Trading Complex Assets*

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Abstract

We perform an experimental study of complexity to assess its effect on trading behavior, price volatility, liquidity, and trade efficiency. Subjects were asked to deduce the value of a particular asset from information they were given about the composition and price of several portfolios. Following that, subjects traded with each other anonymously in a well-defined, simple bargaining process. Portfolio composition ranged from requiring simple analysis to more complicated computation in order to deduce the value of the asset. Complexity altered subjects' bidding strategies, decreased liquidity, increased price volatility, and decreased trade efficiency. However, in follow-up experiments, we show that uncertainty over private values does not lead to the same changes in trading behavior. Therefore, while complexity induces estimation errors and higher uncertainty, this is not what drives our results.

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1 Introduction

Complexity bounds the ability of market participants to accurately value assets. Some assets are easier to analyze (e.g., treasury bonds), whereas others have unbounded contingencies that prevent humans from pinning down their exact values (e.g., corporate bonds with embedded American options and credit default swaps). Indeed, for many financial assets there does not exist closed-form analytical solutions to quantify their value. Moreover, as securities are serially repackaged (e.g. collateralized debt obligations), this further complicates values and leads to higher uncertainty of assessments. In the end, even though complexity may increase uncertainty, this is not its most salient feature. Complexity makes it difficult for market participants to forecast the essential inputs required to value the asset in the first place. Moreover, complexity makes it harder to know whether counterparties have an informational or skill advantage.

The purpose of this paper is to explore how complexity affects trading in a market setting. Specifically, we address the following questions: How does complexity affect willingness-to-trade (i.e., liquidity), price volatility, and gains from trade? Can estimation errors alone (i.e., additional noise) account for these effects? Do differences in demographic background change peoples' responses to complexity? What is the channel through which complexity affects trading behavior?

We address these questions by studying complexity in a laboratory setting. Participants were asked to evaluate the price of certain assets and were then given the opportunity to make trades based on their information. They each participated in fifteen distinct periods, each of which was composed of two stages. In the first stage, each participant was given information regarding several portfolios composed of four assets and was asked to submit their best estimate of the value of a particular asset included in these portfolios. Following that, in the second stage, participants were randomly paired and were given the opportunity to trade the asset through a well-defined, anonymous bargaining process. Each pair contained one buyer and one seller such that buyer's private value of the asset was always greater than seller's private value of the asset. The complexity involved in assessing the asset's value from the portfolios varied across rounds, and we collected information regarding frequency of trade, trading prices, and trading surplus as a function of complexity.

Our results show that complexity affected both the liquidity and price volatility of the assets traded. The frequency of transactions was significantly lower and the payoff asymmetry was significantly higher when the computation required was more complex. Importantly, though, these findings impacted the trade surplus generated in each round: making the required computation simpler increased the trade efficiency by 11% (from 73% to 81%); this was more pronounced when there were more bidding rounds allowed. Whereas efficiency rose from 73% to 84% for the simple treatment when the number of rounds increased from one to three, efficiency remained unimproved in the complex treatment (72% versus 73%). While on average some participants enjoyed an advantage over their counterparts with complexity – higher payoff asymmetry in that treatment – the aggregate surplus tended to be lower.

Complexity also increased prices volatility. At first blush, this might be explained by the fact that participants tended to make more computational errors when valuing the complex assets. However, our analysis shows that this is unlikely to be the case. First, we estimate the sellers' and buyers' bidding strategies as a function of their *estimates* with and without complexity. While controlling for guess estimates, we show that the bid as a function of each trader's estimate is much flatter when the portfolio problem is more complex. This implies that the subjects were more conservative in their bidding when faced with complexity. Furthermore, we find that even subjects who were able to estimate their value correctly in both the simple and complex treatment, bidding strategies flattened in the complex treatment.

Second, different demographic factors had no effect on estimation errors during the experiment, but did change trading and bidding behavior. We collected information such as gender, educational background (i.e., college major), and intellect (i.e., grade point average in college). None of these characteristics altered the tendency for subjects to make estimation errors or on their overall earnings. This was not the case for trading behavior, however. We found that female participants were more affected by complexity: they exhibited greater reduction in transaction frequency, higher payoff asymmetry, a larger bid gap for transactions that failed to occur, and higher price volatility. Females did enjoy *higher* payoffs in the complex treatment, however. Educational background was also related to the effect of complexity on trading. Participants with an economics background (as opposed to engineering background) had a less severe drop in trading frequency, a higher payoff asymmetry, and a lower bid gap for transactions that failed to occur. Finally, college GPA did not predict any differences in any of the treatment groups.

To explore this further, we ran a second set of experiments in which participants faced uncertainty about their private values, but did not have to solve portfolio problems. We linked these experiments to the previous ones by giving subjects a noisy signal about their private value, where we added uncertainty according to the observed average estimation error from the first experiments. We found no statistically discernible difference in efficiency, liquidity, or payoff asymmetry in these experiments when we varied the uncertainty subjects faced about their private values. This suggests that added noise due to estimation error is unlikely to account for the trading effects induced by complexity.

Why, then, does complexity decrease liquidity and increase price volatility if it is not due to added uncertainty over private values? The answer probably rests on adverse selection. That is, as problem solving becomes more challenging, people become concerned whether their trading partner knows more. Theoretically, without any uncertainty over private values, the bargaining game used in our experiment gives rise to an equilibrium which does not achieve full efficiency. Adding exogenous noise may not exacerbate the problem: for example, consider an extreme case in which the problem is so complex that it provides the players with no information about their private values. In such a case, full efficiency can be supported in equilibrium as both players bids are not a function of their private values and they know that their counterparty is also uninformed. However, in a case in which one trader has not figured out the problem and another has (or at least there is a belief over whether they did), it is straightforward to consider that an adverse selection problem may limit trade. Exploring the theoretical underpinnings of this is the subject of future research.

The subjects in our study viewed valuation requiring computational difficulty as being more complex: they were more reticent to trade, even if they correctly determined the value of the asset. Overall, we observed lower trade efficiency in complex treatments. However, it is important to note that computational difficulty is only one potential proxy for asset complexity, and may not be a sufficient condition for particular securities to be *perceived* as complex. For example, consider assessing the value of Goldman Sachs stock. Indeed, their portfolio is immense, changes dynamically, and has unbounded contingencies. Yet, many investors might not perceive Goldman Sachs stock to be complex, especially when compared to a particular exotic option or serially securitized asset.[1] Therefore, while we use computational complexity as a convenient experimental proxy in the analysis here, we do not assert that this is the only way in which complexity may arise or be perceived.

As pointed out by Brunnermeier and Oehmke (2009), asset complexity may have asset pricing implications and may drive how assets are managed and traded. For example, Arora, Barak, Brunnermeier, and Ge (2010) show that once complexity is taken into consideration, derivatives can actually worsen asymmetric information costs instead of decreasing them. Bernardo and Cornell (1997) provide empirical evidence that the complexity of collateralized mortgage obligations (CMO's) causes the variance of bids to be much larger than can be explained by estimation error alone. In comparison to empirical study like these, our experimental investigation does not use professional traders from real markets. However, our work complements theirs because we control for confounding variables that would make real-world tests challenging: imperfect information, hidden attributes (e.g., quality), relationships between traders, self-selection, and the innate liquidity of assets. Moreover, our work also makes several novel predictions that might be studied in financial markets. For example, our results imply that regulation requiring asset standardization should decrease price volatility, increase liquidity, and generate welfare (though increased trade surplus). Likewise, assets with more contingencies should have more price volatility than predicted by the underlying assets used to replicate them.

Our work adds to a growing literature on complexity in financial markets, which demonstrates that complexity is a robust concern that is not alleviated with competition. Carlin (2009) studies the effect of competition on complexity and shows that as the number of firms rises, each firm adds more complexity to its prices. Carlin, Iannaccone, and Davies (2010) show that discretionary

[1] The authors thank Pierre Collin-Dufresne for pointing this out and for this example.

disclosure and market transparency are minimized with perfect competition. Carlin and Manso (2010) show that educational initiatives undertaken by a social planner to increase sophistication may worsen the amount of complexity in the market. Given the presence of such forces, our analysis here appears economically important and interesting.

