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ABSTRACT

There has been a dramatic increase in the use of experimental methods in the past two decades. An oft-cited reason for this rise in popularity is that experimental methods provide the necessary control to estimate treatment effects in isolation of other confounding factors. We examine the relevance of experimental findings from laboratory settings that abstract from the field context of the task that theory purports to explain. Using common value auction theory as our guide, we identify naturally occurring settings in which one can test the theory. In our treatments the subjects are not picked at random, as in lab experiments with student subjects, but are deliberately identified by their trading roles in the natural field setting. We find that experienced agents bidding in familiar roles do not fall prey to the winner's curse. Yet, when experienced agents are observed bidding in an unfamiliar role, we find that they frequently fall prey to the winner's curse. We conclude that the theory predicts field behavior well when one is able to identify naturally occurring field counterparts to the key theoretical conditions.

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One of the main attractions of experimental methods is the control that they provide over factors that could influence behavior. The ability to control the environment allows the researcher to study the effects of treatments in isolation, and hence makes it easier to draw inferences as to what is influencing behavior. In most cases we are interested in making inferences about existing or possible field behavior. We hypothesize that there is a risk that the imposition of an exogenous laboratory control might make it harder, in some settings, to make reliable inferences about field behavior. The reason is that the experimenter might not understand something about the factor being controlled, and might impose it in a way that is inconsistent with the way it arises naturally in the field, and that affects behavior.[1]

We take as a case study the effect of "insiders" on the market phenomenon known as the "winner's curse." For now we define an insider as anyone who has better information than other market participants. The winner's curse (WC) describes a disequilibrium behavior where bidders systematically overbid and thus earn a negative payoff upon winning.[2] The WC arises because individuals fail to correctly process the information about the auction setting. Specifically, they do not take into account the fact that *if they win* then they may have over-estimated the value of the object, and correct their bids for that fact.

If insiders are present in a market, then one might expect that the prevailing prices in the market will reflect their better information. This leads to two general questions about market performance.[3] First, do insiders fall prey to the WC? Second, does the presence of insiders mitigate

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[1] Harrison and List [2004] review the methodological contribution of field experiments, emphasizing their complementarity to more traditional lab experiments.

[2] We will define later a related concept which is more widely used in the experimental literature. Our initial definition of the WC views it as an *ex post facto* condition, whereas the alternative is *ex ante*.

[3] Formal game-theoretic models of bidding in first-price auctions with common values lead to certain specific testable predictions about the effects of insiders. These predictions depend on the precise informational structure of the institution, as one would expect. We review those predictions in section 1.

the WC for the market as a whole?

Our approach is to *undertake experiments in naturally occurring settings in which the factors that are at the heart of the theory are identifiable and arise endogenously, and then to impose the remaining controls needed to implement a clean experiment.* In other words, rather than impose all controls exogenously on a convenience sample of college students, we find a population in the field, a sportscard market, in which one of the factors of interest arises naturally, where it can be identified easily, and then add the necessary controls.[4] To test our methodological hypotheses, we also implement a fully controlled laboratory experiment with subjects drawn from the same field population.

The relevance of field subjects and field environments for tests of the WC is evident from Dyer and Kagel [1996; p.1464], who review how executives in the commercial construction industry appear to avoid it in the field:

> Two broad conclusions are reached. One is that the executives have learned a set of situation-specific rules of thumb which help them to avoid the winner's curse in the field, but which could not be applied in the laboratory markets. The second is that the bidding environment created in the laboratory and the theory underlying it are not fully representative of the field environment. Rather, the latter has developed escape mechanisms for avoiding the winner's curse that are mutually beneficial to both buyers and sellers and which have not been incorporated into the standard one-shot auction theory literature.

This quote highlights that key aspects of the field environment have not been modeled theoretically. This general insight motivated our design.[5] We study the behavior of insiders in their field context, while controlling the "rules of the game" to make their bidding behavior fall into the domain of

[4] Bohm and Lind [1993] make some of the same methodological points in the context of an examination of the relevance of the "preference reversal" anomaly in the field. They find that the anomaly was significantly reduced when using field subjects in a field setting. They do not, however, supplement their analysis by conducting laboratory experiments with subjects drawn from the same field population, as we do. Lichtenstein and Slovic [1973] also undertook experiments with field subjects who had some field experience with gambles, but the objects of study in their experiments were designed to be artificial. Ortmann and Gigerenzer [1997] identify numerous instances in classic psychology experiments where context matters, and explain why it should matter for economics experiments.

[5] Akin to the econometric literature on auctions, Dyer and Kagel [1996] rely on market tests based on non-experimental data.

existing auction theory. In this instance, the term "field context" means the commodity and institution for which they are familiar as well as the type of bidders they normally encounter.

Our design allows us to tease apart the two hypotheses implicit in the conclusions of Dyer and Kagel [1996]. It is plausible to assume that survival in the industry as a dealer provides sufficient evidence that they do not make persistent losses in their natural market setting. Hence, if our insiders fall prey to the WC in our field experiment, then it must be that they avoid it by using market mechanisms other than those we study. We find evidence that is consistent with the conclusion that *dealers in the field do not fall prey to the winner's curse in the field experiment, providing tentative support for the hypothesis that naturally occurring markets are not in disequilibrium because of the WC.* We conclude that the dealers employ heuristics that enable them to avoid the WC, rather than rely *exclusively* on extra-market adjustments in contracts such as those identified by Dyer and Kagel [1996].[6]

This support is only tentative, however, because it could be that these dealers have developed heuristics that protect them from the WC only in their specialized corner of the economy.[7] That would still be valuable to know, but it would mean that the type of heuristics they learn in their corner are not general, and may not transfer to other settings. Hence, we also conducted laboratory experiments, using induced valuations as in the laboratory experiments that Kagel and Levin [1999] conducted with college students, but with field subjects. These laboratory

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[6] In the sportscard market environment, *some* auctioneers allow a full money-back return within a set number of days. This policy applies to dealers and non-dealers, and is usually written. Unwritten conventions exist in trades between dealers, and punishments are relatively easy to effect in future shows or trading opportunities. Price discrimination is widely employed when dealers trade with amateurs, but the degree of discrimination is somewhat limited to avoid the amateurs leaving the market altogether.

[7] For example, Hendricks and Porter [1988] examine "drainage sales" of offshore oil and gas leases, and draw similar conclusions. They are able to exploit knowledge of bidders who are neighbors of the leases being sold, which were known to have produced oil or gas. Such bidders clearly have an asymmetric informational advantage over other bidders on the basis of their own private drilling operations, and bids are consistent with them exploiting that advantage. Their data is also consistent with non-neighbour bidders acting in a strategically cautious manner to avoid the WC.

experiments were designed to examine if the heuristic that dealers apparently use in the field setting transfers to a laboratory setting. Thus we retain our focus on field subjects with experience in the general type of valuation task, but add the controls of a laboratory experiment. We find that their apparent use of a heuristic does indeed transfer when they are acting in familiar roles, adding further support to the claim that *these insiders have developed a "heuristic that travels" from problem domain to problem domain.*

Yet, when dealers are exogenously provided with less information than their bidding counterparts, a role dealers rarely occupy, we find that they frequently fall prey to the WC. We therefore conclude that *the theory predicts field behavior well when one is able to identify naturally occurring field counterparts to the key theoretical conditions.*

In section 1 we review the relevant theoretical predictions. Section 2 reviews previous experimental findings. Section 3 includes a discussion of our experimental procedures and design. Section 4 contains our results. Section 5 concludes.

1. Theoretical Predictions

Economic theory provides game-theoretic predictions of behavior in first price common value actions with certain features. The most important feature for our purposes is the information structure:

- In a Symmetric Information Structure (SIS) auction, each bidder is given a private signal as to the true value of the object.
- In an Asymmetric Information Structure (AIS), the insider knows the value with certainty and the outsiders receive a private signal.

