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Abstract

In modern firms the use of contests as an incentive device is ubiquitous. Nonetheless, experimental research shows that in the laboratory subjects routinely make suboptimal decisions in contests even to the extent of making negative returns. The purpose of this study is to investigate how earning the endowment, demographic differences and individual preferences impact behavior in contests. To this end, we conduct a laboratory experiment in which subjects expend costly resources (bids) to attain an award (prize). In line with other laboratory studies of contests, our results show that subjects overbid relative to theoretical predictions and incur substantial losses as a result. Making subjects earn their initial resource endowments mitigates the amount of overbidding and thus increases efficiency. Overbidding is linked to gender, with women bidding higher than men and having lower average earnings. Other demographic information, such as religiosity, and individual preferences, such as preferences towards winning and risk, also influence behavior in contests.

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Keywords: contest, experiments, overbidding, endowment, gender, religiosity

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1. Introduction

The use of contests as an incentive device has garnered much attention by researchers. Certainly, competition as an incentive device often has advantages over other non-competitive incentive schemes (Lazear and Rosen, 1981; Nalebuff and Stiglitz, 1983). Even so, these advantages may be eliminated if the agents in these situations make systematically inefficient choices. Since the original studies of Bull et al. (1987) and Millner and Pratt (1989), a number of laboratory studies have shown that subjects make significantly higher bids in contests than predicted; for a comprehensive review see Dechenaux et al. (2015). In some instances, the magnitude of overbidding is so high that subjects make negative expected payoffs.

Contests have been used to investigate effort choices of workers (Lazear and Rosen 1981) and the allocation of resources to achieve a goal such as research and development funding (Harris and Vickers, 1985, 1987). To motivate the present study we focus on the allocation of resources but our results could also be applied to the context of effort choices and, as such, add to a quickly growing literature about the impact of competition and choices (Niederle and Vesterlund, 2007; Dohmen and Falk, 2011). We find the allocation of resources question to be of particular importance because the fact that agents in these instances make decisions which generate negative payoffs is of paramount concern to the organizations which may employ contests as an incentive device. In turn, this inefficient use of resources constitutes a problem of moral hazard as managers of organizations must determine the best way to endow subordinates with resources used to perform productive tasks (e.g., research and development). If the way that managers endow subordinates with resources is a causal factor in the misuse of resources then it would be prudent for the manager to provide resources in a circumspect way.

As such, the results of this research should be of interest to both management researchers and management practitioners alike.

In this study, we examine several variables which may have an impact on the use of costly resources in contests. These are endowment differences, demographic differences and differences of individual preferences. To investigate these variables we design a laboratory experiment which allows us to systematically vary the origin of the endowment between a windfall endowment and an earned endowment before subjects participate in a lottery contest. We also capture demographic data on decision makers, such as gender, religiosity, major, economics classes and age, as well as individual preferences, such preferences towards winning and risk.

Our experiment results indicate that when subjects earn their endowments bidding decreases by 11-16% compared to the windfall endowments. Demographic characteristics, such as gender and religiosity, and individual preferences, such as preferences towards winning and risk, are significant predictors of subjects' bidding behavior in contests. We find that subjects who indicate higher utility for winning or higher tolerance for risk make higher bids in contests. Surprisingly, demographic effects are even stronger than treatment effects, with women making 25% higher bids and more religious subjects making 26% lower bids. Furthermore, when including these demographic variables in our analysis, we find that a large portion of the treatment effects are subsumed by the demographic effects.

This study adds to the understanding of two phenomena in the literature: (1) overbidding relative to the standard Nash equilibrium prediction and (2) heterogeneous behavior of contestants (Sheremeta, 2013; Dechenaux et al., 2015). We find that significant portion of overbidding can be explained by the fact that subjects receive windfall endowments (house money) before participating in contests. Additionally, we find that demographic differences, such as gender and religiosity, as well as heterogeneous preferences, such as preferences towards winning and risk, have a significant impact on bidding and thus can explain heterogeneous behavior of contestants. Also, our paper contributes to a large literature on gender differences (Croson and Gneezy, 2009) and the growing literature on religion and economic behavior (Hoffmann, 2013).

The rest of the paper is organized as follows: Section 2 provides a brief literature review. Section 3 details the experimental design and procedures. Section 4 reports the results of the experiment. Lastly, Section 5 concludes and suggests directions for future research.