To our knowledge, our paper is the first to explore the effect of complexity on asset trading. Healy et al. (2010) examine the performance of different market mechanisms in aggregating information in simple or complex environments, but their setting does not permit direct comparisons of the performance of traders in these different settings. There are also other experimental papers that have explored various aspects of bargaining games related to ours, which is often referred to as an incomplete information sealed-bid type. Chatterjee and Samuelson (1983) theoretically analyze such a bargaining game and show that the Nash equilibrium strategy is monotonic in bidders' reservation values. Radner and Schotter (1989) test this experimentally and find that subjects do use strategies that approximate monotonic, linear bidding functions and that subjects capture a large fraction of the available trading surplus. Schotter (1990) discusses a large set of experiments using the same bargaining mechanism while varying different features of the environment. Bidding strategies largely remain monotonic, if not always linear, but the efficiency of the mechanism remains intact. Our work adds to theirs in several respects. Whereas Radner and Schotter (1989) and Schotter (1990) provide their subjects with precise information about their private values, we do not. Instead, our subjects are given full information in a form that requires computation. In some cases, this may lead subjects to have uncertainty about their private value, which may affect their trading behavior. Indeed, as we show, complexity can affect the linear bidding strategy, making it less responsive to changes in value estimates. Further, as we show, while the simple treatments with three bidding rounds reproduced the trade efficiency in Schotter (1990), complexity had an adverse effect on welfare.

Finally, Radner and Schotter (1989) and Schotter (1990) focus on cases where the private values of traders are independent. In contrast, we consider the case where private values are affiliated, which is more relevant to the analysis of financial markets. In this sense the closest theoretical work to ours is Kadan (2007), who investigates the theoretical properties of k-double auctions (a generalization of the bargaining mechanism we employ) with affiliated private values. Kadan (2007), however, also does not consider the case where traders are not directly given their value and also does not consider the specific private value model we consider.

The rest of the paper is organized as follows. In Section 2, we describe our experimental set-up. In Section 3, we describe our data. Section 4 characterizes our results. Section 5 provides some concluding remarks.

2 Experimental Design

Every subject in the study participated in one, and only one, experimental session. Each session was composed of fifteen periods. At the beginning of each period, every subject was given information about the composition and price of four portfolios. They were asked to estimate the value of a particular asset within the portfolio, and this estimate was recorded. Following that, each subject was allowed to trade with an anonymous partner (i.e., another subject) in a well-defined, simple bargaining process that we specify shortly. Assets were traded in Experimental Currency Units (ECU's), with the exchange rate being one ECU equal to ten cents.

The value of the particular security of interest could be solved deductively by using the principal of no arbitrage. Specifically, the subjects received information about four baskets of securities, labeled 'Basket 1' through 'Basket 4'. Each basket contained quantities of four securities, labeled 'Security A' through 'Security D'. Subjects were given information such as the number of units of each security in each basket and the price of each basket. Figure 1 and Figure 2 provide examples of typical problems that a subject might face.

Given the information provided, the problems faced by the subjects were either Simple (as in Figure 1) or Complex (as in Figure 2). Simple portfolio problems could often be solved by inspection, or with minimal computation. Complex problems required more effort and ingenuity. However, no matter how challenging the problem, the information was sufficient to determine the price of the traded security with certainty. Each subject was given three minutes to estimate the asset's value (i.e., assess the value of security D). We divided the fifteen periods in the session into three sets of five periods, with each set either containing simple or complex problems. We made sure that every fifteen-period session included at least one set of Simple and one set of Complex periods, so that we could study within-subject variation in trading behavior. In each period, subjects were asked to submit their best estimates of the fundamental value of the security in question. Subjects whose guess fell within one unit of their true private value received an additional five ECUs.[2]

Following this, each subject participated in a simple and intuitive bargaining game. They were assigned the role of a buyer or a seller, and were randomly paired with another subject with the opposite role. The asset carried a different private value to the buyer and the seller. Specifically, the seller's private value was drawn from a uniform distribution from one to twenty. The buyer's private value was equal to seller's realized private value plus a random draw from a uniform distribution from zero to twenty. The two random draws were independent across subjects and rounds. This structure was designed to be easy to explain to subjects and to ensure that trade is ex-ante efficient.

Subjects were allowed to trade anonymously over either one or three bargaining rounds, chosen randomly. For any bargaining round, the subjects were given thirty seconds to simultaneously submit bids. If either trading partner failed to do so, the bidding round terminated. If the bid submitted by the buyer (weakly) exceeded that submitted by the seller, a transaction occurred in which the buyer paid the seller the average value of the bids. The payoff from a trade for the buyer was equal to their true value of the security minus the traded price. Likewise, the payoff from a trade for the seller was equal to the transaction price minus their true value of the security. In periods with one bargaining round, if no transaction took place, no more bargaining was allowed. In periods with three bargaining rounds, if a bargaining round did not result in a transaction, subjects were notified that no transaction occurred but were not informed of each others' bids. If a transaction did not occur by the end of the three bargaining rounds, subjects forfeited any value from trade.

The following is a timeline of what information the subjects had during each session. First, before each subject began the study, they were given full instructions regarding the protocol. We confirmed understanding of the instructions by giving each subject a formal quiz to test their proficiency regarding the protocol. Following that, at the time that subjects were given each

[2]The first two sessions were conducted without explicitly rewarding subjects for accurate guesses. However, we find no difference in subjects' errors between the first two sessions and the remaining sessions.

portfolio problem, they were also assigned a role of seller or buyer, and were told whether there would be one or three bargaining rounds in the trading game. During each session, we did not allow subjects to communicate with each other. Information collected from subjects and trade between them occurred anonymously via a computer terminal, utilizing a standard z-tree program (Fischbacher, 2003).[3] Finally, it is important to note that both buyers and sellers were provided with the same set of securities with the only difference being the price of the baskets. Thus, the buyer and the seller in each interaction faced the same level of difficulty in ascertaining their own private value. The value of the buyer's baskets was set so that the true value of security D was higher than for the seller. That difference was randomly chosen from a uniform distribution ranging from zero to twenty.

At the end of each period, subjects were informed whether a trade had occurred and the value of Security D to them. At the end of the experiment, subjects received a detailed account of their ECU earnings in each period, and their total pay, which was remitted in U.S. dollars.[4]

3 Data

The data was collected over the course of five independent sessions at the McCombs School of Business, at the University of Texas at Austin. Sessions typically lasted just over an hour and the average pay was $15 (including the $5 show-up fee), with a standard deviation of $4.30.

Panel A in Table 1 describes the data collected. Sessions varied in the total number of subjects (due to variation in enrollment) and the number and order of Simple and Complex periods. However, the total number of periods in each condition is roughly the same: 242 in the Simple treatment and 272 in the Complex treatment. In total, 70 subjects participated in the experiment with no subject attending more than one session.

At the end of each session, subjects completed a short demographics survey. In our sample, the majority of subjects were male (61 of 70), majored in economics (47 of 70), and were at an advanced stage of their school work (47 were third or higher year). The mean (self-reported) GPA was 3.48.

[3]Screen shots of the instructions and the bargaining platform are available upon request.
[4]The screen shots with instructions are included at the end of the paper.

We did not apply any filtering to the data. The only observations that were dropped from the analysis were those in which one or both subjects did not submit a bid during a given round. This happened in 33 rounds (14 of which occurred in the Simple condition and 19 occurred in the Complex condition) of the 804 total rounds of the experiment.

4 Analysis

4.1 Session Level Results

We start by providing simple session-level descriptive statistics. Panel A of Table 2 confirms that periods designed to be complex were perceived differently by subjects than periods designed to be simple. In this table, we report for each session the average, across subjects and periods, of the variables discussed below. The last column of Table 2 reports the p-values from a t-test of mean differences between the two conditions (Simple vs. Complex). This test makes the most conservative use of the data as it treats all observations collected in a given session and treatment condition as a *single observation*. This is intended to capture any correlations across periods and subjects.

Recall that at the beginning of each period, subjects were asked to guess the value of the security. We compare these estimates with the actual value of the security to assess how estimation errors varied with treatments. First, we find that complexity leads to more estimation errors. Subjects guessed the value correctly 72% of the time in the simple treatment and 31% of the time in the complex treatment. Likewise, the average guess error increased from 2.38 in the Simple treatment to 7.55 in the Complex treatment. Finally, we observe a difference in bid errors, not just estimation errors. While it is hard to determine what an optimal strategy might be, it is clear that buyers cannot benefit from submitting bids that exceed their private value estimates. Likewise, sellers cannot benefit from submitting bids that are lower than their private value estimates. Indeed, bids violating these condition are relatively rare in both treatments. However, we find that the frequency of bid errors was higher in the Complex treatment compared with the Simple treatment (22% vs. 14%). As the mean test at the session level results suggest, these differences are all statistically significant at the 10% level. Payoffs from trading were somewhat higher in the Simple treatment

(3.98 vs. 3.58 ECUs) but this difference was not statistically different.

4.2 Within-Subject Treatment Effect

Three broad questions that we addressed were:

(i) Does complexity affect liquidity and efficiency, i.e., the creation of surplus?

(ii) Does complexity affect the division of surplus?

(iii) Does complexity affect price volatility?