Theoretical bidding behavior in these structures has been studied by Wilson [1967], Weverbergh

[1979], Milgrom and Weber [1982], Engelbrecht-Wiggans, Milgrom, and Weber [1983], Kagel and Levin [1986][1999][2002] and Hausch [1987], amongst others.

Kagel and Levin [1999] provided theoretical predictions for SIS and AIS. Let N denote the number of bidders and let ϵ denote the amount by which the signal could be under or over the true value. For the parameters employed in our experiment, Kagel and Levin [1999] provide five general predictions when bidders employ symmetric Nash equilibrium bidding[8] strategies:

1. *Expected Seller Revenue.* Expected seller revenue in the AIS setting *exceeds* expected seller revenue in comparable SIS settings.
2. *Informational Rents to Outsiders.* Outsiders earn *positive* informational rents in the AIS setting, albeit *less* than they would earn in the SIS setting.[9]
3. *Effects of the Number of Bidders.* Increases in the number of outsiders increases bids by insiders in the AIS setting.
4. *Relative Profitability of Inside Information.* Expected profits of insiders exceed those of outsiders in the AIS setting.
5. *Insider Profits.* Expected profits of insiders are larger in the AIS setting than their expected profits in comparable SIS settings.

We empirically evaluate each of these sets of predictions, which apply to the parameters employed

[8] In the SIS setting the equilibrium bid $b^*(s) = s-\epsilon+\Delta$, where $\Delta=[2\epsilon/(n+1)] \exp[-(n/2\epsilon)(s-(\underline{s}+\epsilon))]$, where s is the signal and $\underline{s}$ is the lower bound of the signal. Since Δ vanishes quickly as s drops below its upper support, and none of our bidders received a private signal close to that upper support, this function calls for bids to be equal to the private signal minus some discounting. Thus, bids are approximately linear in the private signal. This equilibrium bid is appropriate for the domain of signals that our bidders were given; Levin, Kagel and Richard [1996; p.445] provide the general case for wider domains. In the AIS setting there are no general results on equilibrium bid functions, but Kagel and Levin [1999 ; §3] show that an excellent approximation for these parameters and signal domain is provided by $b^*(s) = s-\epsilon$ for outsiders, and $b^*(\check{s}) = \check{s} - (2\epsilon/n)$ for insiders, where $\check{s}$ is the true common value. Thus, again, the equilibrium bids are linear in private signals for a given treatment. Advanced numerical methods will soon allow more flexible bid functions to be generated, permitting asymmetries and heterogeneous risk attitudes.

[9] Unless otherwise noted, all claims about expected rents or expected profits are conditional on winning. Expected profit is zero, by design, conditional on not winning. For the AIS setting, Kagel and Levin [1999] approximated the Nash equilibrium.

here.

Important as predictions about seller revenue and profits are, the main focus of attention in common value settings has been the propensity of different institutions to generate instances of the WC. In standard theoretical models, the WC is not predicted since these models presume that individual bidders take into account the inferential implications of winning the auction. In symmetric settings, where all bidders employ the same bid function, winning the auction implies that you received the highest signal. To the extent that unbiased signals can exceed true values, the intuition is that there is some chance that you have over-estimated the value of the object. Thus, conditional on winning, the rational bidder should "shave" his bid to allow for this additional information. More formally, the expectation of the first order statistic of the signals, conditional on the true value, must exceed the true value, so this extra information above and beyond the signals themselves should be used to decide on the optimal bid.

The introduction of insiders might be expected to change the possibility of a WC emerging. As a general matter, the fact that one or more bidders has better information should lead to more informationally-efficient outcomes, assuming that there is more information available to the market than otherwise. This fact would imply less evidence of a WC when insiders with perfect information are added. Alternatively, the fact that some bidders are known to have better information might lead outsiders to bid more aggressively, in the belief that they have to overcome the informational dis-advantage they face. Neither prediction follows directly from traditional auction theory, which assumes the absence of the WC, but each can be evaluated using a controlled experiment.[10]

Kagel and Levin [1999; p.1223ff.] define a number of WC thresholds, which are bid levels

[10] The informational role of insiders has been studied extensively in the older experimental asset market literature (e.g., Plott and Sunder [1982] and Friedman, Harrison and Salmon [1984]).

that signal the bidder faces a certain or expected WC outcome. For SIS auctions, the natural definition is where the bid exceeds the expected value of the object conditional on the signal received being the highest. Bids in excess of this threshold ensure negative expected profits to the bidders.[11] We use this WC threshold for SIS auctions.

For AIS auctions, two bidding thresholds are offered by Kagel and Levin [1999; p. 1223, 1225]. The first threshold is obtained by assuming insiders do not best-respond to bids of outsiders; this threshold is similar to the WC threshold for SIS auctions and ensures that outsiders in an AIS auction, bidding in excess of it, would earn negative expected profits.[12] However, it is conservative in the sense that it does not allow for insiders adopting best responses to the bidding rules adopted by outsiders. If outsiders bid *at* this conservative WC threshold, then they would be expected to earn negative expected profits; in this sense it is a conservative threshold. If one posits a model in which outsiders are boundedly rational in the sense of using a simple linear bid function defined over their signal and the amount by which their signal could over-estimate the true value, and allow insiders to best-respond to this bidding rule, then it is possible to derive a tighter WC threshold for outsiders such that bids equal to it earn zero expected profits. Hence, bids in excess of this WC threshold earn negative expected profits for outsiders, but it is not conservative in the same sense as the previous threshold.[13] The approximations underlying this threshold are generally excellent ones for the parameter space considered here.[14] Unless otherwise noted, we use the tighter WC threshold for the AIS auctions.

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[11] This threshold equals the signal received by the bidder minus $\gamma\ \epsilon$, where γ equals (N-1)/(N+1) for N bidders. Hence when N=4, γ=0.6, and when N=7, γ=0.75. This expression presumes the auction structure employed in our experiments (e.g., the use of uniform distributions to generate the true value and the signal value).

[12] This threshold is the same as the WC threshold for the SIS auction, but with N defined as the number of outsiders bidding in the auction.

[13] This threshold equals the signal value minus $\lambda\ \epsilon$, where λ=0.690 when N=4 and λ=0.825 when N=7. Thus $\gamma<\lambda$ for each case of N, so this WC threshold is tighter than the conservative WC threshold.

[14] See Kagel and Levin [1999; p.1224/5] and Laskowski and Slonim [2000].

2. Previous Experimental Evidence

Previous laboratory experiments have shown that the WC is robust, particularly with inexperienced bidders. They have also shown that the WC results in several key comparative static predictions being violated in observed behavior with inexperienced bidders. Kagel and Levin [1986] demonstrated that the provision of public information in SIS auctions did *not* raise seller revenue with inexperienced bidders, as theory would predict. Again in a SIS environment and with inexperienced bidders, Levin, Kagel, and Richard [1996] demonstrated that the English and first-price auctions yielded similar revenues, which is inconsistent with theoretical predictions. Only with intense experience does the WC decline in SIS laboratory auctions, as demonstrated most thoroughly by Dyer, Kagel, and Levin [1989]. Thus the laboratory experiments with SIS auctions points to the importance of understanding the extent of "experience" in field settings to which one might apply the theory.

Turning to AIS auctions, Kagel and Levin [1999] find that inexperienced bidders continue to fall prey to the WC, and that virtually none of the theoretical comparative static predictions comparing AIS and SIS hold. Thus, adding an "insider" with better information about the true value of the object does not lead to a large enough reduction in the incidence of the WC such that the predictions of theory are confirmed. Behavior is substantially more consistent with "super-experienced" bidders, who exhibit far less WC bidding behavior, and for these bidder types virtually all of the predictions of theory are supported. In their laboratory settings, super-experienced means that the subject had participated in at least two previous experiments with first-price common value auctions and the same number of bidders.[15] Furthermore, no bidder who went bankrupt in early

[15] It is not clear if the previous experiments used the same SIS or AIS treatment in which the super-experienced subjects participated.

trading in the prior experiments was recruited into the super-experienced pool. Nor would one expect traders who made tiny profits to volunteer to return.