2. Related Literature

The literature on contests has generally fallen into one of three categories: selection into contests, performance in contests, and efficiency of contests. Our study here focuses mostly on the last category which has received significantly less attention in the recent surge of research on contests (see Dechenaux et al., 2015).

A number of studies have focused on how people self-select into contests depending on individual preferences and demographic characteristics. This strain of literature has mostly been driven by the findings of gender differences in the decision to enter into competitive situations (Niederle and Vesterlund, 2007). In particular, this line of literature has found that when given the choice, women more than men tend to select out of competitive compensation schemes and into schemes which reward individual productive behavior (e.g., piece-rate).[1] These findings are important because they suggest an explanation for why so few women are represented in high

[1] The lone exception is the study by Price (2010) where the author fails to replicate the findings of Niederle and Vesterlund (2007) using the same experimental design.

paying competitive careers.[2] In addition to gender, Dohmen and Falk (2011) find that when subjects have a choice between a fixed payment and a contest, they are more likely to enter the contest if they are less risk-averse, more productive and more optimistic. Bartling et al. (2009) and Balafoutas et al. (2012) further document that, controlling for beliefs, inequality averse and spiteful subjects are less likely to enter contests. In summary, the findings of the literature indicate that people self-select into contests depending on individual preferences and demographic characteristics.

Studies examining performance in contests are mostly based on three canonical models: a lottery contest of Tullock (1980), a rank-order tournament of Lazear and Rosen (1981) and an all-pay auction of Hillman and Riley (1989). The common finding from studies on lottery contests and all-pay auctions is that subjects routinely overbid (equivalent to over-exerting effort) relative to theoretical predictions.[3] Sheremeta (2013) reviews 30 contest experiments and finds overbidding in 28 of those experiments, with the median overbidding rate of 72%. In rank-order tournaments overbidding is not as severe (Schotter and Weigelt, 1992; Orrison et al., 2004; Harbring and Irlenbusch, 2011), but it is still present in some studies (Chen et al., 2011; Cason et al., 2013).[4]

As noted earlier, this study is more closely related to the literature on efficiency of contests. Recently, there have been several attempts to reduce overbidding in contests and thus to enhance efficiency. Lugovskyy et al. (2010) allow subjects to have an extensive learning experience that results in added efficiency. Group decision-making has also been investigated as

[2] For a thorough review of the gender and competition literature, see Niederle and Vesterlund (2011).
[3] Some studies that document significant overbidding in contests are done by Davis and Reilly (1998), Potters et al. (1998), Sheremeta (2010, 2011), Sheremeta and Zhang (2010), Price and Sheremeta (2011), Cason et al. (2012), Mago et al. (2013, 2015), Savikhin and Sheremeta (2013), and Chowdhury et al. (2014).
[4] A possible explanation why the magnitude of overbidding is not as severe in rank-order tournaments is that in these tournaments subjects effort is distorted by a random noise and efforts have a convex cost structure; see Chowdhury et al. (2014) for a discussion.

a means for enhancing efficiency (Sheremeta and Zhang, 2010). However, even extensive learning and group decision-making do not completely eliminate the overbidding phenomenon. Finally, Sheremeta (2011) shows that constraining individual budgets (and thus constraining the strategy space) can reduce overbidding in contests, but such a mechanism is very unlikely to be effective in the world of competitive capital markets where it is relatively easy to borrow money (D'Avolio, 2002).

Our study examines whether overbidding can be reduced and efficiency enhanced when subjects earn their initial resource endowment. The idea that costly decisions may be influenced by the origin and, in particular, the effort by which the endowment is received is attributed to Locke (1978). The idea is clear: subjects who have to work or earn money to make decisions in the experiment may choose to make different decisions than subjects who receive money for free. The experimental evidence suggests that this is indeed the case. In dictator games, researchers have found that earning the endowment decreases subject's contributions (Cherry et al., 2002; Oxoby and Spraggon, 2006). Earning the endowment has also been shown to have an effect on the risk taking behavior (Thaler and Johnson, 1990) and behavior of subjects in the second-price auction (Jacquemet et al., 2009). In public good games, Muehlbacher and Kirchler (2009) and Harrison (2007) document that subjects who earn (or use their own) money are less likely to contribute to the public good.[5] In summary, it appears that most of these studies document that subjects who earn their endowments behave more in line with standard Nash equilibrium predictions.

[5] In contrast, some researchers do not find any significant effect of the endowment origin on subjects' behavior in public goods and redistribution games (e.g. Clark, 1998, 2002; Rutström and Williams, 2000; Cherry et al., 2005).

