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Auction Markets for Evaluations

Cary A. Deck* and Bart J. Wilson†

When the value of a product or service is uncertain, outcomes can be inefficient. A market for evaluations can theoretically increase efficiency by voluntarily eliciting an evaluation that would otherwise not be provided. This paper uses a controlled laboratory experiment to test the performance of four market mechanisms to provide product evaluations. The mechanisms considered are derived from the oft studied uniform price sealed bid, discriminatory price sealed bid, English clock auction, and Dutch clock auction. Our results indicate for this nonrivalrous product that (i) each of these institutions improves social welfare and (ii) the performances of the four mechanisms are equivalent. This second point is particularly noteworthy given that differing behavior is routinely observed in traditional private value auctions.

JEL Classification: C92, D70, D83, H41

1. Introduction

As the technology of electronic exchange advances, new opportunities emerge for developing markets for products and services whose innate properties hamper efforts to do so in traditional settings. Even though one such good, product evaluations, has been long used for durable and nondurable goods alike, the transaction costs associated with providing and disseminating product evaluations have limited the scope of their use. Historically, evaluations have been limited to "word of mouth" exchanges among acquaintances and the reports of paid critics.[1] The Internet not only has the potential to significantly reduce the transaction costs for evaluation sharing, but it can also significantly reduce the costs for forming a centralized market mechanism to allocate evaluations. Although numerous evaluations are currently freely available on the Internet for everything from CDs and books to articles and professors, these services are incomplete.[2] For example, *Amazon.com* provides free book reviews by other customers who voluntarily provided the review, but there are numerous books that have not been reviewed. *Ratemyprofessors.com* enables students to share evaluations of their college instructors, but not every teacher has been rated. A market for evaluations creates the incentives for individuals to provide evaluations that might otherwise not be provided.

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[1] Additionally, businesses have long attempted to use techniques such as brand image and advertising as an indication of quality. However, when new products or services are introduced within a brand line, buyers can still face uncertainty about their value for the product or service.

[2] Both amateur and professional reviews can suffer from a lack of technical knowledge on the part of the reviewer and can reduce social welfare if, given the heterogeneity of values, the actual reviewers are not the socially optimal ones.

Avery, Resnick, and Zeckhauser (1999) provide a first step in creating a pricing mechanism to induce the efficient provision of evaluations. They discuss how, without a market for evaluations, risk neutral agents will provide a suboptimal level of evaluations because consumption is nonrivalrous[3] and because one person's experience might not perfectly predict another's outcome. The nonrivalrous nature of evaluations could also result in an inefficient ordering of the evaluators themselves, if the potential evaluators have heterogeneous opportunity costs for producing an evaluation. The optimal quantity can also depend on what the early evaluations reveal about the value of the product.

Avery, Resnick, and Zeckhauser (1999) show that a market mechanism can be used to solve these various social problems. They consider both sequential and batch evaluation games and demonstrate that a centralized broker who knows the pool of values and opportunity costs for the players can set a price for providing and receiving information so as to eliminate the social inefficiency. This paper relaxes the assumption that a central broker holds all of the pertinent information, which, as a point of practicality, would not hold in an actual implementation of such a mechanism. Using the experimental method, we study how efficiently four different market mechanisms voluntarily elicit an evaluation in exchange for payment, without *any* centralized information about the pool of values. This subjects the market mechanisms to the challenging task of inducing the optimal person to voluntarily provide a nonrivalrous evaluation. As an initial foray into implementing a market for evaluations, we consider variations of and then compare the efficiency and prices for uniform price sealed bid, discriminatory price sealed bid, English clock auction, and Dutch clock auction.

We follow Avery, Resnick, and Zeckhauser (1999) in considering a framework in which people have already made the decision to enter a product market. As a simplifying assumption for this exploratory research, the individuals in the markets have identical tastes for whether a product is "good" or "bad" and the same ability to discern those tastes. Individuals, however, differ in their values for a "good" or "bad" product. Thus, unlike Avery, Resnick, and Zeckhauser (1999), a single evaluation perfectly reveals whether the product is "good" or "bad." This assumption eliminates the inefficient ordering and optimal quantity problems and makes the underprovision problem binary, thereby allowing us to focus on the crucial issue of how the mechanisms aggregate private information, determine prices, and affect efficiency.

In our controlled test, we find that the provision of evaluations is markedly inefficient without a market mechanism, but not nearly as inefficient as what is predicted. Additionally, we find that each of the four mechanisms succeeds at increasing market efficiency by encouraging the socially optimal agent to undertake the costly evaluation when no one else is willing to do so. Finally, we observe that the four mechanisms are behaviorally equivalent with respect to the prices received by the evaluator. The structure of the paper is as follows. Section 2 presents the experimental design that we consider, and section 3 outlines the treatments and procedures. Section 4 discusses our results, and section 5 briefly concludes.

2. Experimental Design

Suppose there are two identical risk-neutral individuals considering reading a book. A particular book could be "good," resulting in a payoff of $g > 0$, or "bad," resulting in a payoff of $b < 0$ with

[3] As Avery, Resnick, and Zeckhauser (1999) point out in their footnote 3, positive evaluations for some products (e.g., stocks or restaurants) can increase demand and hence could be rivalrous. In such situations, agents could have an incentive not to truthfully reveal.

$|b| > |g|$. If each outcome is equally likely, then both individuals have a negative expected value for reading the book and hence should opt for their next best alternative, provided that alternative payoff c is greater than $(g + b)/2$. However, it is not necessarily socially optimal for neither person to read the book. Consider the case in which one individual decides to read the book and then provides an evaluation to the other person. In this case, the evaluator expects to receive $(g + b)/2$, but the person receiving the evaluation can make a more informed decision. This informed person will read the book if it is "good" and receive a payoff of g. However, if the evaluator reports that the book is "bad," the other person will not read it, choosing the opportunity cost c over the bad payoff b. Hence the person receiving the evaluation has an expected payoff of $(g + c)/2$, and the social payoff from one person evaluating the book is $(g + b)/2 + (g + c)/2$, which is greater than $2c$ so long as $c < (2g + b)/3$.

The solution to this social problem lies with the creation of a market that allows one potential consumer to compensate another for undertaking the costly evaluation. Avery, Resnick, and Zeckhauser (1999) present a formal treatment of this problem and demonstrate the existence of an equilibrium price that attains the socially efficient outcome.[4] This price is paid to the evaluator by the person waiting to make an informed decision. In the example presented above, the unique equilibrium price, P, is $(c - b)/4$. This price is found by equating the expected payoff of the evaluator to the expected payoff of the person who waits, or P insures that $(g + b)/2 + P = (g + c)/2 - P$, thereby making the two identical people indifferent between evaluating and waiting.

Institutions

Equipped with the theoretical foundation that a market can solve this problem, the next step is to identify what market institutions should be implemented in practice. The identification of the market price in the preceding paragraph requires collective knowledge of three pieces of information for each individual i, namely g_i, b_i, and c_i, plus the probabilities that the product or service is "good" or "bad." In practice, these pieces of information are typically private and unobservable (or unverifiable). Thus, one role of a functioning market is to aggregate private information and coordinate behavior. Therefore, any market institution must determine (i) who will be the evaluator and (ii) the price paid or received by each agent. As discussed in Smith (1994) the institution can significantly influence behavior and therefore market outcomes. For example, the four most common types of private value auctions, uniform price sealed bid, discriminatory price sealed bid, English clock, and Dutch clock, are all theoretically revenue equivalent under certain assumptions; yet, there is widespread evidence from the laboratory that these distinct formats elicit different behaviors, which affect market performance.[5] This paper takes the next step toward constructing markets for evaluations by developing variants of these four well-known market institutions and comparing the performance of each. An important distinction between our environment and that of the standard private value auction

[4] Avery, Resnick, and Zeckhauser (1999) show that markets can solve much more complex problems as well, such as the case in which someone else's positive experience only serves as a signal about the probability that one's own experience will be positive. However, as a first step in understanding behavior in markets for evaluations, this study focuses exclusively on the case in which everyone's opinion of the outcome, but not necessarily their payoff from it, is the same.

[5] For a more comprehensive discussion, the reader is directed to Kagel and Roth (1995). Two early studies test the strategic equivalence of first-price and Dutch auctions. Coppinger, Smith, and Titus (1980) and Cox, Roberson, and Smith (1982) both find that (i) prices are higher in first-price auctions than in Dutch auctions and (ii) bidding is consistent with risk-averse behavior. The predicted isomorphism between English and second-price auctions also fails to be observed. Coppinger, Smith, and Titus (1980) and Kagel, Harstad, and Levin (1987) find that bidding in the English outcry auction conforms to the theoretical predictions quite well, whereas in the one-shot second-price auction, bidders consistently bid higher than the dominant strategy prediction, even with experience in the auction mechanism (Kagel and Levin 1993).

is that the evaluation is nonrivalrous.[6] Within the controlled confines of the laboratory, these wind tunnel tests directly compare these institutions to each other, as well as to a baseline case in which no market exists.