Our experiment is designed to focus on the differences identified by Kagel and Levin [1999] between "inexperienced" and "super-experienced" bidder behavior, particularly when "insiders" are included in the auction. Our field setting provides an opportunity to define these terms in a manner that is more natural and role-specific than normally encountered in a lab environment.

3. Experimental Procedures

We recruit subjects from a well-functioning marketplace, the floor of a sportscard show, using essentially the same general procedures explained in List and Lucking-Reiley [2000] and List [2001]. All experiments were run in Tucson, Arizona. The advantage of this field sample is that we can readily identify individuals who are "dealers" and those who are "non-dealers." The former typically have much more experience in these settings.

We run two types of experiments. One type is a traditional laboratory experiment with induced values defined over an abstract commodity, but using field subjects. The other type is a field experiment with homegrown values with which the field subjects are familiar.

A. Laboratory Experiment With Field Subjects

Each participant's experience in the laboratory experiment followed two steps: considering the invitation to participate in an experiment that would take about 1 hour, and participation in the experiment.

In the first step, the experimenter approached potential subjects entering the trading card show and inquired about their interest in participating in an experiment that would take about 1 hour. If the individual agreed to participate, the experimenter briefly explained that in return the subject would have the chance to earn a considerable amount of money. The administrator further explained that at a pre-specified time on the Saturday or Sunday of the show, the subject should enter an adjacent room to take part in the experiment. Directions to the room were provided and the subject was informed that she would receive instructions for the experiment when she arrived. Unlike the field experiments discussed below, these treatments should not be considered field experiments in the strict sense. Rather, they should be considered a laboratory experiment with a field subject pool, or an "artefactual" field experiment in the language of Harrison and List [2004].

The second step began when subjects entered the room and signed a consent form in which they acknowledged their voluntary participation in the experiment and agreed to abide by the rules of the experiment. Subjects were randomly allocated into treatments based on the time that they participated, and each subject participated in only one treatment.[16]

In these experiments we generally followed the procedures and instructions typically used in previous experiments with common value first-price auctions (e.g., Kagel and Levin [1999]), although our instructions are considerably streamlined in comparison to those used previously. Appendix A lists the written instructions provided to subjects. First, we chose a range of commodity values, in this case between \$40 and \$200. Second, we randomly selected the common value. This was \$94.33, and for simplicity and control was used for all treatments. Third, we computed the subject's signal via a random number generator using $\epsilon=6$ or $\epsilon=12$, depending on the treatment. Fourth, subjects were randomly assigned to a treatment with either N=4 or N=7 bidders. Variations in ϵ or N should have predictable effects on bidding behavior. Fifth, several dry practice runs were carried out in each treatment to familiarize subjects with the rules of the auction. The experiment

[16] A pre-experiment survey provided information on whether the subject was a dealer or non-dealer.

was not begun until everyone understood the rules and finished every example.[17] Finally, to ensure that subjects went home with gains, we ran a second experiment that was not announced until after the first experiment was complete.

B. The Field Experiment

In the field experiment subjects drawn from the field are asked to bid on a commodity that is familiar in this field setting. Each subject's participation in the field experiment followed four steps: (1) inspection of the good, (2) learning the rules, (3) bidding, and (4) conclusion of the transaction.

In step 1, a potential subject approached the experimenter's table and inquired about the sale of the 1990 unopened pack of *Leaf* Series 1 wax baseball cards displayed on the table. An unopened pack of 1990 *Leaf* baseball cards retailed for about $10 at the time of the experiment, although it could be obtained on the web for $9 (or even $8 if purchased in bulk).[18] Each pack could contain thousands of dollars of sportscards. The pack is sealed by the factory, and has not been opened, although subjects can view the quality of the pack personally. This visual inspection provides bidders with their own beliefs about the various probabilities of getting certain cards of high quality. These characteristics, coupled with the fact that subjects informed us that they planned to open the pack and sell its contents if they were deemed winners, provides our field auctions with a common value component. Our knowledge of this market makes it clear that experienced traders know the common value aspect of an unopened pack of cards, and indeed many have participated in

[17] The dry practice runs included at least 10 common value auctions in which subjects received a signal, submitted a bid, and were informed of their subsequent payoff. Examples were run until subjects were confident with the rules of the auction. There was no deception in our experiments of any kind.

[18] See HTTP://WWW.BBCKID.COM for a popular web site that has unopened material.

unopened pack auctions and undoubtedly have been "stung" by the WC. Previous experimental auctions with sportscards, such as List and Lucking-Reiley [2000] and List [2001], used actual cards and not packs of unopened cards.

Furthermore, the company producing the pack, *Donruss*, goes to great lengths to ensure that the "collation" of cards is random. In fact, the collation is so good that each pack is effectively a random sample without replacement in our experience. There were 264 cards in the 1990 *Leaf* set, and 10 cards per pack; each "set" refers to a group of players included in the production run. Thus, since Sammy Sosa's rookie card is in the *Leaf* set, there is a 10/264 = 0.038 probability of getting a Sosa card in the auctioned pack. A Sosa card in Gem-Mint condition retailed for more than $350 at the time of our experiment, although a Gem-Mint grade is given to less than 10% of cards.

As mentioned above, all participants stated that if they won the auction they planned to open the pack. Thus they each have their own private signals about the various probabilities of any given card (of certain quality) being included in the pack and the total value of the cards in the pack. The experimenter then invited the potential subject to take about five minutes to participate in an auction. If the individual agreed to participate, he could pick up and visually examine the unopened pack. The experimenter worked one-on-one with the participant, and imposed no time limit on his inspection of the cards.

In step 2 the administrator gave the participant an instruction sheet that consisted of the auction rules for a standard first price auction. This instruction sheet is reproduced in Appendix B. Several examples of the auction were carried out to ensure that a wide range of subjects would understand the auction rules. No decisions were made until the subject fully understood the rules and her task. Each subject was told in the auctions how many bidders would be in their auction.

After having her questions answered, in step 3 the participant placed her bid on the sheet provided. Finally, in step 4 the experimenter explained that the subject should return at a specified time to find out the results of the auction. Transactions took place at 6p.m. on Saturday and Sunday. If a subject did not return for the specified transaction time, she would be notified; after we had received her payment, she would receive her unopened pack of cards within three days via standard postal service, with postage paid by the experimenter. All winners paid, as agreed.

Again, no subject participated in more than one treatment, and subjects were randomized into treatments by changing the treatment every hour. Hence subjects' treatment type was determined based on the time they visited the table at the card show.

C. Treatments

In our laboratory experiment conducted with the field sample, two general treatments were examined. To re-define the key acronyms, the first was a Symmetric Information Structure (SIS) auction in which each person is given a signal. The second was an Asymmetric Information Structure (AIS) auction in which the insider knows the value with certainty, and the outsider gets a private signal. Every subject in the AIS auction knew that there was one, and only one, insider, and that the insider had perfect information. To ensure comparability with traditional laboratory experiments, our experiment includes treatments with N=4 or N=7 bidders.

Moreover, to examine bidding behavior across subjects who are placed in both familiar and unfamiliar roles, in the AIS treatments we randomly allocate dealers and nondealers into the insider and outsider roles. This characteristic of our experimental design permits us to learn about the consequences of exogenously placing a subject in the opposite role of the one they typically fill in the field.

In our field treatments we examined the natural counterparts of the SIS and AIS conditions.

Specifically, in the field SIS condition, dealers are paired with dealers and non-dealers are paired with non-dealers, and everyone is informed of this composition. In the field AIS condition we matched three non-dealers with one dealer, and again everyone is informed of this fact. All of the field treatments used N=4.

All treatments were one-shot, since that is the bilateral, "one-on-one" trading context typically encountered in the field. Of course, dealers typically interact repeatedly with traders during a trade show. Table 1 reports the samples collected in each treatment, and also summarizes the overall design.