To analyze these questions, we created the following dependent variables:

(i) Liquidity

- **Frequency of Transaction**: the fraction of period ended with a transaction $(0, 1)$.
- **Bid Gap**: the bid deviation between the two players (in the last round of bidding) when final bids did not result in transactions. This can be thought of an additional measure of market liquidity (in addition to the frequency of transactions).
- **Number of Rounds Used**: the number of rounds used in a period conditional on there being three possible rounds of bidding.

(ii) Efficiency

- **Efficiency**: the fraction of total surplus available from trade that is captured by both parties together.

(iii) Division

- **Payoff Asymmetry**: the surplus deviation from trading between the two players. This measure captures the payoff uncertainty involved in transacting in the market.

(iv) Volatility

- **Price Volatility**: the volatility of normalized transaction prices (across groups and periods). Normalized transaction prices are obtained by subtracting the mid-point of subjects' private values from the traded price.

Panel B of Table 2 demonstrates the treatment effect on these dependent variables. We find that complexity decreased the frequency of transactions by about 6%, increased payoff asymmetry by 40%, increased the bid gap by 20%, and increased price volatility by 38%. These differences are economically large and statistically different from zero at the 10% significance level.

Most striking, maybe, is the substantial reduction in efficiency. In the Simple treatment, the average efficiency was 81%, which is consistent with the efficiency level found in previous experiments that used this bargaining mechanism (e.g., Schotter, 1990). In contrast, the efficiency in the Complex treatment was 73%. This difference is both economically and statistically significant. It is important to note that prior literature found the level of efficiency to be a robust feature of the bargaining *institution* that is studied here, and not the environment. Schotter (1990), on page 222, states that: "These results substantiate the claim that the mechanism appears to be robust not only to the parameters of the environment but also to the manner in which people behave under it given these parameters". The reduction in efficiency we observe as result of introducing complexity is therefore significant.

Our main empirical approach in the paper was to utilize the within-subject design of the experiment while making conservative use of the data. Since our focus was on the main treatment effect, we treated every subject as a unit of observation, averaging the dependent variable (e.g., frequency of transaction) within each of the conditions and taking the difference between the two averages. That is, each observation in the tests that we performed is the difference in the level of the dependent variable across the two treatments for a given subject. Under the null, this difference should be zero. This approach has a few advantages. First, it controls for the idiosyncratic attributes of each subject's individual behavior as it is netted out when taking differences. Second, it controls for the correlation in behavior across periods for a given subject by measuring the dependent variable as the average across all periods.

We regressed the difference in each of the measures on a constant and a set of subject characteristics. Table 3 presents the results, separated into three panels. Consistent with the session-level summary statistics, we found that complexity lowered the frequency of transactions, increased payoff asymmetry, increased the bid gap, and increased the traded price volatility. We did not find that complexity affected the number of rounds used in a period or the payoffs. The last result may appear surprising at first, but becomes more intuitive when considering the random variables that impacted the payoff realizations. The payoff in a given period depends on whether a transaction took place, the level of surplus (i.e., the difference between the buyer's and seller's true values), and the split of that surplus. Given that the total surplus was randomly drawn for each period and did not depend on the subjects' decisions, payoffs would be a noisy proxy for transaction frequency. Given that payoff asymmetry increased with complexity, the variance of payoffs would go up with complexity.

Table 3 also relates the treatment effect to subject characteristics. Specifically, we ask whether the magnitude of the treatment effect is related to subjects' number of years in school, GPA, gender, and major. To reduce the risk of over-fitting, we did not run the regressions with one characteristic at a time; if we did so, the ratio of independent variables to observations would become very low.

The two characteristics that emerged as being significantly related to the treatment effect, across a number of measures, were gender and college major. The results suggest that female subjects were more affected by the treatment compared with male subjects, as measured by transaction frequency, payoff asymmetry, and price volatility. At the same time, female subjects appeared to obtain *higher* payoffs in the Complex treatment (compared with the Simple treatment), while male subjects appeared to earn *lower* payoffs in the Complex treatment. In contrast, economics majors were affected *less* by complexity, as compared to non-economics majors (primarily engineering majors), as measured by the number of rounds used and by price volatility. However, economics majors earned *less* in the Complex condition compared with the Simple condition.

To check whether these differential treatment effects were driven by a latent relationship between subject characteristics and their overall performance in the experiment, we regressed the various measures of errors and payoffs on the same set of characteristics. For example, it may be the case that economics students were less prone to making mistakes in the experiment, and that the Complex treatment simply loads on this tendency. As we can see from Table 4, this was not the case: none of the characteristics, including gender and major, were significantly related to measures of errors or payoffs.

4.3 Bidding Strategies

To better understand what is driving these aggregate results, we studied subjects' bidding strategies. Equilibrium bidding behavior in the environment we study is more complicated than that in the independent private values setting studied in most of the experimental literature. To see why, observe that the precision of the buyer's information about the seller's signal depends on the signal the buyer receives. If the buyer learns that his own value is 40, he knows that the seller has value 20, while if the buyer learns that his own value is 21 then he puts equal weight on the seller having any value between 1 and 20. Thus, even under the assumption that traders are able to perfectly observe their private value, equilibrium bid functions do not take the the simple, linear form studied for independent private value auctions. Adding in the fact that traders do not perfectly observe their own private values adds a further, significant complication to equilibrium analysis. While being only an approximation, we estimate linear strategies for the traders.[5] This provides a simple measure of how traders respond to their information without imposing likely unjustified structure on trader behavior.

We estimate bid functions for buyers and sellers and analyze separately the bid functions in the single round and three round bargaining environments. Our primary focus here is to test whether this function is different across treatments.

For each subset of observations, we estimated the following regression:

$$Bid_{i,t} = \beta_0 + \beta_1 \times Guess_{i,t} + \beta_2 \times Complex_i + \beta_3 \times Guess_{i,t} \times Complex_i + \epsilon_{i,t} \qquad (1)$$

where $Guess_{i,t}$ is subject's i's guess of the security value in period i, $Complex_i$ is a dummy representing the treatment in period i such that it is equal to one if the condition is Complex (and zero

[5] Focus on linear bidding strategies is common in the experimental literature, and Radner and Schotter (1989) and Schotter (1990) suggest that subjects' bids are approximately linear in their private value when a linear equilibrium exists.

otherwise).

Table 5 presents the estimation results, when using robust regressions to control for outliers, and Figure 3 depicts the bid functions of buyers and sellers in the Simple and Complex condition in one-round periods. First, it is clear that the bids were generally increasing in the subjects' guesses of the security's value. Second, the buyers' and sellers' bidding functions appeared to be somewhat different. Recall that the parameters of our experiment were such that unconditionally (before receiving information about the baskets), sellers' private values of the traded security were uniformly distributed between one and twenty, while buyers' private values of the traded security were uniformly distributed between one and forty. The figures also suggest that the bid function was different across the treatment conditions. In the Complex treatment, the bidding function appeared to be flatter compared with the Simple condition.

The first two columns in Table 5 use data from periods in which there was only one bargaining round, and the last two columns use data from the first round in periods where three bargaining rounds were available. We find that the linear bidding function describes the data very well – the coefficient on subjects' guesses is positive and statistically different from zero across all columns. In addition, the explanatory power of the model is quite high, with R^2s ranging from around 19.5% to 68.5%. In addition, we find a strong treatment effect. In all columns, the bid function is *less* responsive to the subjects' own guess of the value in the Complex condition, compared with the Simple condition. Finally, consistent with one's intuition, we find that subjects' bid functions are less aggressive when they have more negotiation rounds. That is, seller's private value guess coefficient in the three round periods is 0.57, down from 0.74 in the one round periods. Likewise, buyers' private value guess coefficient in the three round periods is 0.61, down from 0.69 in the one round periods. At the same time, the effect of complexity on the bid function appears to be similar. [6]

[6]These results are robust to the inclusion of subject fixed-effects or subject errors.

4.4 Multi-round Periods

We now turn to look at subjects' strategies in periods with multiple bargaining rounds. As we saw from Table 5, the first round of bidding in these periods was characterized by less aggressive bidding by both buyers and sellers as compared with the case when only one round of bidding was available. One would expect subjects to improve their bids after each round of failed bargaining. To test that, we create two measures:

- **Bid Change**: current minus previous bid for sellers, and previous minus current bid for buyers. This quantity is positive when buyers and sellers improve their bids' competitiveness.
- **Bid Improvement**: is a dummy variable taking the value of one if the current bid is weakly more competitive than the prior bid and zero otherwise.