No-market Baseline

In the baseline case, a fictitious product is available for evaluation for a limited time, T. If at any point during this time one of the n individuals consumes and evaluates the product, then the payoff state, good or bad, is revealed to everyone. As a simplification, if the product is good, then everyone who waits receives their own good payoff g_i, but if the product is bad, all of the people who wait receive their opportunity cost c_i. The evaluator also receives g_i if the product is good, but when it is bad, the evaluator receives b_i. In this situation, the dominant strategy is to wait and see whether the others evaluate, regardless of how much time is remaining, assuming that the opportunity cost is sufficiently high. Formally, let λ_i denote a player i's belief about the probability someone else will evaluate during the remaining time. The expected payoff to waiting is $\lambda_i(g_i + c_i)/2 + (1 - \lambda_i)c_i$, which is greater than $(g_i + b_i)/2$, the expected value of evaluating, if $\lambda_i > 1 + (b_i - c_i)/(g_i - c_i)$. This condition is equivalent to $\lambda_i \geq 0$ if $c_i > (g_i + b_i)/2$. We now describe four distinct institutions for providing an evaluation.

Uniform Price Sealed Bid Auction

For the uniform price sealed bid auction in an independent private value environment, each bidder privately submits a bid. The bids are arrayed from highest to lowest and the winners are the bidders who submitted the highest bid. Each winner pays the same price, which is determined by the lowest winning bid and/or the highest loosing bid. In a market for a nonrivalrous evaluation, each of the n agents submit a single bid β_i. This bid represents the minimum amount the person is willing to accept for evaluating the fictitious product. Once the n bidders have submitted their bids, the bids are ranked in ascending order. Let $\underline{\beta}$ and $\underline{\underline{\beta}}$ denote, respectively, the lowest and second lowest submitted bids. The agent submitting bid $\underline{\beta}$ is chosen as the evaluator, and the price he receives for evaluating is $(\underline{\beta} + \underline{\underline{\beta}})/2$. The other $n - 1$ agents wait for the evaluation, and each pays an equal portion of the price. The price paid by each individual for the information is $(\underline{\beta} + \underline{\underline{\beta}})/2(n - 1)$. Hence when entering a bid, agent i knows that $\beta_i/(n - 1)$ is the maximum amount she will have to pay for the evaluation. In the event that multiple agents submit the lowest bid, the evaluator is chosen randomly from that subset of bidders.

Discriminatory Sealed Bid Auction

Like the uniform price sealed bid auction, in this auction, each bidder privately submits a bid, and the winners are the bidders who submit the highest bids. However, in the independent private values environment, each winner pays a price equal to his own bid. In an evaluations market, a bid β_i in this institution represents a minimum price that agent i will be paid to evaluate, and $\beta_i/(n - 1)$ represents a maximum amount that agent i might have to pay for the information, as in the uniform price auction. Again, the bids are ranked from lowest to highest and the person submitting the lowest bid is chosen to evaluate the product. However, the price paid by each agent who waits depends on his bid and the evaluator's bid. Let j denote the person who submitted the lowest bid $\underline{\beta}$. Agent $i \neq j$ pays

[6] Because one bidder will necessarily be selected as the evaluator and receive payment from the other participants, these mechanisms can be classified as fair division games (see Güth and van Damme 1986; Güth et al. 2002).

$(\underline{\beta} + \beta_i)/2(n-1)$ to the evaluator. The total amount paid to agent j, the evaluator, is $(\underline{\beta}/2) + \sum_{i \neq j} [\beta_i/2(n-1)]$. Again, ties are broken randomly.

Descending (English) Clock Auction

An English clock auction with independent private values begins with a low price that the bidders are willing to pay. As long as there are more bidders who are willing to pay the current price than there are units for sale, the price increases. Once there is no excess demand at the current price, the auction ends and all remaining bidders buy the final price. An English clock auction for evaluations is also operationalized by setting an initial price, such that multiple agents are willing to accept the proposed price, and then moving the price in the less favorable direction until all but one agent drops out of the auction. Because an evaluation market is attempting to procure an evaluation, the process works in the reverse of that in an independent private value environment. The initial price on the clock is set sufficiently high such that multiple agents are willing to evaluate the fictitious product and then the price falls until all but one person has indicated a preference to wait for the information (i.e., withdraw from the market and not evaluate) at the current price on the clock. Once a bidder signals to wait, a bidder cannot re-enter the market for that period. The number of active bidders is not publicly stated as the price decrements and bidders withdraw. The clock price, like the bid amount in the sealed bid institutions, refers to the amount that the evaluator receives. Those who wait pay $1/(n-1)$ of the final clock price. Again, ties are settled randomly. Unlike the sealed bid institutions, this mechanism requires up-front parameterization in the form of a starting price, the amount by which the clock decrements, and the rate of time for the decrement. Additionally, a stopping rule is imposed in the event that the clock price reaches zero with at least two agents still in the market. Because a clock price of zero indicates that at least two agents are willing to undertake the evaluation for no payment, one of these agents is randomly selected to provide the information for free to the remainder of the group. It is worth noting that although truthful revelation is a dominant strategy in the independent private value environment, this is not the case in an evaluations market because the price at which the penultimate bidder exits the market affects the price that bidder will pay for the information.

Ascending (Dutch) Clock Auction

As with an English clock auction, a Dutch clock auction also involves systematically changing the price until a winner is declared. With this auction, the price is initially set such that nobody is willing to accept the transaction. The price is then gradually improved until some agent accepts the terms. With independent private values, the price begins high and then is lowered until a bidder accepts the current price. However, in the market for evaluations the price is set sufficiently low such that everyone wants to wait initially, then the price is increased until the first agent agrees to undertake the evaluation for the price shown on the clock. Again, the clock price refers to the price received by the evaluator. Each of the $n-1$ agents who did not indicate a willingness to evaluate pays $1/(n-1)$ of the amount received by the evaluator. This institution also requires additional parameters for the starting price, minimum amount of the price increment, and rate at which the price is incremented.

Parameters

We chose to compare the institutions with $n = 4$ participants in each market. The experimental literature is ripe with examples of markets in which four sellers or four bidders can be considerably competitive, depending on other details of the environment (see, e.g., Cox, Roberson, and Smith

Table 1. Market Values

Agent i	Agent Type	Good Value (g)	Bad Value (b)	Opportunity Cost (c)	Expected Value if Agent Evaluates	Expected Value if Another Agent Evaluates
1	1	320	−340	24	−10	172
2	2	220	−240	24	−10	122
3, 4	3	100	−120	24	−10	62

1982; Isaac and Walker 1985; Isaac and Reynolds 2002; Thomas and Wilson 2002; and Deck and Wilson 2003). Table 1 reports the good values, bad values, and opportunity costs for each of four participants in a market. We continue to assume that the product is good or bad with a 50% probability.

Within a group of four agents is one type 1, one type 2, and two type 3 agents.[7] Each agent has an expected value of −10 for evaluating, which is less than the common opportunity cost of 24. However, agents differ in their expected values from another agent evaluating. Table 2 lists the expected social surplus depending on which agent, if any, evaluates.

The socially efficient payoff is for one of the two type 3 agents to undertake the evaluation, and because there are two type 3 agents, a unique price exists that supports this outcome, assuming all agents are risk neutral.[8] Because the type 3 agents are identical, the equilibrium price is such that the two type 3 agents are indifferent between evaluating and waiting. That is, the price structure satisfies $(g_3 + b_3)/2 + \textit{price received for evaluating} = (g_3 + c_3)/2 - \textit{price paid for information}$. When the price paid for information is $1/(n-1)$ times the price received by the evaluator, the equilibrium price that supports the efficient outcome is 54 for the parameters in Table 1. This price prediction is applicable to the uniform price sealed bid, descending English clock auction, and ascending Dutch clock auction because each agent that waits pays the same price. However, this price prediction does not hold for the discriminatory sealed bid auction because the choices of the type 1 and type 2 agents will affect the price the evaluator receives. Furthermore, an explicit price prediction would depend on the beliefs bidders have about the likely bids and the values of others.[9] Nevertheless, as an exploratory exercise we include it in our comparison of the other three institutions.

We chose the payoffs for the type 1 and 2 agents so that they have the same expected value for evaluating as the type 3 agents, but if a type 1 or 2 agent evaluates, the expected social loss is nontrivial and dependent on which of the two evaluates. The opportunity cost of 24 satisfies the desirable property that $b_i + g_i < 2c$ for each agent type. It also creates an essential separation between the social payoff in the case in which no one evaluates and the efficient case, while maintaining the property that a person who receives one good payoff, one bad payoff, and one opportunity cost payoff

[7] The type 3 agents are similar to those used by Avery and Zeckhauser (1997) in a discussion of a market for evaluations in a similar setting.

[8] Identical expected values for evaluating generate a nontrivial environment to test how well a market institution induces a socially optimal evaluation by a type 3 agent.

[9] As a simplifying assumption, theoretical and experimental work on private value auctions have assumed that values are distributed uniformly and that this is common knowledge among the market participants. In contrast, we chose a challenging environment that parallels the naturally occurring economy, in which participants only have private information on their own values, *à la* a typical double auction market experiment. It is well known that the double auction institution quickly achieves competitive outcomes (at or near 100% efficiency), all with strictly private information on all values and costs. In this paper, we also want to investigate a rather difficult test of a market mechanism when the participants only have strictly private information, as is reasonably the case in the diffuse and impersonal world on the Internet. The reader is referred to Güth and van Damme (1986) and Güth et al. (2002) for the derivation of the optimal bid strategies in fair value games for agents when there is common knowledge on the distribution of values.