4. Results

A. Laboratory Experiment

Bidding Behavior

The most straightforward metric to evaluate bidding behavior is the bid itself, and then the difference between the bid and the WC thresholds defined earlier. Figure 1 uses kernel densities to summarize bids across SIS and AIS treatments (left and right panels, respectively), and for dealers and non-dealers (top and bottom panels, respectively). Figure 2 compares the differences between observed bids and the theoretical equilibrium prediction, and Figure 3 presents the difference between bids and the WC threshold. The results are sharp. In summary, we see that *in both SIS and AIS settings, dealers provide lower bids than non-dealers, leading to non-dealers falling prey to the WC much more often than dealers.* This general result is discussed more fully below.[19]

First, consider bidding patterns in the SIS treatments, since these are the auctions which

[19] While we focus on a regression analysis to illustrate our results, non-parametric tests provide qualitatively similar insights.

have the richest set of experimental evidence from student samples. In the N=4 treatments, dealers bid on average $2.23 less than non-dealers, and this was almost entirely due to their processing of the information about the signal uncertainty (ϵ). In both ϵ cases, bids were equal to the signal received minus some fraction of the signal uncertainty. Non-dealers reduced their bids by only 40% of this uncertainty, whereas dealers reduced their bids by 93% of the signal uncertainty. In the N=7 treatments, dealers bid nearly $2.40 lower, again due to differential shading of the signal uncertainty (by 88% versus 82%). We conclude that dealers appear to be paying more attention to the signal uncertainty than non-dealers, which suggests that they are less likely to fall prey to the WC.

Turning to the AIS bidding patterns, in the N=4 treatment, dealers bid $3.50 lower than non-dealers, again due to differential shading of the signal uncertainty (by 72% versus 50%). In the N=7 treatments, dealers bid $3.92 lower than non-dealers due to differential shading of the signal uncertainty (by 66% versus 55%). Thus we observe the same general pattern in SIS and AIS settings in terms of the differences in bidding patterns by dealers and non-dealers.

Table 2 displays regression results to confirm these conclusions. The dependent variable is the level of discount: observed bid minus the signal. The explanatory variables are the parameters of the auction, interacted with whether the individual was a dealer. We observe the striking result in the SIS auctions that dealers react differently than non-dealers: an F-statistic of 2.15, with a *p*-value of 0.096, indicates the overall value of dealer-specific parameter estimates. This difference manifests itself primarily in two variables. First, as the signal increases non-dealers discount less, whereas dealers do not, consistent with the theory based on rational bidding presented earlier. Second, dealers are significantly more sensitive to the size of uncertainty around the signal, again in contrast to non-dealers. These results reveal that dealers simply do not bid lower for some unexplained reason, but that they appear to be responding to the parameters of the auction in the direction predicted by theory.[20]

The results in Table 2 for the AIS auctions are less striking, since dealers are either unable to exploit their naturally occurring informational advantage when they are insiders, or are in an unaccustomed position when they are outsiders. In each case, according to an F-test, their behavior is not significantly different than non-dealers. We return below to the inference we make from this result. The only significant variable in these models relates to uncertainty, and we find that both bidder types discount to a greater degree when uncertainty increases.

Winner's Curse

These observed bidding patterns map directly into the incidence of the WC, which is summarized in Figure 4. Figure 3 shows the average difference between the bid and the WC threshold, to provide some sense of the monetary significance of the violations underlying Figure 4.

The first result from Figure 4 is that the *WC incidence is much higher in the AIS auctions compared to the SIS auctions*.[21] This difference is not affected by signal uncertainty, as bidding data across $\epsilon=6$ and $\epsilon=12$ are statistically indistinguishable. Similarly, the number of bidders does not make a considerable difference. Employing comparable WC thresholds, these findings are consistent with those reported by Kagel and Levin [1999; Table II, p. 1227] for inexperienced subjects.

The second result is that the *WC incidence is significantly lower for dealers compared to non-dealers*. This holds whether one is looking at symmetric or asymmetric information environments.

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[20] If we remove the dealer interaction terms, the coefficient on Dealer is -2.48, with a *p*-value of less than 0.001, indicating that dealers bid less than non-dealers. The power of the experimental method, in this case using artefactual field experiments, is that we can identify the difference as due to their reactions to the parameters of the task rather than some gut instinct.

[21] Unless otherwise stated, all claims are supported by statistical tests at the 1% level. For unconditional comparisons, we employ a two-sample *t*-test, assuming unequal variances, and a two-sample Wilcoxon rank-sum test. Conditional comparisons of regression coefficients employ an *F*-test.

The third result is that *when dealers are placed in an unfamiliar role they perform relatively poorly*. The WC incidence for dealers increases as we compare SIS auctions to AIS auctions, and this is a statistically significant difference. In the SIS environment, dealers know that they are not at an informational dis-advantage, and that no other trader knows more than them. But in the AIS environment they know that they are the "informational underdog" when they are not the insider, and this is an unfamiliar role for them.[22] Controlling for the number of bidders and signal uncertainty, moving from the SIS to the AIS environment is associated with an increase in the WC incidence for dealers of 24%.[23] This result is statistically significant, with a p-value less than 0.001.

One concern with our measure of the incidence of the WC is that it captures the number of violations of the WC, and hence provides a qualitative measure of the WC, but it does not reflect the quantitative magnitude of those violations. To some extent the analysis of bid deviations from Table 2 accomplishes that, but the two are not exactly the same. If we examine the dollar difference between the observed bid and the WC threshold, we find that the first two conclusions stated above are confirmed, but the third is not. That is, we do not find a statistically significant effect of the type of auction environment (SIS or AIS) on the quantitative extent of the WC among dealers, even though we do see an effect on the qualitative extent of the WC. In part this is due to the relatively small magnitude of the WC violations by dealers, which range between -6% of bids to just over +8% of bids. We view these two measures of the incidence of the WC as complementary, since each addresses a slightly different question of interest.

[22] The WC incidence is evaluated only for outsiders, since it is meaningless for insiders because they know the true value. Thus, the dealers underlying the WC incidence in the right panel of Figure 4 are all outsiders. No insiders bid more than true value for the good.

[23] These are marginal effects from a probit regression model.

Seller Revenue

Turning to the equilibrium comparative static hypotheses, we find that the seller revenue predictions are generally borne out in the data. Table 3 displays a regression of seller revenue across the 73 distinct laboratory auctions, with various controls added.

Consider the top panel of Table 3, which reports empirical results for the full sample. The relevant variable for the hypothesis test is AIS, which represents the AIS information condition. Theory predicts that seller revenue will be higher in the AIS setting compared to the SIS. Since SIS is the omitted information category in this regression, these estimation results can be used to directly test these hypotheses. We find strong support for the hypothesis regarding the positive effect on revenue of private information, even if the quantitative effect is small: on average, seller revenues increase by $2.11 over the SIS average of $92.59, and this increase is statistically significant with a *p*-value of 0.03.

The remaining panels of Table 3 examine possible interaction effects between the number of bidders and the signal size. The positive effects of private information on revenue are present in each case considered, but are particularly strong with a smaller number of bidders or smaller signal uncertainty.

Bidder Profits

Economic theory also adequately organizes the observations on individual bidder profits. Simple *t*-tests allow us to examine the theoretical predictions for the profits of winners, defined here as those who submitted the highest bid in any group.[24] We cannot reject any of the hypotheses stated

[24] In cases where two or more bidders tied with the highest bid, the actual winner in each auction was selected at random from amongst those who submitted the highest bid. This fact was explained to all participants through one of the examples.

earlier. Specifically, insiders earn much more than outsiders as a general matter, insiders in AIS settings earn much more than bidders in comparable SIS settings, and outsiders earn less in AIS settings than bidders in SIS settings.