In Table 6 we ask whether there is evidence that subjects made their bids more competitive across rounds of bargaining. In the first two columns, the dependent variable is Bid Change and we use standard regressions. In the next two columns, the dependent variable is Bid Improvement and we use probit regressions. The results suggest that subjects indeed improved their bid in at least 63% of rounds, and that the average improvement size was 1.5 ECUs. Without controlling for differential change in behavior over periods for the two treatment conditions, we find that the cross-round improvement for subjects in the Simple and Complex treatments were similar. However, when we interact the period number with the treatment dummy, we find that initially subjects in the Simple treatment were much more prone to increase their bids' competitiveness than subjects in the Complex treatment. At the same time, there was some evidence that the competitiveness of the bids in the Complex treatment improved over time.

Another interesting finding was the difference in efficiency across periods with one and three rounds of bargaining. The improvement was significantly more pronounced in the Simple treatment, where average efficiency went from 73% in one-round periods to 84% in three-round periods – a full eleven percentage point increase. However, efficiency increased only marginally in the Complex treatment, rising from 72% in one-round periods to 73% in three-round periods.

Our findings suggest that subjects' strategies were substantially affected by complexity. This result is consistent with Schotter (1990), who finds that changes in prior distributions of private values altered subjects' bid strategies. However, he points out that while subjects' strategies are affected by changes in the environment, the overall efficiency does not. Interestingly, the average efficiency across a large set of experiments and conditions was around 80%, identical to the efficiency we observe in the Simple condition (81%). However, the efficiency in the Complex condition was substantially lower at 73%.

4.5 Complexity and Private Value Uncertainty

Complexity has the direct effect of making the information available to the subjects more noisy. That is, the subjects may fail to solve the complex problem or may reach an incorrect solution. Given this, one might assume that the change in efficiency and liquidity are driven mechanically by estimation errors. Specifically, it could be the case that an increase in the noise of the bidding function inputs may induce lower efficiency, even if the subjects' behavior was unchanged across treatments. Based on our analysis so far, this seems unlikely. First, as we have shown, when we condition on the *guesses* (not realizations) of private values, the bidding functions themselves change substantially across treatments. Second, demographic variation does not alter the tendency to make estimation errors, but does influence the effect that complexity has on bidding and trading behavior. Third, we can consider the behavior of a subset of subjects who estimated their private values correctly in both the simple and complex conditions and compare their bidding behavior across treatments. As Table 7 shows, these subjects, both in the role of buyers and sellers, shade their bids down and their asks up in the complex treatment compared to the simple treatment. This suggests that subjects' change in behavior in the complex environment is separate from uncertainty about values.

To explore this further and directly test whether the treatment results obtained from complexity are solely due to changes in private value uncertainty, we ran a new set of sessions in which subjects did not solve portfolio problems, but faced uncertainty regarding their private values. To link these added sessions to the original set, we measured the private value uncertainty induced by complexity for each of the twenty possible complex problems during the first experiment. For each set, we calculated the fraction of times that subjects guessed their private value correctly (that is, their guess was at most one unit away from the true value). We then used these errors to add noise about private values in a series of five independent sessions. In each period, instead of presenting security baskets, subjects received a clue about their private value. They were informed that with some probability the clue is correct and with the complementary probability the clue is simply noise. The probability provided to subjects was designed to mirror the private value uncertainty under the same problem in the complexity treatment. The data is described in Table 1, Panel B.

The new set of sessions created a setting in which complexity is absent, but where private value uncertainty varied while matching the levels obtained in the complex treatment. Panel C in Table 2 summarizes the results for the same set of dependent variables we studied in the complexity treatment. In the panel, the first row under each of the dependent variable corresponds to data collected from low uncertainty periods, while the second row under each dependent variable corresponds to data collected from high uncertainty periods. First, we find no statistically discernible difference in efficiency, liquidity, or payoff asymmetry. The only somewhat statistically significant difference is in the number of rounds used – it is somewhat higher under high levels of uncertainty. Interestingly, this is the one measure that is found to be the least affected by complexity (see Panel B for comparison). Second, the average levels of the dependent variables in the uncertainty condition are generally comparable to those found in the complexity treatment. Finally, studying the bidding strategies of subjects in the uncertainty treatment, similar to the analysis conducted for the complexity treatment, we find that subjects bid less aggressively when uncertainty is high (see Table 7). At the same time, it is interesting to note the patterns described in Table 5 are generally also observed in Table 7.

Are these findings surprising? Upon reflection, we don't think so. Simply increasing the noise in the information about the value of the security is unlikely to account for our findings regarding complexity. To see why, consider the extreme case where the problem is so difficult that neither the buyer nor the seller can solve it. When neither player believes she has successfully solved the problem and neither player believes that the other player has successfully solved the problem, the auction is equivalent to an auction in which private values are common knowledge. This game, in contrast to the game in which players are assumed to correctly learn their individual private value, will have a fully efficient equilibria. Since the equilibrium of the game without noise is not fully efficient, we see that at least for sufficiently high noise values the maximum efficiency that can be obtained in equilibrium is *increasing* in noise. This is not consistent with our results in the first experiment, where increased complexity led to less efficiency and less frequent trade. Based on this, we argue that it is not increased noise, but adverse selection that likely accounts for decreased trade and inefficiency. Specifically, complexity increases the probability that one opponent has an informational advantage. If people do worry about this possibility when they fail to solve a problem, then this could lead to the classic trade frictions induced by adverse selection. Exploring the theoretical underpinnings of this finding is the subject of future research.

4.6 Learning and Declining Effort

One possible concern with the external validity of our experimental results are the issues of learning during the experiment or tiring out. In short, one may worry that the results obtained in the laboratory reflect lack of experience with the task and that upon repetition, subjects would substantial alter their behavior. Alternatively, it might be possible that subjects might become apathetic or bored during the experiment, thereby making more errors as the fifteen periods elapsed.

These are valid concerns and we consider whether learning or tiring out during the course of the experiment was a significant factor. To that end, we analyzed the variation of the some key measures across the different stages of the experiment. Since the treatment condition varied with periods, we separated the data by condition. Table 9 presents the results. Looking at various error measures, subjects' payoffs and the frequency of transactions do not vary monotonically with period number during the experiment. The only measure that appeared to be significantly higher during the first set of periods (1-5) was the average guess error in the Complex treatment. Therefore, while subjects might learn or alternatively tire out during the experiment, this does not appear to be a first order concern.

5 Conclusions and Discussion

Complexity of financial markets has become a fact of life. Based on recent events in financial markets, it is now clear that financial models can fail and that market participants are not all-knowing. Previous crashes and crises have also verified this, but our profession appears to be more receptive to investigating the effects of complexity on markets at the present time.

In this paper, we study the effects of complexity on trading behavior in an experimental market setting. We show that complexity leads to lower liquidity, higher price volatility, and a loss in trade efficiency. Strikingly, this appears to be separate from the estimation errors made by the study participants. For example, considering subjects of varied demographic characteristics, estimation error did not vary among them, but trading behavior did. Moreover, in follow-up experiments, we showed that adding uncertainty to peoples' private values did not decrease their willingness to trade or economic efficiency.

What, then, explains why noise does not cause changes in trading behavior, but complexity does? We believe that a likely source of trade breakdown comes from the traders' estimations (or possibly misperceptions) of their own ability. Healy and Moore (2008) demonstrate theoretically and experimentally that agents tend to overestimate their relative ability when facing easy tasks and underestimate their relative ability when facing difficult tasks. They argue that, when faced with a hard task, agents are not able to determine if their difficulty in completing the task arises from the characteristics of the task or from their own lack of ability. A Bayesian agent facing a problem that is, in fact, more difficult than expected will lose confidence in his own ability relative to that of other agents.

To see why, consider an individual who is uncertain about the difficulty of a problem and is uncertain about his own ability. If he has a strong prior belief that the problem to be posed is only moderately difficult, then he will respond to his own failure to solve the problem by increasing the weight he places on his own ability being low. He may continue to overestimate the likelihood that he solved the problem based on his prior on the difficulty of the task and on his own ability, but he will underplace himself relative to other participants in the market. This logic applies to fully rational, Bayesian agents and will also apply to agents who have biased self-assessment, as long as they at least partially update beliefs according to Bayes law.

In our setting, a complex problem might lead traders to question their own ability relative to others. Since private values are affiliated,[7] buyers who cannot determine their own private value with confidence, might fear that trade will only occur when the seller, who is presumed to be better informed, has a lower private value. Affiliation then implies that the uninformed buyer is likely to lose by trading against the informed seller. Note that this will hold even when the hypothetical informed seller believes that he trades against a fully informed buyer. That is, a buyer who loses confidence in his own ability will be concerned about trading against a skilled seller, even if the skilled seller believes himself to be trading against a skilled buyer.