Table 2. Expected Social Surplus by Type of Evaluator

Type of Agent Evaluating	Expected Social Payoff	Efficiency (= Social Payoff − No Evaluation Payoff)/ (Maximum Social Payoff − No Evaluation Payoff)
None	96	0%
Type 1	236	56%
Type 2	286	76%
Type 3	346	100%

will experience nonnegative earnings.[10] Also, an opportunity cost of 24 leads to an integer price prediction that is not a natural focal point.

As discussed above, some of the institutional treatments also require parameterization. In the no-market baseline, the time available for product evaluations is $T = 30$ seconds. Both clock institutions require an increment and an initial price. The clock increment/decrement is 1 and updated every second. The initial prices are set such that, *a priori*, the mechanisms would last as long as the baseline no-market treatment. In the no-market baseline, no one should evaluate and a period lasts 30 seconds. Therefore, the starting price in the English clock auction is $54 + 30 = 84$, and the starting price in the Dutch auction is $54 - 30 = 24$ (which is conveniently equal to the opportunity cost). For the two sealed bid institutions, subjects also have 30 seconds to enter their bids. If a subject does not enter a bid in the allotted time, then his previous bid serves as the default.[11] For the payoffs to be comparable across treatments, we desired that each laboratory session consist of the same number of periods and thus each should last approximately the same amount of time.

3. Experimental Procedures

A market consisted of four subjects who were constantly and anonymously matched throughout the 48 decision periods of the experiment. Subjects retained the same agent type each period and never knew the payoff parameters associated with the other subjects or even the distribution of those parameters. Before the experiment began, each subject was given a set of written instructions. After all subjects completed the instructions and had the opportunity to ask questions, the computerized experiment began.

For the first 24 periods, all subjects, regardless of institution treatment, participated in the no-market baseline. This insures that before introducing the market mechanism the subjects have substantial experience with the payoff implications for evaluating and not evaluating with good and bad values. When making decisions in the baseline environment, subjects knew only their own payoff parameters and the time remaining in the period.

After each period, subjects received feedback about their own payoff and whether the product was good or bad that period if and only if someone evaluated. Subjects were not told who evaluated or anyone else's payoff. At any point during the experiment, regardless of treatment, a subject could scroll through a table that displayed for all previous periods their own action, whether or not someone

[10] This positive gain property helps prevent a loss of control over a subject's motivation because negative earnings cannot be enforced. This is particularly important in the early stages of the experiment when subjects are relatively inexperienced.

[11] Participants in an unpaid pilot experiment indicated that more than 30 seconds was too long and rarely was the 30-second time limit binding. Also, the starting price of 84 in the English clock auction should discourage type 1 and type 2 agents from evaluating because risk-neutral agents of both types would prefer to exit the auction immediately. The results of the experiments are interpreted accordingly.

else had evaluated; the payoff state if revealed; and their own profit. After the first 24 periods were completed, subjects in the baseline treatment continued in this environment for an additional 24 periods. Subjects in the other four treatments were given additional written instructions about a single market mechanism that would be in place for the remaining 24 periods. After all subjects read the instructions and had the opportunity to ask questions, the computerized experiment resumed. In addition to the information revealed in the baseline case, subjects in the four market treatments were told the price they paid for waiting or the price received for evaluating. This design constitutes a nontrivial challenge to the auction mechanisms. First, every subject is identical in expected value terms (−10) for not evaluating, and second, subjects have no information at all on the values of the other participants in the market. Subjects were not told the number of periods in the experiment or a portion thereof, nor were they informed in advance that a mechanism would be imposed in the latter part of the session.

A total of 25 sessions were conducted, five for each of the five treatments. We held constant across all sessions a random sequence of 48 good and bad value states; that is, one sequence was randomly determined in advance and then employed in all sessions. This serves to reduce the variation across sessions.

The 100 participants in this study were undergraduates from the general student population at George Mason University, where the experiments were conducted. For participating in the one-hour experiment, each subject was paid $7 for showing up on time, plus his or her salient earnings. All payoffs, parameters, and prices were denoted in terms of experimental dollars (EXP). At the conclusion of the experiment, a subject's cumulative profit was converted into U.S. dollars at the rate of EXP 200 = US$1, which was stated to the subjects before the beginning of the experiment.[12] The average earnings in the experiment were approximately $15.25, excluding the $7 show-up fee.

4. Experimental Results

The data consist of observed behavior in 720 periods under the no-market baseline and 120 periods under each of the four market mechanisms. We found that each of the market mechanisms was successful at increasing efficiency relative to the baseline by increasing the frequency with which the optimal agent type evaluated. This result is presented as a series of findings, each with supporting analysis that treats each session as an independent observation. To control for learning, the analysis focuses exclusively on data from the latter half of the periods in a particular institution (periods 13–24 and 37–48).

The first 24 periods in each session consist solely of the baseline situation. If observed differences in the latter part of the experiment are attributable to the institution and not subject heterogeneity, behavior should be similar across the groups before the auction mechanisms are introduced. The first finding is largely a calibration result demonstrating that subject behavior is indeed similar across all treatments before the implementation of a market mechanism. To compare the choices of individuals, and hence performance of the five institutions, we use the metric of average *ex ante* efficiency from the appropriate periods within a session. This is a measure of the expected social welfare conditioned on the frequency with which, if any, agents of a particular type evaluated. Thus, two sessions in which the same numbers of each type undertook the evaluation would be considered as performing identically, even though realized surplus might vary across the sessions

[12] The use of an exchange rate allows the clock prices to adjust over finer increments. Previous research has shown that clock speed and increment can be significant factors that influence market prices in standard clock auctions, which is also why we calibrated the starting prices for the clock auctions at 54 ± 30 experimental dollars.

depending on who evaluated when the product was good or bad. Because each session is independent of the others, this metric allows for a comparison of independent observational units.

FINDING 1. *Ex ante* efficiency is statistically indistinguishable across all five treatments in the first half of the experiment before an auction mechanism is introduced.

SUPPORT. For the null hypothesis that *ex ante* efficiency in the initial no-market phase of each session did not differ by treatment, we employ a Kruskal-Wallis test on the 25 average *ex ante* efficiency observations (one for each session) for periods 13–24. The test statistic, corrected for ties in the ranking, is 0.467, which cannot be rejected in favor of the two-sided null at any standard level of significance. ■

The frequency with which subjects choose to evaluate is nontrivial in periods 13–24. Figure 1 illustrates when the subjects choose to evaluate during the 30-second period without a market mechanism in place. The hatched bar indicates how many times no one evaluated, and the solid bars indicate how many evaluations occurred for the six, five-second blocks of time. Notice that when a subject evaluates, it is most often within the first 5 or last 5 seconds of the period. This suggests that our choice of a 30-second period is not too short because subjects choose either to evaluate quickly or to wait until the end of the period. Having established that the sessions do not differ before the implementation of a market mechanism, any differences across treatments can be attributed directly to the institutions. Therefore, our focus now turns to the effect of the market mechanisms as observed over the last 12 periods of each session.

FINDING 2. The introduction of a market mechanism significantly increases efficiency.

SUPPORT. Figure 2 displays the average *ex ante* efficiency over the last 12 periods by session. We use the Wilcoxon rank sum test (W statistic) to determine whether each mechanism improves efficiency relative to the no market baseline. In each pairwise comparison, the W statistic was 40, the largest value possible. Thus, the null hypothesis of no change in efficiency can be rejected at the 99% confidence level in favor of the alternative that the mechanism improved efficiency for each of the four market mechanisms considered. ■

The above analysis clearly shows that implementing a market for evaluations increases the expected efficiency. The next finding explores the differences between the four market mechanisms in terms of efficiency. A separate finding then addresses the source of the institutions' success.

FINDING 3. The four market mechanisms are statistically indistinguishable with respect to efficiency.

SUPPORT. Given no *a priori* ordering of *ex ante* efficiency by institution treatment, we employ a Kruskal-Wallis test to test the null of no difference by mechanism against the alternative that efficiency differs for some institution. Adjusting for ties in the efficiency rankings of individual sessions, the test statistic is 0.352, which means that the null hypothesis cannot be rejected at standard levels of significance. ■

Market mechanisms necessarily assign one person to evaluate the fictitious product in periods 25–48. For example, in either sealed bid auction someone must submit the lowest bid. Given the payment schemes, participation is individually rational for each agent. This is a distinct advantage of a market because it avoids the worst-case scenario in terms of *ex ante* efficiency, namely no one evaluating, which can and does happen without a market. It is reasonable to ask whether the greater efficiency is due simply to this aspect of the design. To answer this question, we determine what efficiency would

