep the good.

With this method of determining the selling price the best thing for you to do is use your true value for the good as the selling price. Let's see why this is true. First consider the case where you offer to sell for less than your true value. Suppose you offer $B, which is less than you really value the good. If the draw of $A is greater than $B but still less than your true value you must sell the good for a price that is less than your value. Your loss is the difference between $A, the price you receive, and your value, which is greater than $A.

Suppose instead that you write on your offer sheet a price greater than your true value. Let's call your offer price $C. If my draw of $A is greater than your true value but less than $C, you keep the good when you would have preferred to sell it and receive $A. The amount of your loss is the difference between your value and $A. Do you have any questions about the selling process?

Please indicate the price at which you are willing to sell the good: _____