When the problem facing both traders is in fact very difficult, as holds for our complex treatment, both players may mistakenly think that they have below average skill at the task in question and trade defensively. If this is the case, the buyer will shade his bid down, and the seller will shade his asking price up. This produces something akin to two-sided adverse selection, and when the problem is sufficiently difficult relative to the expectations of the traders such concerns may override the potential gains to trade and lead to a breakdown in trade.

We view our work in this paper as a first step to understanding how complexity affects markets and affects policy considerations. Indeed, our results tend to support the policy implication that standardization of assets may improve welfare, making assets more liquid and less volatile. Admittedly, whether traders in real markets have the same response to complexity remains an open, but important question. Additionally, understanding the effect of complexity on behavioral biases warrants future investigation. Evaluation of this, as well as its theoretical underpinnings, is the subject of future research.

[7] Recall that affiliation means that a higher value for the buyer will generally be associated with a higher value for the seller, and vice versa.

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6 Figures and Tables

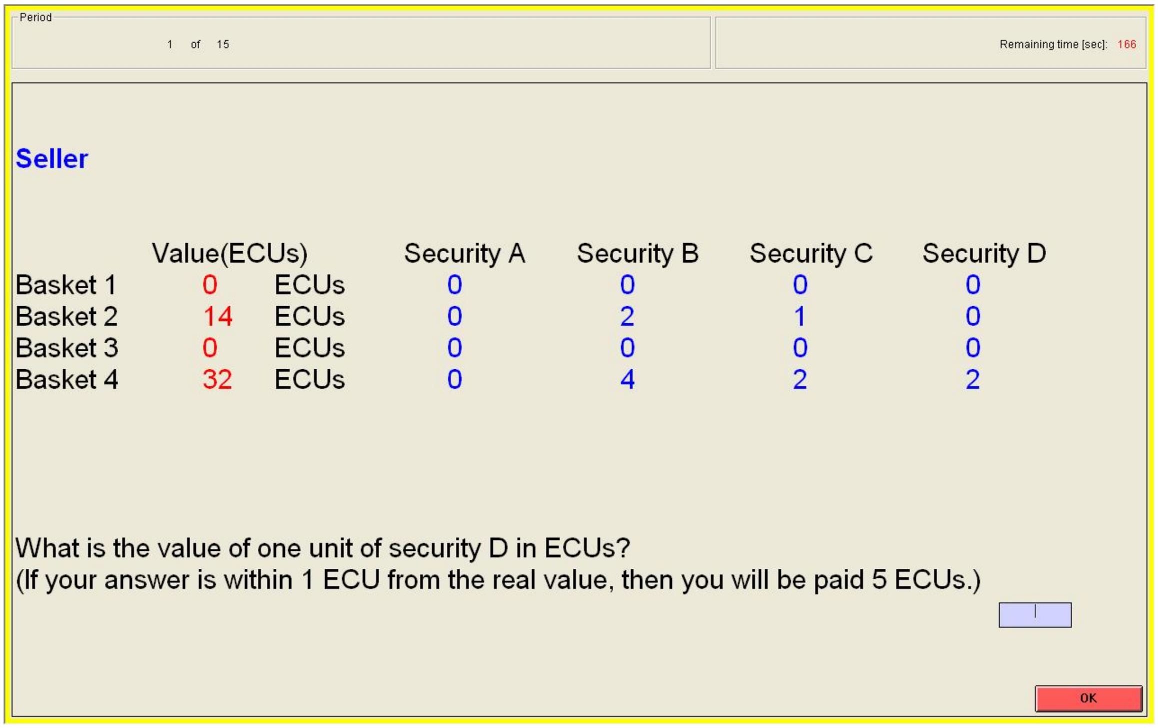


Figure 1: **Screenshot Example**

This is a screenshot from the interface used for the experiment. It provides an example of a decision problem used in the Simple condition.

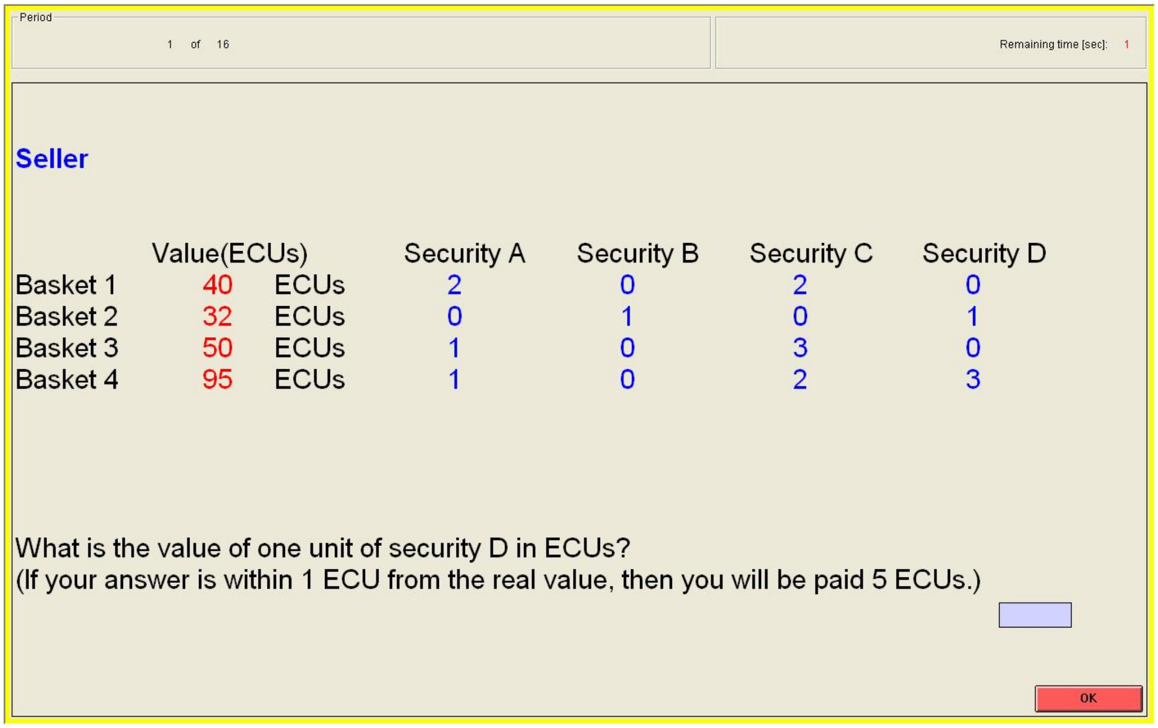


Figure 2: **Screenshot Example**

This is a screenshot from the interface used for the experiment. It provides an example of a decision problem used in the Complex condition.

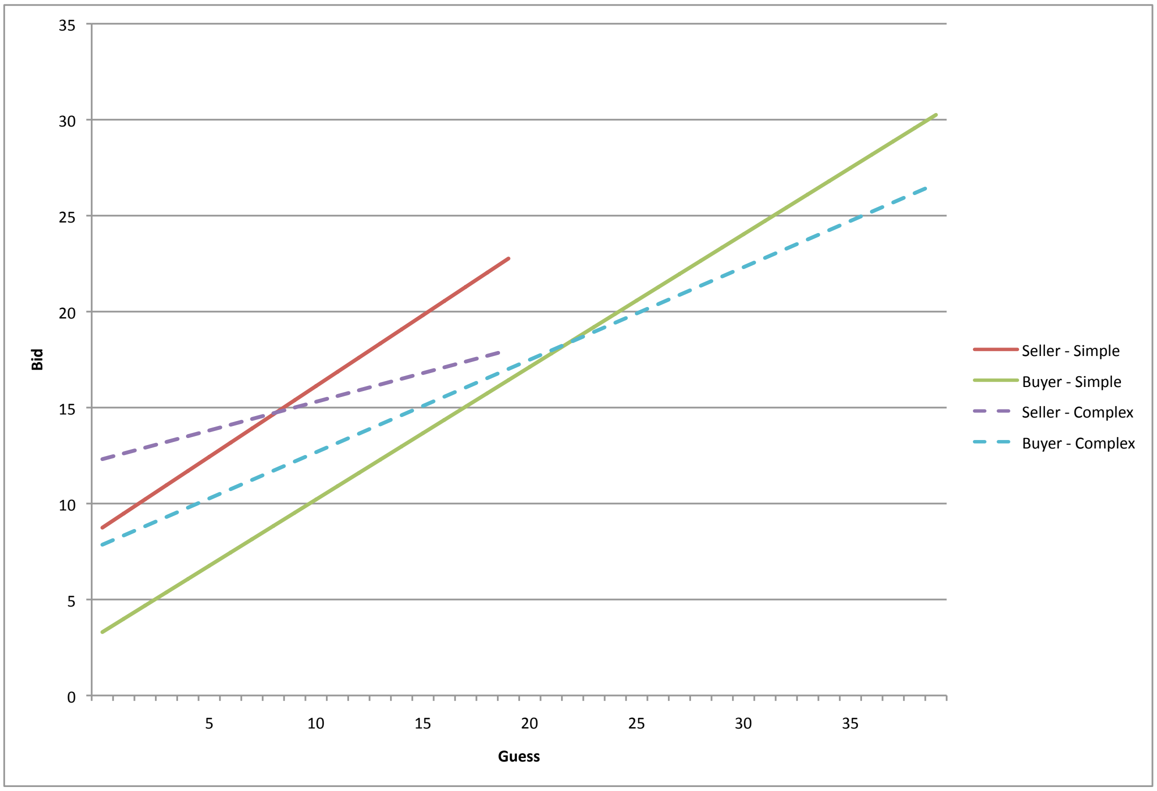


Figure 3: **Experiment 1: Estimated Bid Functions (one round periods)**

The figure depicts the estimated bid behavior as a function of private values. We plot the functions separately for buyers, sellers, Simple and Complex conditions, all for bidders participating in one-round bargaining games.