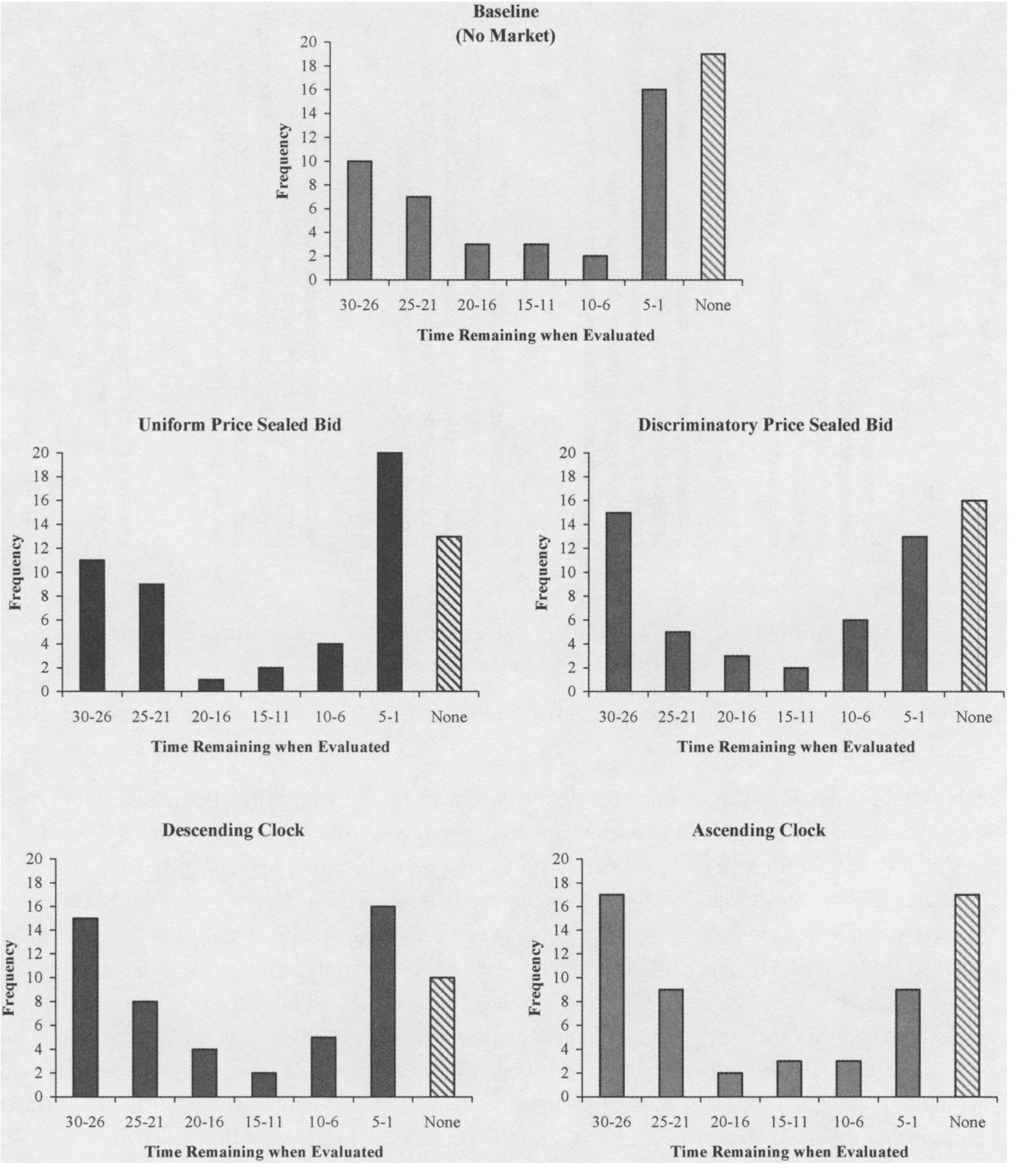

Figure 1. Histogram of Time Remaining When Good Was Evaluated for Periods 13–24 ("None" Indicates That No One Evaluated)

have been in the no-market baseline if one person had been randomly selected as the evaluator when no one volunteered. With this more conservative accounting for the no-evaluation outcomes, efficiency with the four market mechanisms is still statistically greater than efficiency in the no-market baseline, although the increase is only 5.3 percentage points. Formally, let M_j denote the frequency over periods 37–48 at which no one evaluated in session j in the no-market baseline groups, and let m_{ij} denote the frequency at which an agent of type i was observed to evaluate voluntarily over the same period in session j. For this conservative test, we recalculate the *ex ante* efficiency for the no-market baseline treatment by allocating one agent, in proportion to the types listed in Table 1, to be the evaluator for

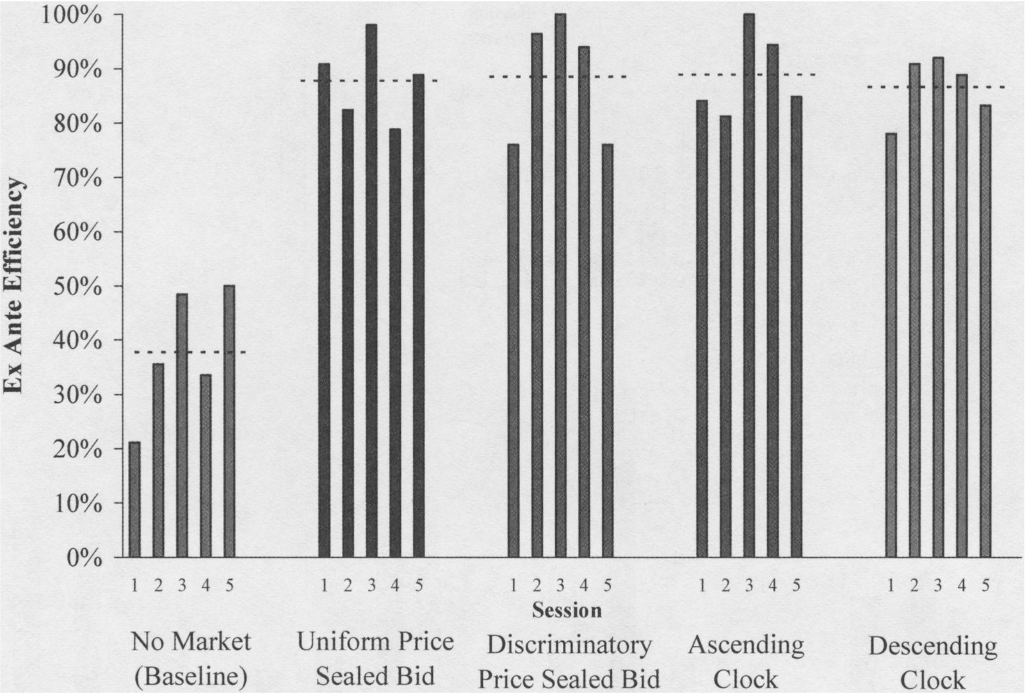

Figure 2. *Ex Ante* Efficiency by Session and Treatment for Periods 37–48

each of the periods in which no one voluntarily evaluated. More specifically, the frequency of type i agents evaluating in a no-market baseline session is imputed to be $m_{ij} + \theta_i M_j$, where $\theta_i = 0.5$ if $i = 3$ and $\theta_i = 0.25$ for $i = 1$ and 2. Let E_i denote the *ex ante* efficiency when a type i agent evaluates (see Table 2). The recalculated *ex ante* efficiency for baseline session j is thus $\sum_i (m_{ij} + \theta_i M_j)E_i$.

We use a Wilcoxon rank sum test to compare the recalculated efficiency of the baseline to the observed aggregated efficiency of the market treatments. The null hypothesis of no effect from a market can be rejected at the 95% confidence level in favor of the alternative hypothesis that the market mechanisms increase *ex ante* efficiency ($W = 38$, $p = 0.0331$). This demonstrates that an auction market increases efficiency by more than would be expected from merely randomly and involuntarily assigning one person to evaluate.[13] It should be emphasized that in the market mechanism, the subjects are volunteering to evaluate because they each have the choice in their bid to indicate their willingness to evaluate or wait.

Although someone will be chosen to evaluate with the market institutions, these auctions do not always induce the optimal agent to evaluate in each period as show in Figure 2. Also, it is not the case that type 1 and 2 agents are never willing to undertake the evaluation for free. In fact, a suboptimal agent evaluated 30% of the time on average for the last 12 periods in the no-market baseline treatment. Thus it remains to be explained the extent to which the markets succeed. Our conjecture is that the performance increase is due to the mechanisms encouraging type 3 agents to evaluate when no one else will. Finding 4 discusses this explanation formally.

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[13] An alternative metric for evaluating market performance is to select an evaluator for the no-evaluation periods in the same proportion as when there was a volunteer, or, $\theta_i = m_{ij}$. A Wilcoxon rank sum test with this metric leads to the same conclusion ($W = 37$, $p = 0.0284$).