3. Experimental Environment, Design and Procedures

The experiment is based on the seminal rent-seeking lottery contest of Tullock (1980). We chose this model for several reasons. First, as we already mentioned, the lottery contest of Tullock (1980) is the seminal model of rent-seeking and prior research shows that subjects systematically overbid in these contests resulting in compromised efficiency. Second, to the extent that an agent's overbidding is caused by ancillary concerns such as the thrill of winning, demographic effects, or some form of conspicuous consumption the agent's actions constitute a moral hazard problem for the principle. Whereas the principle's interests lie in ensuring that costly resources are used efficiently (i.e., to ensure expected profit maximization), this environment is of concern to managerial technicians which we find particularly attractive. Finally, this particular model provides clear theoretical predictions for which to measure efficiency, while the structure of the contest is easy to understand for subjects in the laboratory.

In a simple lottery contest, there are n risk-neutral players who compete for a prize value of v. Each player i makes an irreversible bid b_i in order to increase the probability of winning the prize, which is modeled with the lottery contest success function $p_i = b_i/\sum b_j$. The expected payoff for player i is equal to the probability of player i winning, p_i, times the prize valuation, v, minus bid, b_i, i.e. $E(\pi_i) = p_i v - b_i$. The symmetric Nash equilibrium bid is $b^* = v(n\text{-}1)/n^2$ and the equilibrium expected payoff is $E(\pi^*) = v/n^2$. It is important to emphasize that there are no asymmetric equilibria (Chowdhury and Sheremeta, 2011) and the symmetric equilibrium is unique (Szidarovszky and Okuguchi, 1997).

In each treatment of our experiment, there are n=4 players competing with each other for the prize of v = 120 experimental francs. Therefore, the equilibrium bid is b^* = 22.5 and the expected payoff is $E(\pi^*)$ = 7.5. A key feature of our experiment is that there is no theoretical reason why bidding would be different based upon how the subjects receive their endowments. Therefore, the equilibrium bid is constant across our treatments since it does not depend on how the subjects are endowed (see Table 1).

We define three treatments based on how the subjects are endowed with resources in which to make bids. In the baseline Gift treatment, subjects received a free endowment as a show up fee of $20 to play the lottery contest for 30 periods, 5 of which were randomly selected for payment at the end of the experiment. In the Earn treatment, subjects earned their endowments through a real effort task. Specifically, the subjects are given the opportunity to add up sets of five randomly generated two-digit numbers by hand, as quickly as possible.[6] In the Earn treatment, subjects received $0.85 per problem that they correctly answered during a timed ten-minute period. This piece-rate was chosen so that on average the subjects would attain a similar endowment to that of the other two treatments. Finally, in the Yardstick treatment, subjects earned their endowments through the same real effort task, however, this time subjects received a $20 if they correctly solved more problems than a predetermined amount in a timed ten-minute period.[7] This predetermined hurdle was set at 2 problems but the subjects were not made aware of this fact. This extremely low hurdle was chosen so that all subjects would earn the $20 endowment.

The experiment involved 216 undergraduate subjects from Purdue University. The computerized experimental sessions were conducted in the Vernon Smith Experimental Economics Laboratory using z-Tree (Fischbacher, 2007). We ran 6 sessions of each of the three

[6] This task is commonly used in the experimental literature because it is easy to explain, and there is substantial variability in individual performance that is due partly to skill and partly to effort (Niederle and Vesterlund, 2007; Cason et al., 2010).

[7] The Yardstick treatment was meant as a robustness check of the impact of earning an endowment on the propensity to bid. As in the Earn treatment, *ex-ante* subjects were tasked with finding the correct sums, but *ex-post* they were paid the same amount as in the Gift treatment. The two treatments were used as both a consistency check and as a way of ensuring that the variance of the endowment remains low so as not to convolute the results. As we will see, the Earn and Yardstick treatments produce similar behavior.

treatments. In each session, there were a total of 12 subjects and the session proceeded in five parts (or four parts in the Gift treatment). Instructions, available in the Appendix, were given to subjects at the beginning of each part and the experimenter read the instructions aloud.