Table 1: **Experiments 1 and 2: Data Summary**

The table reports the data collected in the experiment, divided into sessions. It reports (in the order of the columns) the number of subjects per session, the periods in which the Simple condition was conducted, the periods in which the Complex condition was conducted, the total number of period observations, the total number of round observations, the average payoff (across subjects and periods), the number of period observations collected under the Simple condition, and the number of period observations collected under the Complex condition.

Panel A: Complexity Sessions

Session ID	N of subjects	Simple periods	Complex periods	N of Periods	N of Rounds	Ave Payoff	N of Simple periods	N of Complex periods
1	16	6-10	1-5, 11-15	115	166	3.93	40	75
2	8	11-15	1-10	60	87	3.87	20	40
3	8	1-5, 11-15	6-10	60	105	3.69	40	20
4	18	6-10	1-5, 11-15	133	190	3.83	45	88
5	20	1-5, 11-15	6-10	146	223	3.57	97	49
Total	70	NA	NA	514	771	3.78	242	272

Panel B: Uncertainty Sessions

Session ID	N of subjects	Low Unc. periods	High Unc. periods	N of Periods	N of Rounds	Ave Payoff	N of Low Unc. periods	N of High Unc. periods
11	12	1-5, 11-15	6-10	90	143	4.5	60	30
12	8	6-10	1-5, 11-15	59	67	3.36	19	40
13	10	1-10	11-15	75	105	3.67	50	25
14	10	1-5, 11-15	6-10	75	103	3.23	50	25
15	8	1-5, 11-15	6-10	60	78	3.1	40	20
Total	48	NA	NA	359	496	3.572	219	140

Table 2: **Experiments 1 and 2: Result Summary – Complexity and Uncertainty**
The table reports the average level of various measures across the two treatment conditions (Simple and Complex), and sessions. Panel A focuses on measures of complexity: the fraction of periods in which subjects provided an exact guess of their private value, the average guess error, the fraction of rounds in which buyers (sellers) submitted bids that were higher (lower) then their estimated private values. Panel B focuses on the main dependent variables: the average fraction of surplus captured by both subjects, the fraction of periods resulting in a transaction, the average payoff asymmetry (across the two subjects), the average bid gap (in rounds that did not result in a transaction), the average number of rounds used before a transaction took place, and the adjusted price volatility. The final column reports the p-values from a mean equality test across treatments, treating each session as an observation.

		Session Number						
		1	2	3	4	5	Total	t-test
		Panel A: Measures of Complexity						
Freq of exact guesses	Simple	0.69	0.88	0.84	0.67	0.68	0.72	0.0018
	Complex	0.25	0.35	0.57	0.38	0.16	0.31	
Average guess error	Simple	3.14	1.45	1.01	2.92	2.57	2.38	0.0016
	Complex	9.75	7.39	5.2	7.35	5.65	7.55	
Freq of bid error	Simple	0.31	0	0.1	0.17	0.1	0.14	0.0675
	Complex	0.3	0.19	0.12	0.2	0.2	0.22	
		Panel B: Complexity Sessions						
Average Efficiency	Simple	0.884	0.681	0.775	0.843	0.791	0.806	0.023
	Complex	0.801	0.675	0.71	0.726	0.671	0.725	
Freq of Transaction	Simple	0.85	0.55	0.62	0.71	0.66	0.69	0.106
	Complex	0.72	0.6	0.55	0.69	0.57	0.65	
Payoff Asymmetry	Simple	5.49	2.64	4.7	5.83	3.34	4.42	0.01
	Complex	7.13	5.42	4.95	6.57	4.84	6.21	
Bid Gap	Simple	3.45	3.35	3.48	4.27	3.54	3.62	0.0807
	Complex	5.18	3.81	4.94	4.51	3.15	4.33	
Number of Rounds used	Simple	1.62	1.14	1.67	1.44	1.39	1.47	0.4332
	Complex	1.52	1	1.33	1.72	1.25	1.49	
Price Volatility	Simple	7.21	3.79	6.79	10.23	4.69	6.98	0.0427
	Complex	9.71	8.94	7.72	10.72	7.89	9.68	
		Panel C: Uncertainty Sessions						
Average Efficiency	Low Unc	0.793	0.683	0.771	0.695	0.672	0.738	0.898
	High Unc	0.874	0.733	0.468	0.72	0.763	0.718	
Freq of Transaction	Low Unc	0.72	0.58	0.7	0.62	0.62	0.66	0.794
	High Unc	0.87	0.65	0.44	0.68	0.7	0.67	
Payoff Asymmetry	Low Unc	9.57	4.64	4.73	5.21	6.52	6.57	0.552
	High Unc	6.35	6.13	6.09	9.24	6.68	6.83	
Bid Gap	Low Unc	4.12	3.94	3.39	3.63	3.93	3.8	0.725
	High Unc	3.75	3.4	3	3.71	4.7	3.65	
Number of Rounds used	Low Unc	1.52	1	1.75	1.47	1.07	1.43	0.069
	High Unc	1.54	1.33	2.25	1.58	1.9	1.65	
Price Volatility	Low Unc	9.84	6.58	5.87	5.78	8.18	7.97	0.427
	High Unc	7.68	7.99	7.21	9.69	8.08	8.39	

Table 3: **Experiment 1: Subject-level Treatment Effect**

The table reports regression results of differences in the observed level of the dependent variables for each subject, across the two treatment conditions, on a constant and a number of subject characteristics. The dependent variables are the change in transaction frequency, the change in payoff asymmetry, the change in bid gap, the change in the number of rounds used to reach a transaction, the change in payoffs, and the change in price volatility. The independent variables are the school year of the subject, the overall GPA, gender (equals one for female), and major (equals one for economics). Each subject is treated as an observation.

Panel A	**Δ Transaction Frequency**					**Δ Payoff Asymmetry**				
School year		0.051					0.467			
		[0.033]					[0.533]			
GPA			-0.006					0.252		
			[0.032]					[0.666]		
Female				0.197					-2.428	
				[0.047]***					[1.431]*	
Econ Major					-0.009					1.345
					[0.053]					[0.932]
Constant	0.061	-0.083	0.083	0.036	0.067	-1.227	-2.553	-2.106	-0.915	-2.130
	[0.025]**	[0.094]	[0.109]	[0.026]	[0.043]	[0.442]***	[1.570]	[2.376]	[0.457]**	[0.763]***
Observations	70	70	70	70	70	70	70	70	70	70
R^2	0.000	0.047	0.000	0.104	0.000	0.000	0.012	0.001	0.049	0.030

Panel B	**Δ Bid Gap**					**Δ Rounds Used**				
School year		-0.068					0.080			
		[0.348]					[0.074]			
GPA			0.118					-0.083		
			[0.366]					[0.073]		
Female				-0.738					-0.106	
				[0.603]					[0.200]	
Econ Major					0.543					0.263
					[0.515]					[0.135]*
Constant	-0.629	-0.434	-1.038	-0.534	-0.993	0.022	-0.207	0.310	0.035	-0.155
	[0.245]**	[1.095]	[1.349]	[0.270]*	[0.415]**	[0.062]	[0.211]	[0.225]	[0.066]	[0.116]
Observations	70	70	70	70	70	70	70	70	70	70
R^2	0.000	0.001	0.001	0.015	0.016	0.000	0.019	0.008	0.005	0.058

Panel C	**Δ Payoffs**					**ΔPrice Volatility**				
School year		0.051					1.145			
		[0.542]					[0.876]			
GPA			-0.022					0.395		
			[0.785]					[1.030]		
Female				-2.249					-4.175	
				[1.066]**					[1.480]***	
Econ Major					2.161					2.684
					[0.842]**					[1.283]**
Constant	0.516	0.371	0.593	0.806	-0.935	-2.368	-5.624	-3.743	-1.832	-4.171
	[0.434]	[1.634]	[2.755]	[0.470]*	[0.647]	[0.663]***	[2.470]**	[3.739]	[0.716]**	[0.967]***
Observations	70	70	70	70	70	70	70	70	70	70
R^2	0.000	0.000	0.000	0.044	0.079	0.000	0.033	0.002	0.064	0.052

Table 4: **Experiment 1: Subject Demographics**

The table reports the average level of the dependent variables (across periods and rounds) for each subject on a number of demographic data. The dependent variables include the fraction of periods in which subjects provided an exact guess of their private value, the average guess error, the fraction of rounds in which buyers (sellers) submitted bids that were higher (lower) then their estimated private values, the fraction of periods resulting in a transaction, the average payoff asymmetry (across the two subjects), the average bid gap (in rounds that did not result in a transaction), the average number of rounds used before a transaction took place, and the average payoffs. The independent variables include the gender (equals one for a female), major (equals one for economics), school year, and GPA.