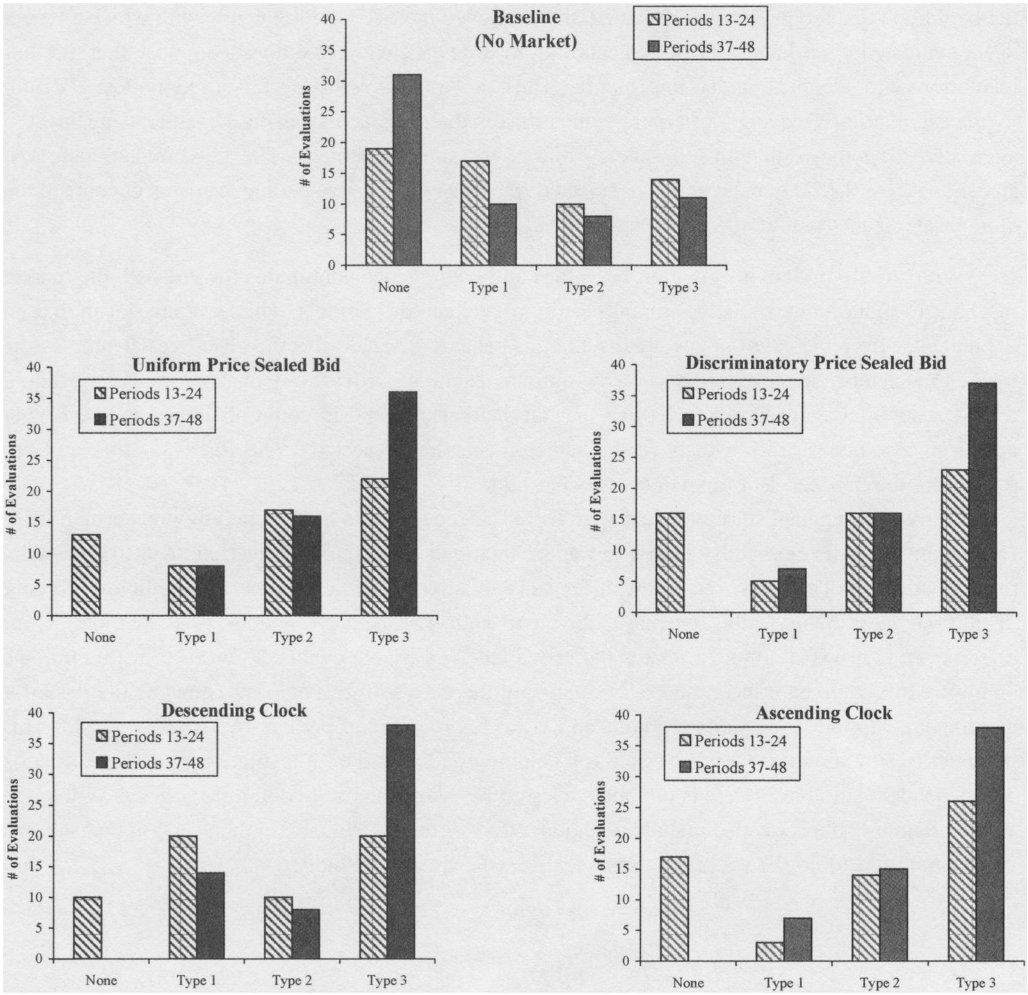

Figure 3. Frequency of Evaluations by Agent Type

FINDING 4. The auction mechanism induces the optimal agent to undertake the evaluation when no one else is willing to do so, thereby increasing efficiency.

SUPPORT. Figure 3 illustrates the qualitative support for this finding. The hatched (solid) bars for each treatment indicate the frequency with which each agent type evaluated in periods 13–24 (37–48). The "None" category specifies how many times no agent voluntarily evaluated and contains the same data displayed in Figure 1 with the hatched bars. In the no-market baseline treatment, all types of agents reduce their evaluations, most noticeably the type 1's (the least efficient evaluators). In marked contrast, the number of evaluations by type 3 agents increases substantially for all of the market institutions and generally by the number of times no one is willing to evaluate in the no-market periods 13–24. This finding is supported quantitatively by a comparison of observed market behavior with behavior in the no-market baseline in which type 3 agents are assumed to have evaluated if no one else volunteered. For brevity, this requires an imputation similar to that discussed following Finding 3. Specifically, the frequency of type i evaluating in a no-market baseline session j was recalculated as $m_{ij} + \phi_i M_j$, where $\phi_i = 1$ if $i = 3$ and $\phi_i = 0$ otherwise. Because the conjecture involves the likelihood that a type 3 agent evaluated, the employed metric is the frequency with which a type 3 agent evaluated over the last 12 periods and not *ex ante* efficiency. We do not suppose that the four institutions are identical under this metric. Thus, a Kruskal-Wallis test is used to test the null hypothesis that the frequency of type 3 evaluations is the same across all five treatments against the alternative hypothesis that this frequency differs for some treatment. The tie-corrected rank test statistic is $\chi^2_3 = 0.352$, which cannot be rejected ($p = 0.9499$). Hence, the mechanisms encourage the appropriate agents to undertake the evaluations.[14] ■

With strictly private information on values and heterogeneous attitudes toward risk, the market mechanism induces the socially optimal person to evaluate the product. This is nontrivial in that no explicit cues from the environment are driving this behavior. Rather, it is the incentives created by the market mechanism that lead the socially optimal agent to provide an evaluation when without a market such an agent would not. Note that the market mechanism could also have induced other agents to evaluate: agents who have the same risk-neutral expected value for evaluating and not evaluating the product. But this is not we what find.

Many experimental studies have shown that efficiency is quite high, generally greater than 95%, for uniform and discriminatory sealed bid and Dutch and English clock auctions with independent private values. Prices, however, often differ substantially by auction format, even though all four institutions are theoretically revenue equivalent with risk-neutral bidders (see footnote 3 for references). Our next finding compares the prices received by the evaluator for each institution. We employ a linear mixed effects model for analyzing the data with repeated measures as the basis for quantitative support.[15] Sessions are indexed by $j = 1, \ldots, 20$ and periods by $t = 37, \ldots, 48$. This parametric estimation treats each session as a random effect ε_j and each institution as a fixed effect β_j. The dependent variable $Price_{jkt}$ is the price received by evaluator k in period t of session j. Within the session random effect, we also include a random effect e_k for subject k within the session that submits the winning bid to be the evaluator. Specifically, we estimate the model

$$Price_{jkt} = \beta_0 + \beta_1 Descending_j + \beta_2 Uniform_j + \beta_3 Discriminatory_j + \varepsilon_j + e_k + u_{jkt},$$

where $\varepsilon_j \sim N(0, \sigma^2_{\varepsilon,j})$, $e_k \sim N(0, \sigma^2_e)$, and $u_{jkt} \sim N(0, \sigma^2_u)$.[16]

Finding 5. The null hypothesis of identical market prices across all four market mechanisms cannot be rejected. Additionally, the theoretical risk-neutral price prediction is included in the confidence interval for any standard level of significance.

Support. Table 3 reports the fixed effects parameter estimates with the ascending Dutch clock auction serving as the basis for comparison. The considerable lack of significance on β_1, β_2, and β_3

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[14] The approach of conducting *ex ante* efficiency analysis similar to that discussed following Finding 3 would assume that the efficient type is assigned to evaluate (i.e., $\theta_i = 1$ if $i = 3$ and $\theta_i = 0$ otherwise. In this case, the null hypothesis that the market mechanisms generate the same level of efficiency as the baseline cannot be rejected in favor of the two-sided alternative with the Wilcoxon rank sum test. This also suggests that the markets induce the optimal agent to evaluate when no one volunteers ($W = 71$, $p = 0.6832$).

[15] See Longford (1993) for a description of this technique that is commonly employed in experimental sciences. A linear mixed effects model is not appropriate for the efficiency analysis because *ex ante* efficiency is discrete, taking on only one of three values each period.

[16] The linear mixed effects model for repeated measures treats each session as 1 degree of freedom with respect to the treatments. Hence, with four parameters, the degrees of freedom for the estimate of the institution treatment fixed effects are 16 = 20 sessions − 4 parameters. This estimation accommodates sessionwise heteroscedastic errors when estimating the model via maximum likelihood. Adjusting the model to include first-order autoregressive errors in u_{jkt} does not significantly increase the efficiency of the estimates.

Table 3. Estimation Results for Linear Mixed Effect Model of Price ($Price_{jkt} = \beta_0 + \beta_1\, Descending_j + \beta_2\, Uniform_j + \beta_3\, Discriminatory_j + u_j + e_k + \varepsilon_{jkt}$)

Parameter	Estimate	Standard Error	Degrees of Freedom	t	p
β_0	47.06	11.763	178	4.00	<0.0001
β_1	11.84	16.647	16	0.711	0.4871
β_2	−5.44	16.615	16	−0.326	0.7475
β_3	4.39	16.661	16	0.263	0.7957

fixed effects indicates that, on average, the *Descending* English clock and *Uniform* price and *Discriminatory* price sealed bid auctions result in the same price, respectively, as the *Ascending* Dutch clock auction. The second part of the finding is supported by a t-test of the null hypothesis that $\beta_0 = 54$ against the two-sided alternative. The test statistic is 0.59 with 178 degrees of freedom, which cannot be rejected at standard levels of significance. ■

Although the average price is statistically indistinguishable under each treatment, it is important to notice that there is considerable variation in observed prices. Figure 4 depicts this price variability. Over the last 12 periods, one session in each treatment has a median price less than 35. Also, over the last 12 periods, one session in each treatment has a median price over 75. Overall, the lowest median price in a session is 3.25, and the highest is 109.17. Even after controlling for variation from the random effects of sessions and the evaluators within each session, considerable variation in the observed prices remains, as evidenced by the size of the standard error on β_0 reported in Table 3. This suggests that while the theoretical price may characterize the central tendency of behavior in these institutions, it is not the case that people are behaving in strict accordance with the prediction. This can also be gleamed from Figure 2. If each subject behaves as theoretically predicted then in sessions with an operating market institution the ex ante efficiency should be 100%, which it clearly is not.