In the first part of the experiment, subjects made 15 choices in simple lotteries, similar to Holt and Laury (2002).[8] This method was used to elicit subjects' risk preferences. In the second part, subjects in Earn and Yardstick treatments earned money through adding up sets of five randomly generated two-digit numbers by hand. In the Gift treatment all subjects automatically were given the $20 endowment. In the third part, after learning how much money they have received, subjects participated in a total of 30 periods of the lottery contest. At the beginning of each period, subjects were randomly re-grouped to form a 4-player group. Subjects were then allowed to make bids between 0 and 120 for a prize of 120 francs. After all subjects submitted their bids, the computer chose the winner by implementing a simple lottery rule: the chance of receiving the prize was calculated as the number of francs a subject bids divided by the total number of francs all 4 subjects in the group bid. In the fourth part of the experiment, similar to Sheremeta (2010), subjects were asked to bid for a prize with a value of zero francs. Subjects were told that they would be informed whether they won the contest or not and that all subjects would have to pay their bids. This procedure was used to measure how important it is for subjects to win when winning is costly and there is no monetary reward for winning. Lastly, in the fifth part of the experiment, subjects were asked to fill out a short demographic questionnaire.

[8] Subjects were asked to state whether they preferred safe option A or risky option B. Option A yielded $1 payoff with certainty, while option B yielded a payoff of either $3 or $0. The probability of receiving $3 or $0 varied across all 15 lotteries. The first lottery offered a 0% chance of winning $3 and a 100% chance of winning $0, while the last lottery offered a 70% chance of winning $3 and a 30% chance of winning $0.

At the conclusion of the experiment, 1 of the 15 lottery choices subjects made in part one was randomly selected for payment. Subjects were also paid for 5 of the 30 periods in the lottery contest and for the 1 decision they made for a prize of zero francs. The five rounds of the lottery contest were selected randomly by picking five numbers out of a bingo cage. The earnings were converted into US dollars at the rate of 60 francs to $1. Average earnings were $20.37 per subject and the experiment lasted for about 60 minutes.

4. Results

4.1. Overbidding and Heterogeneous Behavior

Table 1 summarizes average bids and payoffs. Overall, subjects in all treatments significantly overbid relative to the Nash equilibrium prediction (all p-values < 0.01).[9] As a result of significant overbidding, average payoffs are negative. The persistence of overbidding is also shown in Figure 1, displaying the average bid over 30 periods of the experiment. Although there is a declining trend (suggesting learning), even in the last periods of the experiment subjects continue to substantially overbid relative to the Nash equilibrium prediction.[10] Such significant overbidding is consistent with previous findings of lottery contest experiments (Sheremeta, 2013; Dechenaux et al., 2015).

It is also important to emphasize that individual bids appear completely inconsistent with play of a unique symmetric pure-strategy equilibrium. Figure 2 displays the distribution of the average bid by subject in each treatment. Subjects are sorted in increasing order by the average

[9] We ran a random effects model on a constant with clustered standard errors at the session level for each treatment. The constant coefficients for each treatment are higher than the predicted theoretical values as in Table 1 (all p-values < 0.01).

[10] Based on the estimation of a random effects model where the dependent variable is a bid and the independent variables are a constant and a period trend, we find that the period trend is significant in two out of the three treatments (p-value < 0.01 for Yardstick and Earnings; p-value = 0.12 for Gift).

bid, which is indicated by solid line. The error bars represent standard deviations of bids for each subject over all 30 periods. Therefore, by focusing on the solid line one can see the cumulative empirical distribution of the average bid across subjects (signifying between-subjects variation), while by focusing on the error bars one can get a sense of the degree of within-subject variation. Examining Figure 2, one can clearly conclude that there is a very high degree of both between-subjects and within-subject variation, which is consistent with previous findings of contest experiments (Sheremeta, 2013; Dechenaux et al., 2015).

Since the subjects play the contest game for 30 periods it is interesting to investigate how the subject's choices vary as the experiment periods progress. Table 2 displays, by treatment, the measures of between-subjects and within-subject variation for the first half and the second half of the experiment. The between-subjects variation measure is calculated as the absolute difference between the individual bid in period *t* and the average group bid in period *t*, averaged over all periods of the experiment. The within-subject variation measure is calculated as the absolute difference between the individual bid in period *t* and period *t*-1, averaged over all periods of the experiment. Both measures are very similar across treatments. Also, the between-subjects variation is similar in the first half and the second half of the experiment, suggesting that there is a persistent variation of bidding behavior between subjects. On the other hand, the within-subject variation is lower in periods 16-30 than in periods 1-15, suggesting that learning takes place. This kind of within-subject learning has also been documented by Davis and Reilly (1998), Sheremeta and Zhang (2010), Chowdhury et al. (2014), and Mago et al. (2015).[11] Although in the rest of this paper we mainly focus on the overbidding phenomenon and between-subjects heterogeneity, in our analyses, we also control for learning.