	Freq of exact guess	Estimate error	Bid error	Transaction frequency	Payoff asymmetry	Bid gap	Rounds used	Payoff
Gender	-0.138	2.685	0.048	0.062	0.406	0.253	0.179	0.229
	[0.135]	[2.656]	[0.087]	[0.053]	[0.768]	[0.635]	[0.199]	[1.129]
Econ	-0.027	0.519	-0.022	0.047	0.469	-0.365	-0.154	0.088
major	[0.082]	[1.036]	[0.060]	[0.051]	[0.697]	[0.378]	[0.125]	[0.723]
School	0.015	0.647	-0.019	0.009	-0.122	0.074	0.053	0.551
year	[0.034]	[0.685]	[0.023]	[0.018]	[0.321]	[0.185]	[0.047]	[0.310]*
GPA	0.025	-1.627	-0.045	-0.020	-0.094	-0.267	-0.115	0.165
	[0.095]	[2.044]	[0.064]	[0.031]	[0.414]	[0.310]	[0.094]	[0.503]
Constant	0.548	5.570	0.354	0.612	5.107	4.550	1.635	1.328
	[0.441]	[7.906]	[0.276]	[0.146]***	[2.365]**	[1.641]***	[0.482]***	[2.645]
Observations	70	70	70	70	70	68	70	70
R^2	0.029	0.054	0.033	0.040	0.006	0.045	0.126	0.023

Table 5: **Experiment 1: Bid Functions**

We regress bids on estimated private values (guesses) and a treatment dummy using robust regressions (controlling for period number). We estimate the model separately for buyers, sellers, one negotiation round periods, and three negotiation round periods.

	One Round Periods		**Three Round Periods**	
	Seller	*Buyer*	*Seller*	*Buyer*
Complex	3.948	4.767	5.927	4.842
	[1.738]**	[1.669]***	[1.333]***	[1.482]***
Guess	0.73	0.707	0.577	0.612
	[0.108]***	[0.060]***	[0.074]***	[0.048]***
Complex x	-0.432	-0.207	-0.463	-0.2
Guess	[0.121]***	[0.065]***	[0.089]***	[0.062]***
Period	0.097	-0.204	-0.042	-0.134
	[0.107]	[0.092]**	[0.091]	[0.085]
Constant	7.288	4.006	10.11	3.879
	[1.692]***	[1.586]**	[1.228]***	[1.332]***
Observations	223	222	285	284
R^2	0.273	0.701	0.196	0.5

Table 6: **Bid Change Across Rounds**

We regress the bid change across rounds of negotiation (columns 1 and 2) and a dummy for bid improvement (columns 3 and 4) on the treatment dummy, number of periods, and their interaction. Bid change equals to round t minus round $t-1$ bid for buyers and round $t-1$ minus round t bid for sellers. The Bid Improvement dummy equals one if the bid change is weakly positive, and zero otherwise. In columns 1 and 2 we use OLS regressions and in columns 3 and 4 we use probit regressions.

	Bid Change		**Bid Improvement**	
Complex	0.414	-1.671	-0.010	-0.775
	[0.373]	[0.742]**	[0.119]	[0.252]***
Period		-0.048		-0.029
		[0.044]		[0.019]
Complex x		0.288		0.106
Period		[0.080]***		[0.029]***
Constant	1.504	1.950	0.634	0.913
	[0.226]***	[0.503]***	[0.082]***	[0.196]***
Observations	514	514	514	514
R^2	0.002	0.034	0	0.0274

Table 7: **Experiment 1: Bid Functions for Subjects with Constant Noise**

We regress bids on estimated private values (guesses) and a treatment dummy using robust regressions. We estimate the model separately for buyers, sellers for one negotiation round periods. The data is collected for subjects whose private value estimation error was similar across treatments.

	Seller	**Buyer**
Complex	4.042	8.743
	[3.110]	[2.104]***
Guess	0.95	0.649
	[0.191]***	[0.071]***
Complex x	-0.413	-0.392
Guess	[0.232]*	[0.084]***
Constant	6.68	3.24
	[2.628]**	[1.781]*
Observations	124	138

Table 8: **Experiment 2: Bid Functions under Private Value Uncertainty**

We regress bids on private value signals and a treatment dummy using robust regressions (controlling for period number). We estimate the model separately for buyers, sellers for one negotiation round periods. The data is collected for subjects participating in the variant of the experiment in which there was no complexity but rather uncertainty about private values .

	One Round Periods		**Three Round Periods**	
	Seller	*Buyer*	*Seller*	*Buyer*
High Unc.	1.731	3.351	3.089	6.057
	[1.934]	[2.252]	[3.204]	[2.820]**
Signal	0.695	0.682	0.393	0.53
	[0.092]***	[0.069]***	[0.168]**	[0.076]***
High Unc. x	-0.264	-0.175	-0.21	-0.22
Signal	[0.152]*	[0.100]*	[0.268]	[0.118]*
Period	0.262	0.119	-0.134	-0.418
	[0.092]***	[0.107]	[0.207]	[0.171]**
Constant	6.741	3.101	13.873	8.873
	[1.336]***	[1.878]	[2.479]***	[1.994]***
Observations	193	193	164	164
R^2	0.304	0.446	0.04	0.286

Table 9: **Experiment 1: Learning**

The table reports the average level of a number of dependent variables across period blocks (1-5, 6-10, 11-15) and treatment conditions. The dependent variables include the fraction of periods in which subjects provided an exact guess of their private value, the average guess error, the average payoffs, and the fraction of periods resulting in a transaction.

	Periods 1-5	Periods 6-10	Periods 11-15	Total
	Simple condition			
Freq of exact guesses	0.72	0.68	0.76	0.72
Average guess error	2.85	3.02	1.42	2.38
Freq of bid error	3.77	4.51	3.64	3.98
Average payoffs	3.77	4.51	3.64	3.98
Freq of Transaction	0.67	0.78	0.61	0.69
	Complex condition			
Freq of exact guesses	0.32	0.29	0.33	0.31
Average guess error	9.48	5.58	7.27	7.55
Freq of bid error	0.25	0.17	0.24	0.22
Average payoffs	3.67	3.33	3.72	3.58
Freq of Transaction	0.66	0.56	0.75	0.65

Period

1 of 15

General Information

Thanks for participating in today's experiment. This is an experiment in decision-making. This study has been reviewed by the University of Texas' Review Board and been given an exempt approval.
I will read through a script to explain to you the nature of today's experiment as well as to navigate the computer interface, from which you will be working. I will be using this script to make sure that all sessions of the experiment receive the same information.

You will be paid for your participation in today's experiment. In addition to a $5 participation fee, you will be paid any earnings you accumulate from a task that will be described to you in a moment. You will be paid privately, in cash, at the conclusion of the experiment. **The exact amount you receive will be determined during the experiment and will depend on your decisions.** All the earnings during the experiment are denominated in Experimental Currency Units (ECU). At the end of the experiment, your earnings in ECU will be converted into US dollars at the ratio of $1 per 10 ECU.