The same heterogeneity among subjects in terms of willingness to evaluate that explains the relatively low efficiency could also contribute to the price variability. Of the 100 subjects in the experiment, 12 clearly enjoyed providing the evaluation, taking on the risk at least 50% of the time in periods 13–24, the last 12 periods before a market mechanism was introduced.[17] Table 4 compares the frequency with which these risk takers evaluated when a market was in operation with the frequency of evaluation by others of the same type in the same institution.[18] Except in two cases, subjects who frequently volunteered to provide an evaluation for free were also more likely to be paid for providing an evaluation than subjects in similar situations. But we also note that of the six type 1 and 2 (inefficient) agents that evaluate more than 50% of the time in the premarket periods, only one evaluates more than 50% of the time after a market mechanism is introduced.

5. Conclusion

As demonstrated by Avery, Resnick, and Zeckhauser (1999), a pricing mechanism for product evaluations can increase efficiency by voluntarily eliciting an evaluation that would otherwise not be provided. With a controlled laboratory experiment, we evaluate the performance of four market mechanisms for providing product evaluations: uniform price sealed bid, discriminatory price sealed

[17] No subject volunteered to evaluate more than 83% of the time during these periods, and in many periods, no evaluations were provided.

[18] Table 4 only includes 11 of the 12 subjects that were in the auction mechanism treatment. The 12th was in the baseline treatment.

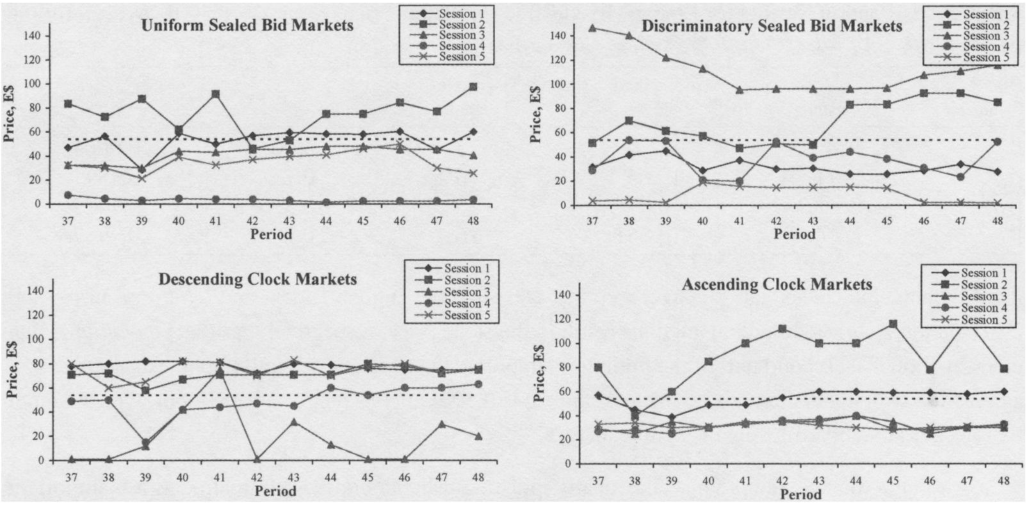


Figure 4. Prices Received by Evaluator for Periods 37–48

bid, descending (English) clock auction, and ascending (Dutch) clock auction. Our results indicate for this nonrivalrous information good that (i) each of these institutions improves social welfare and (ii) the four mechanisms are behaviorally equivalent with respect to the prices received by the evaluator. Each of the four institutions improves efficiency by inducing the socially optimal agent to undertake the evaluation when no one else is willing to do so. In our design, this is not an easy feat. All of the subjects had strictly private information on their values and the same (negative) expected value for evaluating the good. Moreover, the subjects might have heterogeneous attitudes toward risk, and yet, a market mechanism leads the socially optimal agent to voluntarily provide an evaluation when without a market such an agent would not. However, it is not the case that the four market mechanisms achieve 100% efficiency. There is heterogeneity in our subjects, leading some socially suboptimal agents to evaluate. This is consistent with naturally occurring voluntary evaluations, such as book evaluations provided (and not provided) by *Amazon.com.*

These efficiency and price results contrast with the standard experimental results of these auction formats for independent values. In laboratory auctions with independent private values, prices clearly separate with a relatively small variance across sessions, and efficiency is consistently greater than 95%. In those experiments, buyers' values and surplus are induced with certainty each period. In the case of

Table 4. Market Behavior of Subjects Who Evaluated ≥50% of the Time in Periods 13–24

			% of Periods Evaluated with Auction (Periods 37–48)	
Agent Type	No. of Individuals	Auction Treatment	By These Agents	By All Others of the Same Type
2	1	Uniform	58	22
2	1	Uniform	8	22
3	1	Uniform	33	30
2	1	Discriminatory	25	3
3	2	Discriminatory	13	35
2	1	Ascending clock	33	23
3	2	Ascending clock	88	18
1	2	Descending clock	42	11

nonrivalrous good such as evaluation, there is risk in the product value, which might be a source of the price variability that we find here. The large heterogeneity in willingness to evaluate often led agents that were suboptimal from an expected payoff perspective to evaluate. Therefore, the possible efficiency improvement that the market mechanisms could generate was limited. This might account for the similar efficiency performance across the four institutions. Collective or individual heterogeneity of subjects, uncertainty about the outcome, and the nonrivalrous nature of evaluations could explain why the market outcomes here differ from standard auction results in the laboratory. Of course, only a systematic analysis of these differences can reveal the explanation, but that we leave to future research.

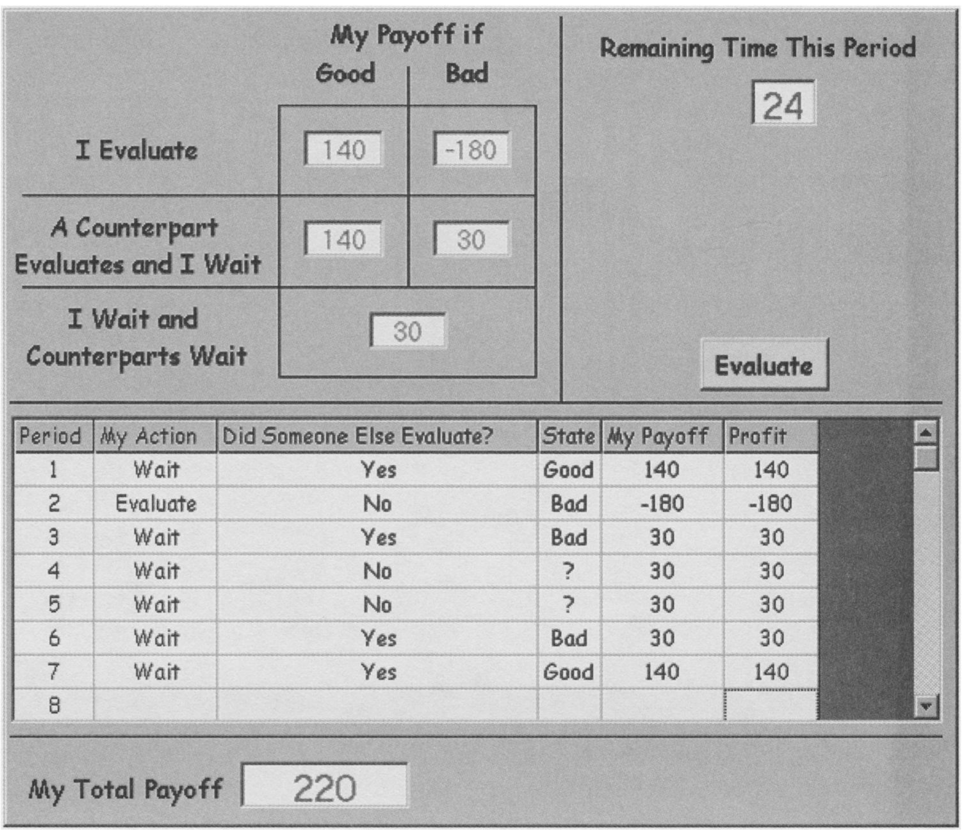
Appendix

Experiment Instructions

This is an experiment in the economics of decision making. Various research foundations have provided funds for this research. The instructions are simple, and if you understand them, you may earn a considerable amount of money that will be paid to you in CASH at the end of the experiment. Your earnings will be determined partly by your decisions and partly by the decisions of others. If you have questions at any time while reading the instructions, please raise your hand and a lab monitor will assist you.

This is what your screen will look like in the experiment. In each period of the experiment you will be matched with three other people, your counterparts. All four of you have a decision to make: either evaluate a fictitious product or wait for a counterpart to do so. Your payoff will be determined in part by the decisions you and your counterparts make.

You and your counterparts for the period will have 30 seconds to decide if you want to evaluate. At any point during the 30 seconds, you or your counterparts can click on the **Evaluate** button. Only one of you can evaluate during a period. If the clock expires without any of you choosing to evaluate, then all of you have waited for that period.



How is your payoff determined? The fictitious product will either be **Good** or **Bad**. When you and your counterparts are making your decision to either evaluate or not, you will not know if the product is good or bad. You and your counterparts will know whether it is good or bad only *after* one of you has chosen to evaluate. There is a 50% chance that the fictitious product will be **Good** and a 50% chance that it will be **Bad**. Your payoff depends on whether the product is **Good** or **Bad**, if you or a counterpart has chosen to evaluate. If any of you evaluate the good, then your payoff is higher if the product is **Good** than if it is **Bad**.