B. Field Experiments

Since our field experiments used home-grown values, there is no WC threshold that can be defined. However, we can examine patterns of bidding levels to gain insights into whether the various theoretical predictions are met. Figure 5 displays the bid distribution in the SIS and AIS treatments for dealers and non-dealers. Again, one sees from a vertical comparison of the bid distributions that *dealers generate lower bids than non-dealers in the same setting*.[25] This result holds in each of the SIS and AIS treatments. These conclusions are supported by regressions reported in Table 4, which regress auction bid on a constant and a dealer indicator. Dealers in the SIS auctions bid $1.21 lower than non-dealers on average, although this is only a significant difference at the 11.4% level. In the AIS auctions, dealers bid $2.31 lower than non-dealers, and this is statistically significant.

From the horizontal comparison of the bid distributions in Figure 5 we see that non-dealers bid more aggressively when an insider is added. In fact, they bid on average $1.46 more in the AIS setting, and this is significantly different from zero at the 3.7% level. Recall that the equilibrium bid functions derived earlier suggest that moving from an SIS to an AIS environment should cause *outsiders* in the AIS environment to bid approximately the same as *bidders* in the SIS environment, providing their signals are below their upper support. In the field experiment dealers are insiders and non-dealers are outsiders. So we expect non-dealers to bid roughly the same in the SIS and AIS field experiments, and

---

[25] We infer that such bidding patterns occur due to the WC avoidance. Of course, in a natural field experiment of this sort we do not have the necessary control to eliminate other alternative hypotheses, such as differences in risk posture, education, etc. This highlights the importance of our complementary lab results, which provide greater control and insights consistent with the field experiments.

they do not. Figure 5 suggests that there is *some* evidence that dealers might bid more aggressively as well. In fact they bid $0.57 more on average when insiders in the AIS treatment than when they are ordinary bidders in the SIS treatment, but this difference is not statistically significant at any conventional level. Theory predicts that they should discount their signal by less when they are the insider than when they have the same information as any other bidder. But the difference is predicted to be only one-half of ϵ, the amount by which the signal might be above or below the true signal. Since ϵ is intrinsic and subjective in the field, we do not know *a priori* if it is large or small in relation to the signal. If it is small, then the observed bidding behavior is consistent with theory; if it is large, then observed behavior is inconsistent with theory.

Figure 6 displays the average seller revenue across the three types of field treatments. There is a large and significant increase in seller revenue in the AIS treatments compared to the SIS auctions with dealers, but not with respect to the SIS auctions with non-dealers. This result is consistent with the evidence from lab experiments that reveals dealers shave their bids significantly more than non-dealers in common value settings.

No precise tests of the profitability hypotheses are possible in the field setting, since we do not know the true value of the object (it is a subjective certainty-equivalent for each bidder). However, if we take the current market price of $9 as a measure of the current value of the object, or alternatively as the informationally efficient estimate of the actuarial value, then we can derive some tests of the profit hypotheses conditional on that assumption.

We find that outsiders do *not* earn positive profits in the AIS auctions, and actually earn *less* than their SIS counterparts, although neither result is statistically significant at conventional levels. Although insiders tend to earn less than outsiders in the field experiments, our data reveal *very* few insiders (dealers) winning these auctions when there is a mix of insiders and outsiders in the same auction. In fact, 10 outsiders win for every 1 insider that wins in the field AIS auctions. This is a corollary of them being able to avoid the WC, by bidding lower than outsiders (non-dealers). We are unable to report any statistical tests of this hypothesis, since the data provide so few realizations of dealers winning these auctions.

5. Conclusion

Auction theory provides a rich set of predictions concerning bidders' behavior.[26] One particularly salient finding in a plethora of laboratory experiments that is not predicted in first price common value auction theory is that bidders commonly fall prey to the winner's curse. Only "super-experienced" subjects, who are in fact recruited on the basis of not having lost money in previous experiments, avoid it regularly. This would seem to suggest that experience is a sufficient condition for an individual bidder to avoid the winner's curse. We show that this implication is supported when one considers a natural setting in which it is relatively easy to identify traders that are more or less experienced at the task. In our experiments their experience is either tied to the commodity, the valuation task and the use of auctions (in the field experiments with sportscards), or simply to the use of auctions (in the laboratory experiments with induced values). In all tasks, experience is generated in the field and not the lab. Thus we provide support for the notion that context-specific experience does appear to carry over to comparable settings, at least with respect to these types of auctions.[27]

[26] One weakness is the absence of general results when one allows for risk aversion and affiliated values. For example, Kagel and Levin [2002; p.348] note that, "Unfortunately there is no clear cut predictions as to the effects of risk aversion on the equilibrium bid function. Depending on the nature and degree of risk aversion, equilibrium bids could be greater or less than the SRNNE [symmetric risk-neutral Nash Equilibrium]." On the other hand, failing to control for risk aversion does not generate known biases in the direction of equilibrium bids, as it does with first-price independent private value auctions (e.g., Harrison [1990]). Thus it might be viewed as just adding noise to our design, comparable across treatments. One caveat to this perspective is that the stakes in our lab and field experiments were different, so one might a priori expect more risk aversion to be present in the lab setting than in the field setting due to the higher monetary stakes. We expect that advanced numerical methods may be used to address this general weakness in the future.

[27] These results are strikingly complementary to findings from laboratory experiments reported by Charness and Levin [2006]. They sequentially strip away all strategic aspects of the environment that leads to the WC. Their results "... suggest that the origin of this phenomenon must stem from some form of bounded rationality, such as the decision-maker's failure to recognize that a 'future' event *per se* is informative and relevant for their current decisions,

Our experimental design emphasizes the identification of a naturally occurring setting in which one can control for experience in the way that it is accumulated in the field. Experienced traders gain experience over time by observing and surviving a relatively wide range of trading circumstances. In some settings this might be proxied by the manner in which experienced or super-experienced subjects are defined in the lab, but we question whether standard lab settings can reliably capture the full extent of the field counterpart of experience. This is not a criticism of lab experiments, just their domain of applicability.

The methodological lesson we draw is that one should be careful to generalize from the evidence of a winner's curse by student subjects that have no experience at all with the field context. Our results do not imply that *every* field context has experienced subjects, like our dealers, that avoid the winner's curse. Instead, they point to a more fundamental need to consider the field context of experiments before drawing general conclusions. *It is not the case that abstract, context-free experiments provide more general findings if the context itself is relevant to the performance of subjects.* In fact, one would generally expect such context-free experiments to be unusually tough tests of economic theory, since there is *no control for the context that subjects might themselves impose on the abstract experimental task*.

The main result is that if one wants to draw conclusions about the validity of theory in the field, then one must pay attention to the myriad of ways in which field context can affect behavior. We believe that conventional lab experiments, in which roles are exogenously assigned and defined in an abstract manner, cannot ubiquitously provide reliable insights into field behavior. One might be able to modify the lab experimental design to mimic those field contexts more reliably, and that would make for a more robust application of the experimental method in general.

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compounded by poor updating when this idea is even considered" (p.11). If this is true, then it provides one explanation for one of our key findings, that the ability of dealers to avoid the WC in their natural setting "travels" to the more abstract setting of our lab experiments, providing the environment is similar enough to the one they naturally participate in.