[11] For the interested readers, Parco et al. (2005), Amaldoss and Rapoport (2009), and Sheremeta (2011) examine within-subject learning patterns in details.

4.2. Earned Endowment Effects

The focus of this study is to determine if the origin of endowment influences the degree of overbidding in contests. Table 1 shows that subjects bid 16% less in the Yardstick treatment relative to the Gift treatment (36.4 versus 43.2). Similarly, subjects bid 11% less in the Earn treatment relative to the Gift treatment (38.6 versus 43.2). It is also clear from Figure 1 that the average bids in the Gift treatment are higher than the average bids in the Yardstick and Earn treatments over most periods of the experiment. Nonetheless, as pointed out in Harrison (2007), we must be careful in looking at only average bids within our treatments in a repeated experiment such as ours.

To formally test the differences in bids across treatments, we use a random effects model with standard errors clustered at session level, where the dependent variable is the bid and the independent variables are treatment dummy-variables and a period trend.[12] The results of the estimation are reported in Table 3. Specification (1) shows results for a comparison of the Gift treatment to the pooled data of the Earn and Yardstick treatments.[13] Subjects in the Gift treatment bid significantly higher than in the two treatments where they had to earn the endowment. This difference between the Gift and Yardstick treatment in specification (2) is significant. The difference between the Gift and Earn treatment in specification (3) fails to be

[12] The properties of clustered errors are not well known when the numbers of clusters are few (see Cameron et al. 2008). As a robustness check of our analysis we also considered additional analysis where we clustered errors at the subject level. The results are similar to those in Table 3 and are available from the authors upon request.

[13] For a robustness check, we also tried to control for potential wealth effects by including correct problem counts. The number of correct problem counts in the Yardstick and Earn treatments is not correlated with bids. Moreover, the estimation results on all other coefficients are virtually the same and are available from the authors upon request.

significant at a traditional 5 percent level (p-value = 0.11).[14] Lastly, specification (4) indicates that bidding behavior across the Earn and Yardstick treatments is similar.

Given that there is substantial learning, as a robustness check, we have re-estimated Table 3, including additional independent lag variables such as wining in period *t*-1(*win-lag*), the subject's own bid in period *t*-1(*bid-lag*), and the sum of other group member's bids in period *t*-1 (*otherbid-lag*). The results are reported in Table 4. We find that in all four specifications a subject's *bid* in period *t* is significantly positively correlated with *bid-lag*. Only in specification (4), *bid* is correlated with *win-lag* and *otherbid-lag*. Most importantly, even after controlling for lag variables designed to capture learning, we still find significant treatment effects.

The significantly lower bids in the Earn and Yardstick treatments translate into relatively higher payoffs. Specifically, in the Gift treatment subjects on average lose 13.2. These loses are reduced in half when subjects earn their endowments (lose 6.4 in the Yardstick treatment and 8.6 in the Earn treatment). To test whether these differences are significant we estimated similar models as in Table 3, with *payoff* (instead of *bid*) as a dependent variable. The results, available for authors upon request, indicate that the average payoff in the Gift treatment is significantly lower than the average payoff in the Earn and Yardstick treatments (pooled data) and the average payoff in the Yardstick treatment. This difference also remains significant in the second half of the experiment.

4.3. Determinants of Bidding Behavior

In each experimental section, we elicited information about individual preferences for winning (Sheremeta, 2010) and risk (Holt and Laury, 2002). Also, at the end of the experiment

[14] Restricting our analysis to only the last 15 rounds strengthens our results. In particular, all p-values of treatment effects are significant with p-values < 0.08.

we conducted a demographic questionnaire. The survey was composed of questions regarding gender, religiosity, major, economics classes and age. Table 5 provides summary statistics of the information that we collected from subjects. Accounting for individual preferences and demographic differences, we find a number of interesting results. The estimation results of different random effect models, where the dependent variable is the bid and the independent variable are different individual characteristics, are reported in Table 6.[15] Specifications (1)-(3) use random effects models, with the individual subject as the random effect, to account for the multiple decisions made by individual subjects. We also cluster the standard errors at the session level, as well as use controls for treatment effects (treatment dummy-variables) and learning (period trend). Specification (4) uses a random effects probit model.