Now we will go through an example of how to read the payoffs which are listed in the table. The payoffs in these instructions are for illustrative purposes only. The payoffs in the experiment will be different from those displayed here.

Suppose that you chose to evaluate. Then your payoff would be determined by the first row of the table. The payoffs depend on whether the product is **Good** or **Bad**. If the product is **Good**, your payoff would be 140. If the product is **Bad**, you payoff would be −180.

Suppose that while you are waiting, your counterpart chooses to evaluate. In this case the second row displays your payoff. If the product is **Good**, your payoff would be 140. If the product is **Bad**, your payoff would be 30.

Finally, is none of you decide to evaluate, then your payoff is shown on the bottom row. Notice that your payoff will be 30, regardless of the product being **Good** or **Bad**. None of you will know the state of the product that period because none of you chose to evaluate it.

At the end of the period you will have **5** seconds to review the results. At the end of that time, the clock will reappear and the next period will begin. Your counterparts' payoffs may or may not be the same as yours.

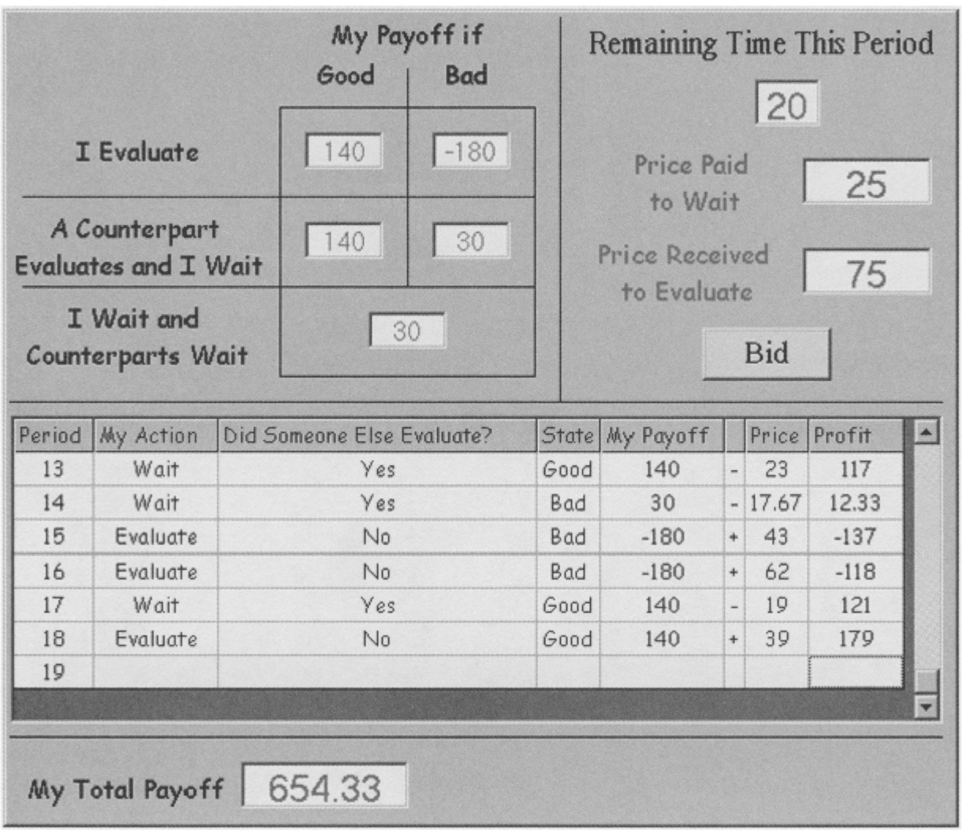
At the end of the experiment, your experimental dollars will be converted into cash at the rate of **200** experimental dollars for **US$1**.

Any questions? If not, please raise your hand to indicate that you have finished reading the instructions.

Uniform Price Sealed Bid Auction Instructions

For the next portion of the experiment, the way you and your counterparts' payoffs are determined based on the fictitious good will remain the same, but all of you will be bidding to pay to **Wait** and to get paid to **Evaluate**. In each period of the experiment you will continue to be matched with the same three counterparts.

This is what your screen will look like in the next portion of the experiment. Each period there will be three people who



will wait and one person who will evaluate. Whether you wait or evaluate depends upon the bids that you and the other three people submit.

At the beginning of each period you will submit a bid for the "Price Received to Evaluate." This is the amount you are willing *to be paid* to evaluate. One third of this bid also serves as the price you are willing *to pay* to wait.

Once all of the bids have been submitted, the computer ranks the bids from lowest to highest. The person who is chosen to **evaluate** is the person who submits the **lowest** bid. (Any ties will be broken randomly.) This person will receive as payment the average of her bid and the second lowest bid. Hence, the person who evaluates will always be paid at least as much as the bid that she submitted. This amount will be recorded in the "Price" column and will be added to the "My Payoff" column for that period.

The three people who submitted the three highest bids will each *pay* $^1/_3$ of the amount received by the evaluator. This amount will be recorded in the "Price" column and will be subtracted from the "My Payoff" column for that period. Notice that these three people will not pay more than the amount they each submitted as the "Price Paid to Wait."

Just as before, the evaluator will also receive a payoff depending upon whether the product is **Good** or **Bad**.

At the end of the period you will have **5** seconds to review the results. At the end of that time, the next period will begin. If you do not submit a new bid before the time on the clock expires, the computer will use last period's bid as the bid for the current period.

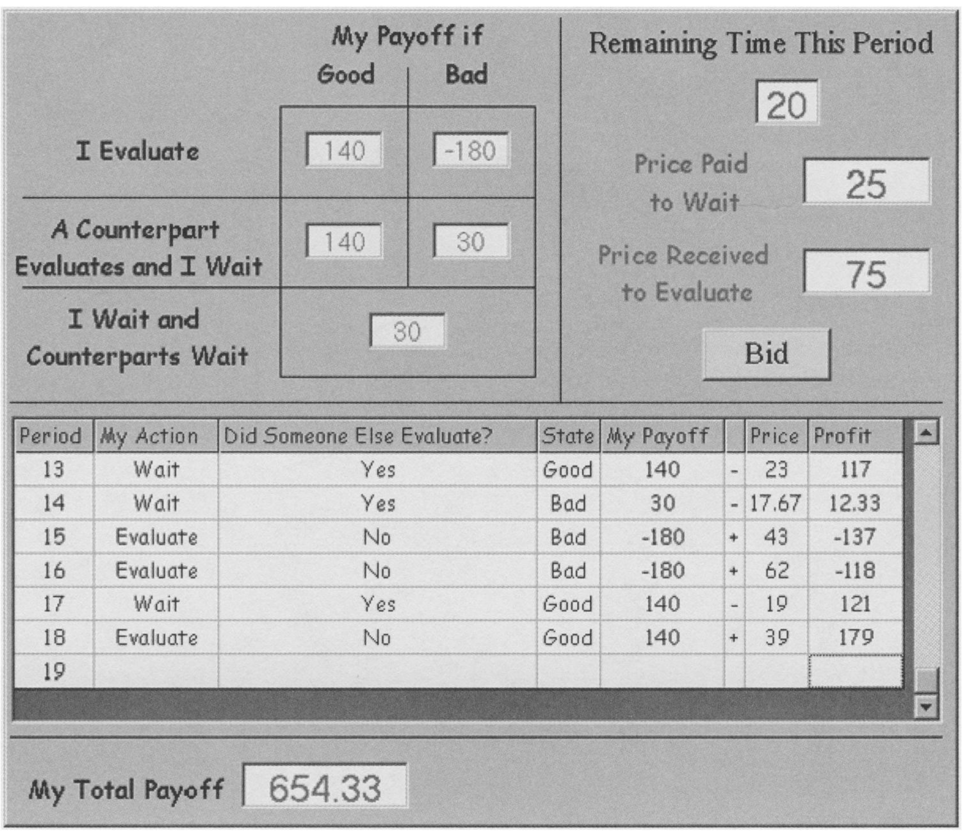
At the end of the experiment, your experimental dollars will also be converted into cash at the rate of **200** experimental dollars for **US$1**.

Any questions? If not, please raise your hand to indicate that you have finished reading the instructions.

Discriminatory Price Sealed Bid Auction Instructions

For the next portion of the experiment, the way you and your counterparts' payoffs are determined based on the fictitious good will remain the same, but all of you will be bidding to pay to **Wait** and to get paid to **Evaluate**. In each period of the experiment you will continue to be matched with the same three counterparts.

This is what your screen will look like in the next portion of the experiment. Each period there will be three people who will wait and one person who will evaluate. Whether you wait or evaluate depends upon the bids that you and the other three people submit.



At the beginning of each period you will submit a bid for the "Price Received to Evaluate." This is the amount you are willing *to be paid* to evaluate. One third of this bid also serves as the price you are willing *to pay* to wait.

Once all of the bids have been submitted, the computer ranks the bids from lowest to highest. The person who is chosen to **evaluate** is the person who submits the **lowest** bid. (Any ties will be broken randomly.)

The three people who submitted the three highest bids will each *pay* the evaluator. The amount that each waiter pays has two factors. The first factor is the average of their own bid and the lowest bid. This amount is then multiplied by 1/3 (or equivalently divided by 3) because each of the three waiters pays the evaluator. This amount will be recorded in the "Price" column and will be subtracted from the "My Payoff" column for that period. Notice that these three people will not pay more than the amount they each submitted as the "Price Paid to Wait."