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Table 1: Experimental Treatments and Sample Sizes

Information Treatment	∈	Lab		Field
		N=4	N=7	N=4
Symmetric (SIS)	12	48	49	
	6	48	49	
	Intrinsic			64
Asymmetric (AIS)	12	56		
	6	56	49	
	Intrinsic			60

Figure 1: Bids in Laboratory Experiments

True Common Value = $94.33

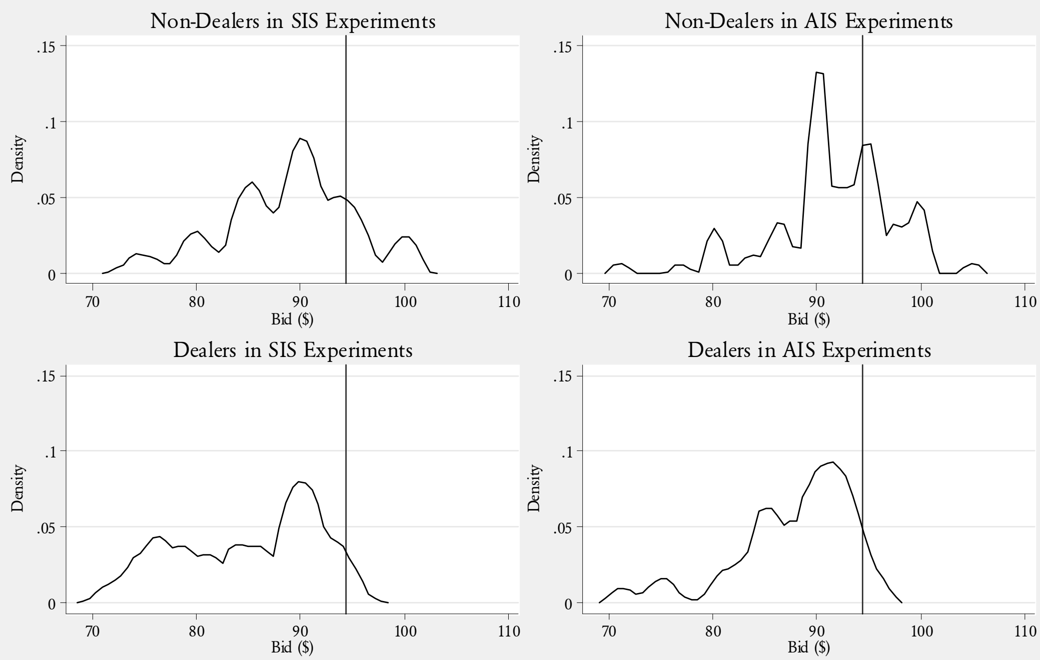


Figure 2: Equilibrium Bid Deviations in Laboratory Experiments

Bids in Excess of the RNNE Prediction

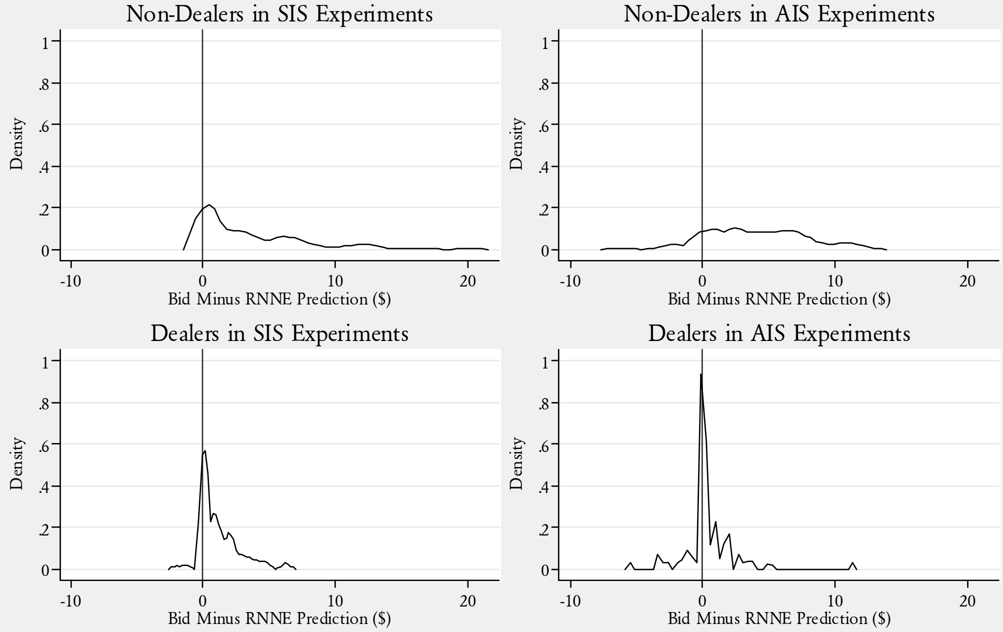


Figure 3: Excess Bids in Laboratory Experiments

Bids in Excess of the Winner's Curse Threshold

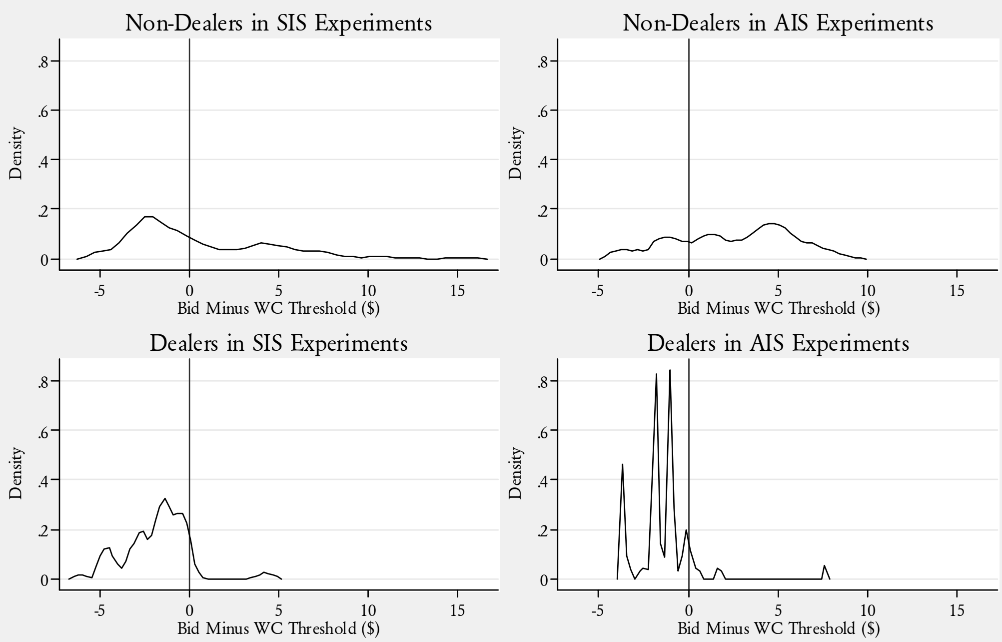


Figure 4: Incidence of Winner's Curse in Laboratory Experiments

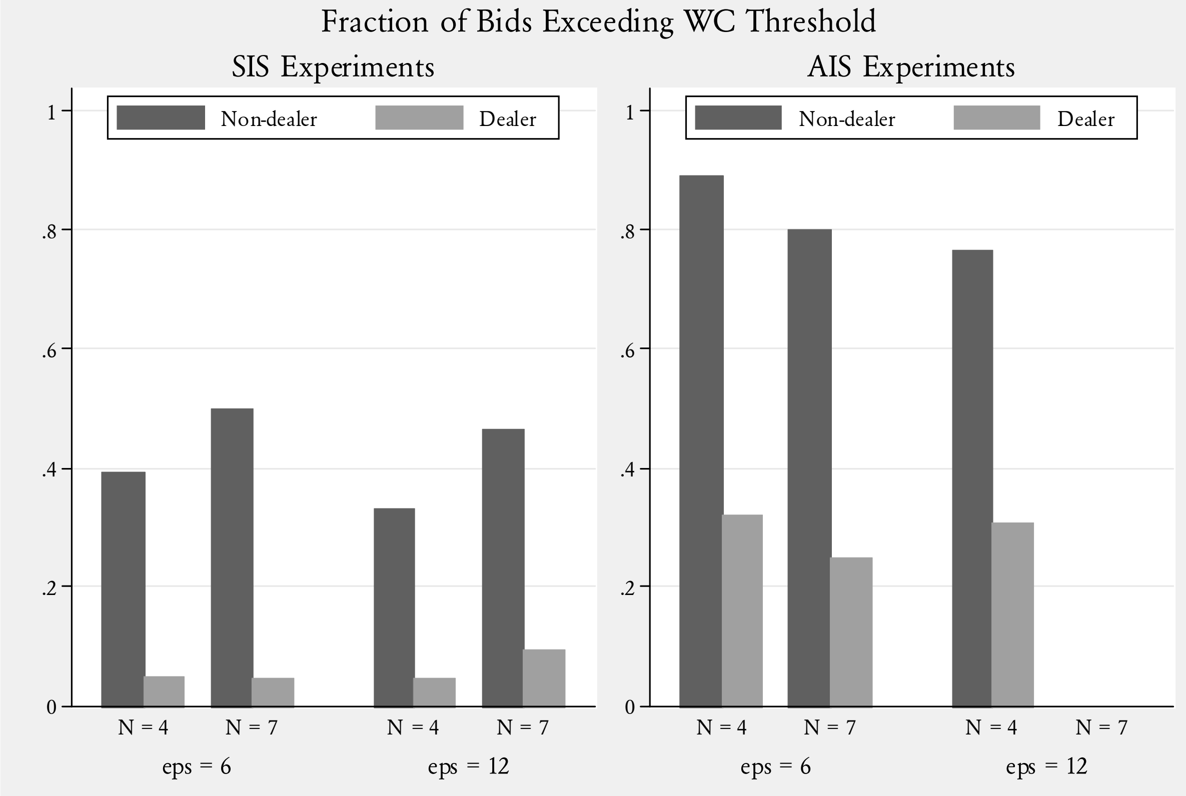


Table 2: Bid Deviation Regressions for Lab Experiments

Dependent variable is observed bid - signal

Variable	Description	Point Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
A. SIS Auctions (N=194)						
	Constant	12.32	8.74	0.16	-4.92	29.56
Dealer	Dealer	-6.89	9.34	0.46	-25.32	11.54
n	N	-0.01	0.25	0.98	-0.51	0.50
signal	Signal	-0.13	0.09	0.17	-0.31	0.06
eps	ϵ	-0.62	0.13	0.00	-0.88	-0.37
D_n	Dealer × N	-0.15	0.28	0.60	-0.70	0.40
D_signal	Dealer × Signal	0.09	0.10	0.39	-0.11	0.28
D_eps	Dealer × ϵ	-0.32	0.14	0.03	-0.60	-0.04
B. AIS Auctions – Outsiders (N=125)						
	Constant	9.24	8.93	0.30	-8.44	26.93
Dealer	Dealer	-6.14	10.07	0.54	-26.08	13.80
n	N	-0.21	0.26	0.41	-0.72	0.30
signal	Signal	-0.07	0.09	0.45	-0.25	0.11
eps	ϵ	-0.62	0.17	0.00	-0.96	-0.28
D_n	Dealer × N	0.13	0.27	0.62	-0.40	0.67