OK

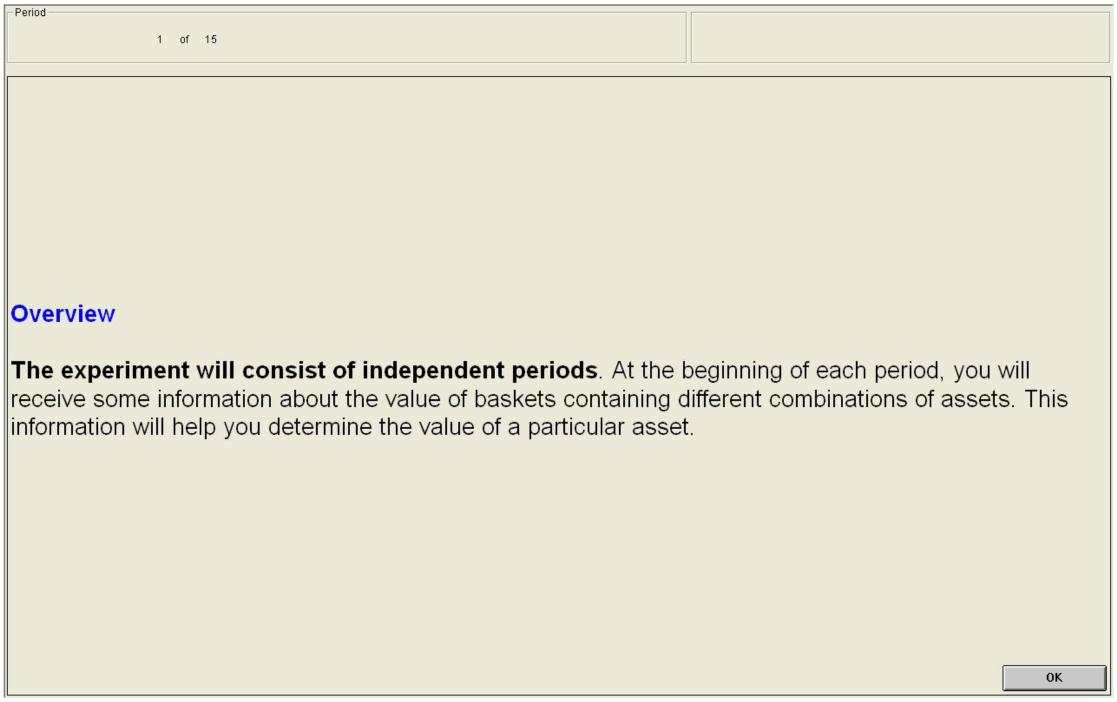
Period

1 of 15

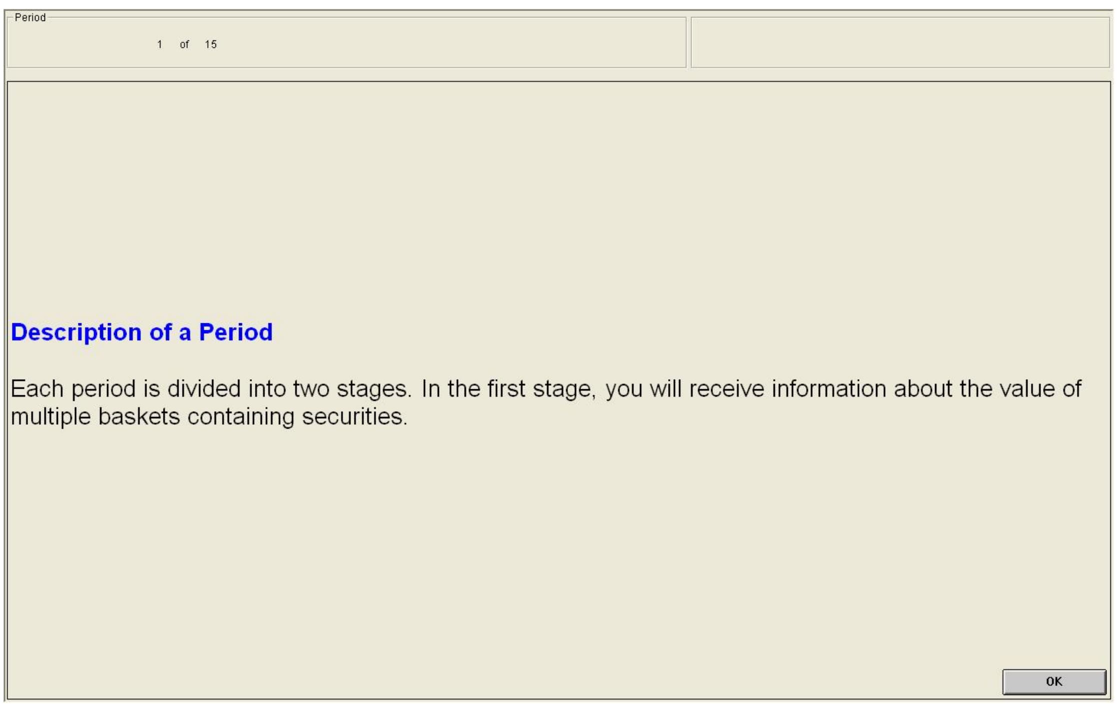
If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

In today's experiment you will be randomly assigned an ID. Your screen will display your participant ID. This participant ID should not be shared with anyone and it will not be revealed to other participants at any point.

OK



Period
1 of 15
Overview
The experiment will consist of independent periods. At the beginning of each period, you will receive some information about the value of baskets containing different combinations of assets. This information will help you determine the value of a particular asset.
OK



Period
1 of 15
Description of a Period
Each period is divided into two stages. In the first stage, you will receive information about the value of multiple baskets containing securities.
OK

Period

1 of 15

Stage 1

There are four different securities, labeled Security A, Security B, Security C, and Security D. The information you receive will indicate the value of different baskets, labeled Basket 1, Basket 2, etc. Each basket contains some units of securities and has a value associated with it.

For example, Basket 1, valued at 10 ECUs, contains 2 units of security A. Basket 2, valued at 20 ECUs, contains 1 unit of security A and 1 unit of security D. The information will be presented in a form of a table. Using the example above, you will be presented with the following table:

	Value	Security A	Security B	Security C	Security D
Basket 1	10 ECUs	2	0	0	0
Basket 2	20 ECUs	1	0	0	1

Your goal is to use this information in order to assess the value of security D.
You will get 3 minutes to review the information and be asked about the value of 1 unit of security D. **If your answer is within 1 ECU from the real value of the security D, then you will get 5 ECUs.** After that, you will move to the second stage of the period.

OK

Period

1 of 15

Stage 2

At this stage, you will be **paired with another anonymous subject** in the session and you will have the opportunity to trade a unit of security D with that subject. You will be **randomly assigned a role of a buyer or a seller** and the subject you are paired with will be assigned the complementary role.

In a succession of bidding rounds, **you will be asked to submit a price at which you are willing to buy/sell**. Neither the buyer nor the seller observe the other subject's bid. You will have 30 seconds to submit a bid, in between bidding periods. If you do not submit a bid within the said time period, the bidding round will be eliminated and no trade would occur. **The number of bidding rounds will be random:** before submitting your first bid, you will be notified whether the number of bidding rounds is one or three.

If you are a buyer and the price you bid is higher than the price the seller bid in that round, a transaction will occur at the mid point between the buyer's and seller's bids. Otherwise, no transaction occurs. For example, if you are the buyer and you submitted a bid of 20 ECUs and the seller submitted a bid of 10 ECUs, you will buy Security D from the seller for 15 ECU [(10+20)/2]. Likewise, **if you are a seller and the price you bid is lower than the price the buyer bid in that round, a transaction will occur at the mid point between the buyer's and seller's bids.** Otherwise, no transaction occurs.

OK

Period

1 of 15

In a given period, **both the buyer and the seller receive the same information about the baskets.** However, **the values of all the baskets are uniformly higher for the buyer compared with the seller.** The difference between the basket values for the buyer and the seller is determined randomly, ranging from 0 to 20.

For example, the table presented to the **buyer** may look like:

	Value	Security A	Security B	Security C	Security D
Basket 1	20 ECUs	2	0	0	0
Basket 2	30 ECUs	1	0	0	1

While the table presented to the **seller** may look like:

	Value	Security A	Security B	Security C	Security D
Basket 1	5 ECUs	2	0	0	0
Basket 2	15 ECUs	1	0	0	1

Notice that only the column labeled "Value" is different across the two tables.

OK

Period

1 of 15

Payoffs from trading

If a transaction occurs, the buyer receives the difference between the purchase price and the value of Security D as determined by the values of the baskets. For example, if the buyer purchased Security D from the seller for 15 ECUs, and the value of the security is 20 ECUs, the payoff to the buyer are 5 ECUs.

Likewise, if a transaction occurs, the seller receives the difference between the sell price and the value of Security D as determined by the values of the baskets. For example, if the seller sold Security D to the buyer for 15 ECUs, and the value of the security is 12.5 ECUs, the payoff to the seller are 2.5 ECUs.

If no transaction occurs, the payoffs to both the buyer and the seller from trade are zero.

At the end of the experiment, your payoffs will equal to the sum of your payoffs across the 15 periods of stage1 and stage2 including show-up fees.

OK