The evaluator will receive as payment each of the amounts paid by the three waiters. Notice that the evaluator will always be paid at least as much as the bid that she submitted. This amount will be recorded in the "Price" column and will be added to the "My Payoff" column for that period.

Just as before, the evaluator will also receive a payoff depending upon whether the product is **Good** or **Bad**.

At the end of the period you will have **5** seconds to review the results. At the end of that time, the next period will begin. If you do not submit a new bid before the time on the clock expires, the computer will use last period's bid as the bid for the current period.

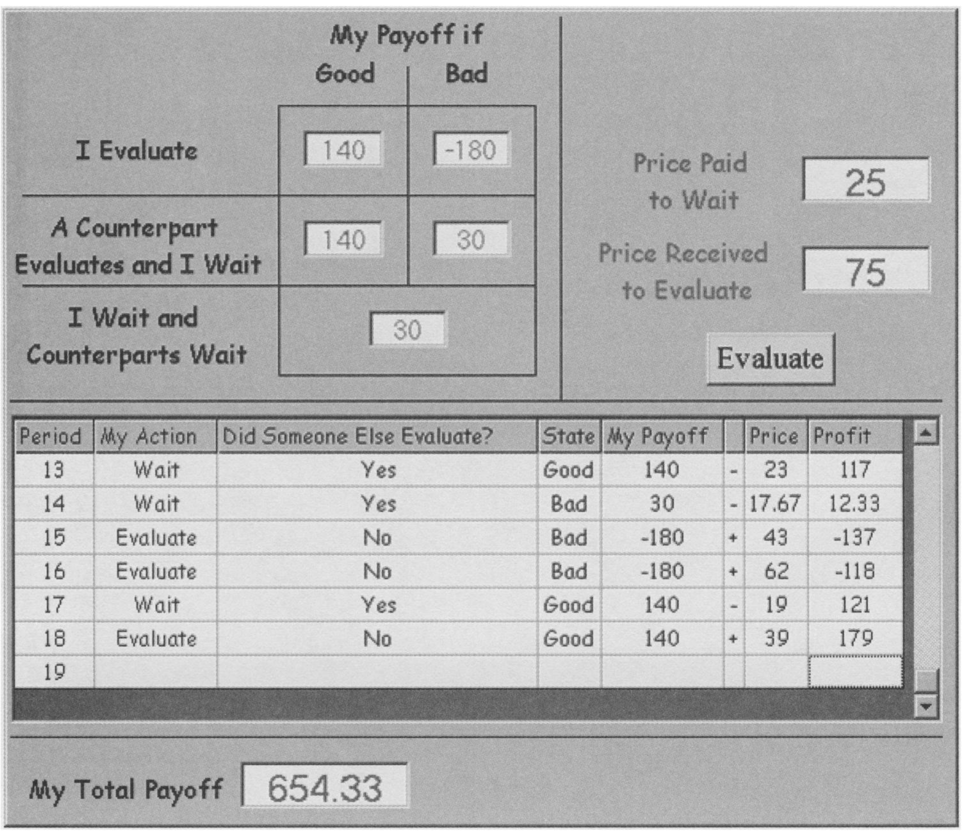
At the end of the experiment, your experimental dollars will also be converted into cash at the rate of **200** experimental dollars for **US$1**.

Any questions? If not, please raise your hand to indicate that you have finished reading the instructions.

Ascending Clock Auction Instructions

For the next portion of the experiment, the way you and your counterparts' payoffs are determined based on the fictitious good will remain the same, but all of you will be bidding to pay to **Wait** and to get paid to **Evaluate**. In each period of the experiment you will continue to be matched with the same three counterparts.

This is what your screen will look like in the next portion of the experiment. At the beginning of each period the "Price



Received to Evaluate'' starts at a price of **24** and then continues to increase by one experimental dollar each second. The ''Price Received to Evaluate'' will increase until the first person clicks on the **Evaluate** button. The first person who clicks on the **Evaluate** button will evaluate the product and receive as payment from the three counterparts the amount in the ''Price Received to Evaluate'' box. This amount will be recorded in the ''Price'' column and will be added to the ''My Payoff'' column for that period.

Just as before, the evaluator will also receive a payoff depending upon whether the product is **Good** or **Bad**.

The three other people who did *not* click on the **Evaluate** button will wait that particular period. The three waiters will each pay the amount next to the label ''Price Paid to Wait.'' This amount will be subtracted from the ''My Payoff'' column. Notice that because there are three waiters, the amount paid by each waiter is $^1/_3$ of the price received by the evaluator.

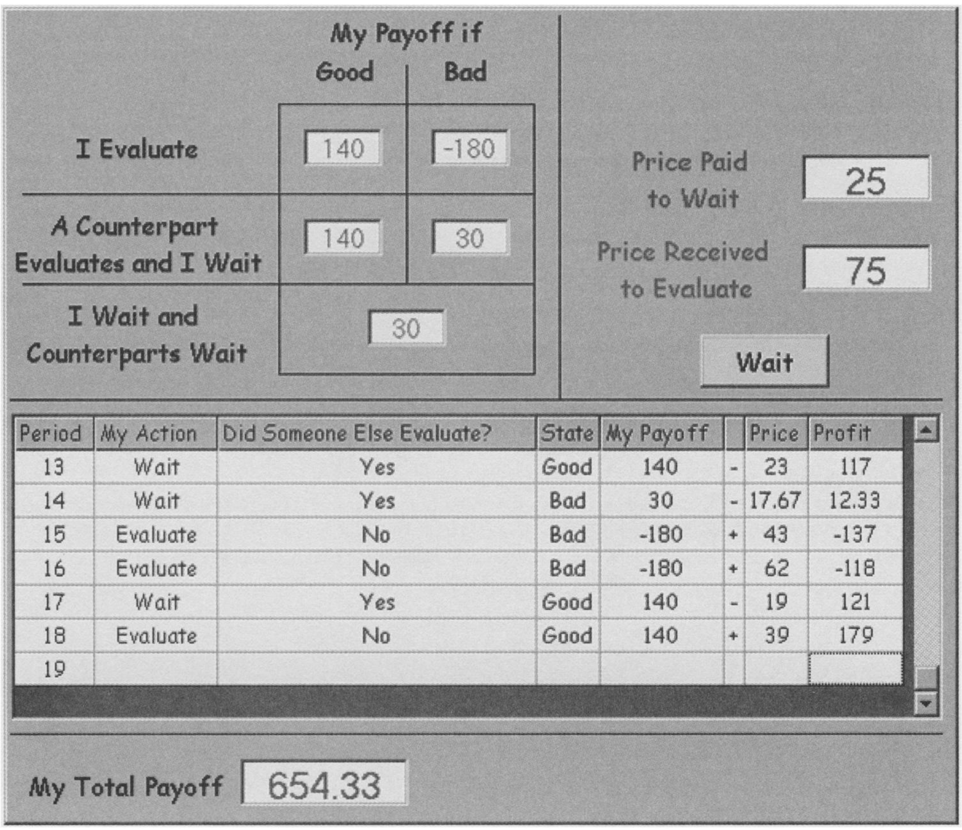
At the end of the period you will have **5** seconds to review the results. At the end of that time, the prices will again start at **24** and will increase until one person clicks on the **Evaluate** button.

At the end of the experiment, your experimental dollars will also be converted into cash at the rate of **200** experimental dollars for **US$1**.

Any questions? If not, please raise your hand to indicate that you have finished reading the instructions.

Descending Clock Auction Instructions

For the next portion of the experiment, the way you and your counterparts' payoffs are determined based on the fictitious good will remain the same, but all of you will be bidding to pay to **Wait** and to get paid to **Evaluate**. In each period of the experiment you will continue to be matched with the same three counterparts.



This is what your screen will look like in the next portion of the experiment. At the beginning of each period the ''Price Received to Evaluate'' starts at a price of 84 and then continues to decrease by one experimental dollar each second. The ''Price Received to Evaluate'' will decrease until the first *three* people click on the **Wait** button. The remaining person who has not clicked on the **Wait** button will evaluate the product and receive as payment from the three counterparts the amount in ''Price Received to Evaluate'' box. This amount will be recorded in the ''Price'' column and will be added to the ''My Payoff'' column for that period.

Just as before, the evaluator will also receive a payoff depending upon whether the product is **Good** or **Bad**.

The three other people who clicked on the **Wait** button will wait that particular period. The three waiters will each pay the amount next to the label "Price Paid to Wait." This amount will be subtracted from the "My Payoff" column. Notice that because there are three waiters, the amount paid by each waiter is $1/3$ of the price received by the evaluator.

If the "Price Received to Evaluate" falls to zero and there are still at least two people who have not clicked on the **Wait** button, then one of the people who has not clicked the button will be randomly selected to be the evaluator and will receive zero (because by not clicking the **Wait** button the person has indicated that they are willing to receiving nothing for evaluating).

At the end of the period you will have **5** seconds to review the results. At the end of that time, the prices will again start at 84 and will decrease until the first **three** people click on the **Wait** button.

At the end of the experiment, your experimental dollars will also be converted into cash at the rate of **200** experimental dollars for **US$1**.

Any questions? If not, please raise your hand to indicate that you have finished reading the instructions.

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