D_signal	Dealer × Signal	0.04	0.10	0.74	-0.17	0.24
D_eps	Dealer × ϵ	-0.24	0.20	0.23	-0.63	0.15
C. AIS Auctions – Insiders (N=36)						
	Constant	4.60	4.25	0.29	-4.08	13.28
Dealer	Dealer	-7.57	5.27	0.16	-18.34	3.20
n	N	-0.94	0.67	0.17	-2.31	0.43
eps	ϵ	-0.46	0.24	0.07	-0.95	0.04
D_n	Dealer × N	0.93	0.78	0.24	-0.65	2.52
D_eps	Dealer × ϵ	0.29	0.34	0.40	-0.40	0.98

Table 3: Seller Revenue Regressions for Lab Experiments

Variable	Description	Point Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
	A. Full Sample (N=73)					
	Constant	92.59	1.90	0.00	88.79	96.39
N	N	0.86	0.28	0.00	0.30	1.42
eps	Value of ϵ	-0.09	0.16	0.56	-0.40	0.22
Dealer	Dealer	-5.85	1.37	0.00	-8.58	-3.12
AIS	Asymmetric Information	2.11	0.94	0.03	0.23	3.99
	B. N=4 Sub-Sample (N=52)					
	Constant	97.41	1.66	0.00	94.07	100.76
eps	Value of ϵ	-0.26	0.20	0.19	-0.65	0.14
Dealer	Dealer	-6.34	1.69	0.00	-9.74	-2.94
AIS	Asymmetric Information	2.73	1.23	0.03	0.26	5.21
	C. N=7 Sub-Sample (N=21)					
	Constant	94.62	2.36	0.00	89.63	99.60
eps	Value of ϵ	0.34	0.26	0.20	-0.20	0.88
Dealer	Dealer	-4.34	1.34	0.00	-7.17	-1.51
AIS	Asymmetric Information	1.66	1.28	0.21	-1.05	4.37
	D. ϵ = 6 Sub-Sample (N=40)					
	Constant	92.89	1.48	0.00	89.89	95.89
N	N	0.39	0.26	0.14	-0.13	0.91
Dealer	Dealer	-2.89	0.79	0.00	-4.49	-1.30
AIS	Asymmetric Information	2.40	0.73	0.00	0.92	3.89
	D. ϵ = 12 Sub-Sample (N=33)					
	Constant	88.79	4.33	0.00	79.94	97.64
N	N	1.67	0.73	0.03	0.17	3.17
Dealer	Dealer	-8.48	2.30	0.00	-13.18	-3.78
AIS	Asymmetric Information	2.39	2.15	0.27	-2.00	6.78

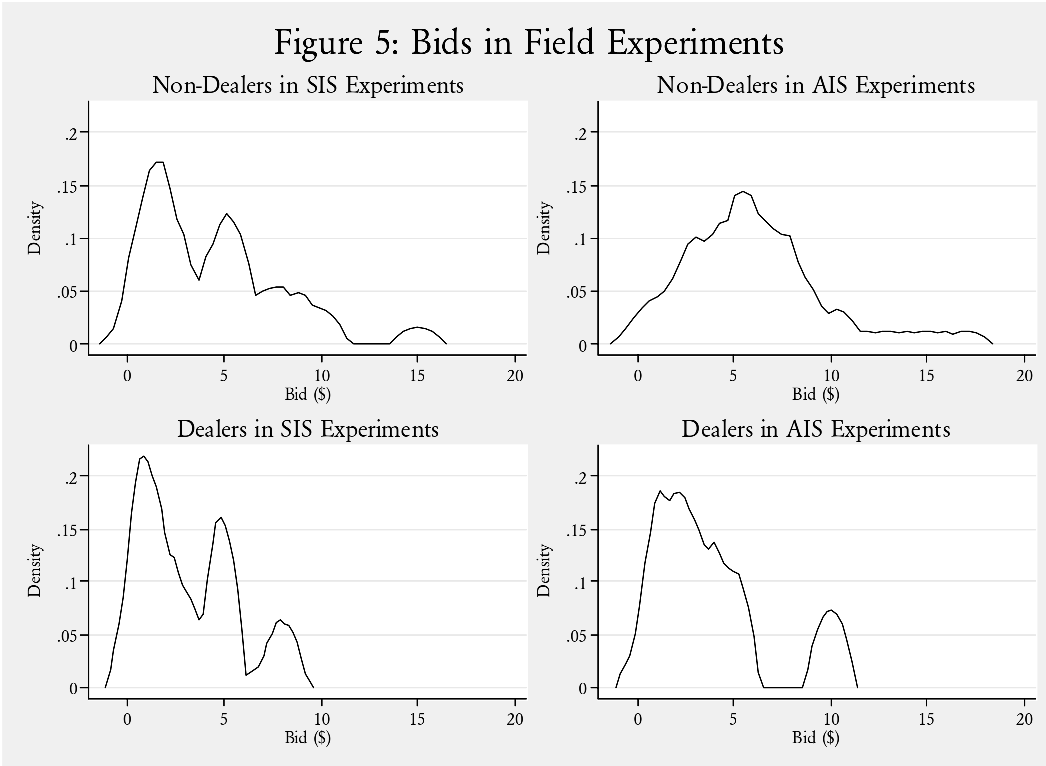
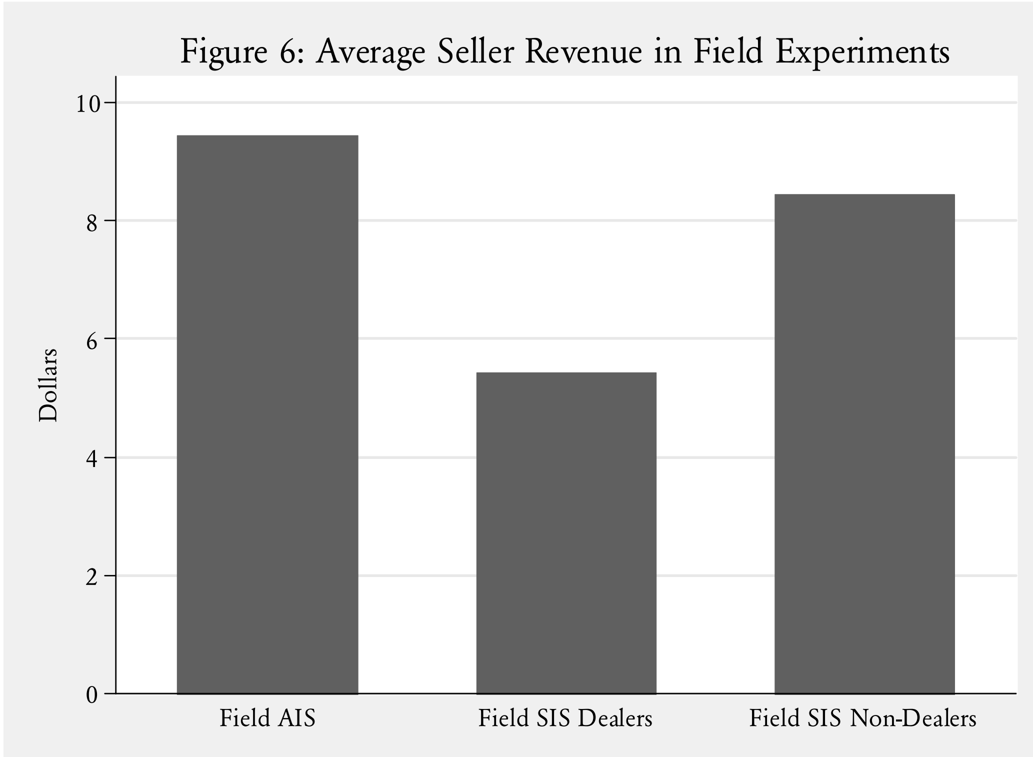


Table 4: Bid Regressions for Field Experiments

Variable	Description	Point Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
		A. SIS Auctions (N=64)				
	Constant	4.31	0.61	0.00	3.09	5.54
Dealer	Dealer	-1.21	0.76	0.11	-2.72	0.30
		B. AIS Auctions (N=60)				
	Constant	5.99	0.53	0.00	4.92	7.05
Dealer	Dealer	-2.31	0.92	0.02	-4.16	-0.46



Appendix A. Subject Instructions in the Laboratory Auctions

[These instructions are for the $6 symmetric lab auctions with 4 participants, unless noted by AIS below.]

This is an experiment in the economics of decision making. The instructions below should be carefully read and understood before we proceed to the actual auction. If you follow the instructions carefully, and make a good decision, you may earn a considerable amount of money-which I will personally pay you in cash at the end of the experiment.

In this experiment I will auction off a fictitious commodity in a first price auction.

Your task is simple: to submit a written bid for the commodity. But, the value of the commodity at the time you make your bid will be unknown to you. Instead of knowing the true value, each of [FOR AIS, DELETE each of] you will receive a private signal as to the value of the item, which you should find useful in determining your bid (more on this below).

The value of the auctioned commodity (denoted V*) will lie between $40 and $200 inclusively. In the auction, any value within this interval has an equally likely chance of being drawn. Hence, each person has the same value (V*) for the commodity.

Although you do not know V* precisely, you will receive information which will narrow down the range of possible V* values. The information will come via your private signal: the signal is selected randomly from an interval: V* - $6 and V* + 6. Any value within this interval has an equally likely chance of being drawn and being assigned to one of [FOR AIS, DELETE one of] you as your private information signal (note that each of you will receive your own individual signal).

An example will help illustrate the value of such a signal. Lets suppose that V* = $150 (the value of the commodity is $150). Then each of [FOR AIS, DELETE each of] you will receive a private signal that will be a randomly determined number between \$144 and \$156 (these two numbers come from (\$150 - \$6) and (\$150 + \$6)). Consider four such signals for V* = \$150: \$145.07; \$149.59; \$152.89; and \$154.96.

You can see that some of the signals in this example are below the true value of the auctioned item and some are above the true value. But, you will note that V* must always be greater than or equal to your signal value - \$6. Likewise, V* must always be less or equal to your signal value + \$6. In the actual auction, these upper and lower bound estimates for V* and your signal value will be provided on your bidding sheet.

As you can see, a bid above V + \$6 makes little sense because you are guaranteed to lose money if you are the high bidder; thus I will restrict bids to not exceed V + \$6 in the auction.

In this first price auction, you will be competing with 3 other bidders, who also receive a private signal. [FOR AIS, REPLACE THE LAST SENTENCE WITH: In this first price auction, you will be competing with 3 other bidders. Two of these bidders will receive a private signal selected randomly from an interval: V* - \$6 and V* + 6. The third will receive a private signal that is equal to V*. Thus, this third bidder knows exactly what the fictitious commodity is worth.] The rules of the first price auction are straightforward -- the high bidder wins the item and makes a profit (or loss) equal to the difference between the value of the commodity and the amount he/she bid:

V* - Highest bid = profit (or loss)

Of course, a loss results if the winner bids more for the item than it is actually worth. If you do not make the high bid in the auction, you will earn zero profits.

You are not to reveal your bid or private signal to any other subject during this experiment. Also, please do not speak to anyone except the monitors. After all bids are received, we will post V* and compute everyone's profits and/or losses.

Lets now proceed through a few practice auctions to assure that everyone understands the auction and payoff rules before we proceed to the actual auction.

ARE THERE ANY QUESTIONS?

Appendix B: Subject Instructions in the Field Auctions

[These instructions are for the symmetric field auctions with 4 participants, unless noted.]

Welcome to Lister's Auctions! You have been invited to participate in an auction for the unopened pack of 1990 *Leaf* cards displayed on the table. I guarantee to sell the unopened pack to the highest bidder, no matter how low the price. You will be bidding against 3 other bidders in this auction, but you will not know their identities (nor will they know yours); the other bidders will be randomly chosen from other participants at today's card show. If you are a card dealer you will be paired randomly with three other card dealers. If you are not a dealer, you will be paired with three other non-dealers. [IN THE ASYMMETRIC AUCTIONS, REPLACE THE PREVIOUS TWO SENTENCES WITH: If you are a card dealer you will be paired randomly with three other non-dealers. If you are not a dealer, you will be paired with two other non-dealers and one card dealer.]

This is a first-price sealed-bid auction. In a first-price auction, the person with the highest bid wins the good and pays the amount of his or her bid. For example, I will order the four bids from highest to lowest in order to determine the winner.

Example 1: if the bids are ranked highest to lowest as follows:

\$A (from bidder 3)
\$B (from bidder 4)

$C (from bidder 1)
$D (from bidder 2)

Bidder 3 wins the unopened pack and pays $A. Would you like to proceed through another example? {If "yes", then show another example}

There is no secret "reserve price." The pack will be sold to the highest bidder, no matter how low the price.

Your bid represents a binding commitment to pay for the pack according to the rules of this auction. I will determine the winner at 5PM today. After the winner pays me (cash or check) for the pack, the pack will be awarded to the winner. If you win the pack in this auction and are not in attendance at 5PM, it will be sent to you via first-class mail upon the receipt of your payment. There is no charge for shipping.

Please provide your name, mailing address, and phone number below:

Name______________________

Address____

Phone#_____________________

Email_________________________

Signature____

Good luck - please write your bid on the sheet provided. Thanks for participating.