The estimation of specification (1) in Table 6 indicates a significant and positive correlation between the *bid* and the *winning* variable. The *winning* variable is measured by the bid for the prize value of 0 (the task that we presented subjects with at the end of thirty periods of bidding for the prize value of 120).[16] The significant positive correlation between the *bid* and the *winning* variable suggests that subjects who value winning more make higher bids in lottery contests. This is consistent with previous findings of Sheremeta (2010) and Price and Sheremeta (2011). One may argue that the *winning* variable is capturing confusion instead of a utility of winning. The problem with such an argument is that subjects participated in the contest with prize of 0 after they played other contests for 30 periods. Moreover, in estimation of specification (1) we use the *quiz* variable, measuring the number of correct quiz answers, to

[15] Due to the mass of bids at the upper and lower bounds we also considered a tobit regression as a robustness check. The results, including interaction effects in Table 7, were qualitatively similar and are available from the authors upon request.

[16] Although subjects were explicitly told that they would have to pay their bids, we still find that 28% of all subjects made positive bids, with an average bid of 9.1 in the Gift treatment, 6.1 in the Yardstick treatment, and 8.3 in the Earn treatment. Moreover, there are no statistically significant differences in bidding between three treatments (all p-values > 0.40).

control for confusion.[17] Although we find that subjects who understand the instructions better make lower bids in contests, the significant *winning* variable suggests that winning is a component in a subject's utility.

Another strong predictor of subjects' behavior in contests is risk preferences. The estimation of specification (2) in Table 6 indicates a significant and negative correlation between the *bid* and the *safe* variable. The *safe* variable is measured by the number of safe options that subjects chose in the Holt and Laury (2002) risk elicitation task (for the details see footnote 8). A higher number corresponds to a higher level of risk-aversion. The significant negative *safe* coefficient indicates that more risk-averse subjects make lower bids in contests. This finding is consistent both with theoretical predictions of Hillman and Katz (1984) and experimental findings of Sheremeta (2011) and Shupp et al. (2013).

In addition to *winning* and *safe*, in specification (3) of Table 6 we include different demographic characteristics summarized in Table 5.[18] The *gender* variable is an indicator variable, taking a value of 1 for women and 0 for men. The positive and significant *gender* coefficient implies that women bid more than men. This difference is substantial in magnitude and it is persistent throughout the duration of the experiment (see Figure 3). Remarkably, the gender effect is even bigger than the treatment effects (compare Figure 1 and Figure 3), with women making 25% higher bids than men (45.5 versus 36.3). This difference also holds across the treatments, with women bidding more than men in all three treatments.[19] This gender difference is even more surprising given that we control for other demographic characteristics as

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[17] This is a measure of how well subjects understand the instructions. Before the actual experiment, subjects completed the quiz on the computer to verify their understanding of the instructions. If a subject's answer was incorrect, the computer provided the correct answer. The experiment started only after all participants had answered all quiz questions.

[18] In 3 out of 18 sessions, we did not conduct the demographic questionnaire.

[19] Results are available upon request.

well as individual preferences. As a result of significantly higher bids, women receive lower average payoffs from the contest than men (-10.5 versus -8.9).[20]

Another interesting observation is that the *winning* coefficient becomes insignificant when we introduce demographic controls. This suggests that the utility of winning and demographic characteristics are correlated. For instance, we find that women bid twice as much for the prize of 0 than men (9.75 versus 4.86). The correlation between *gender* and *winning* may be due to women being more sensitive to the context of the experiment and they associated a bid of zero with doing nothing (Croson and Gneezy, 2009). Even so, the fact that *gender* is significant even after controlling for *winning* suggests that the gender effect is not captured merely by women's being more sensitive to the context of the experiment. To examine whether this result is impacted by women's participation, we estimate a random effects probit model reported in Table 6 as specification (4), where the dependent variable is an indicator variable taking value of 1 if subject's bid is greater than 1 (signifying participation) and 0 otherwise. The results indicate that women are more likely (although marginally) to participate in contests.[21] Moreover, bidding for 0 (measured by *winning*) is a significant indicator of participation in contests.

Finally, we find that the *religiosity* variable, which measures the importance of religion in daily life, is significant in specification (3) of Table 6. Subjects who consider religion to be a very important part of their daily life make 26% lower bids in contest (32.5 versus 41.2).[22] As

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[20] The lower earnings for women is not statistically significant as evidenced by a regression similar to Table 3 but including a female indicator. These results are available upon request.

[21] Alternatively, Morgan et al. (2012) find that when given the choice to either participate in a contest or take a sure payoff, females enter into the contest at a similar rate relative to males. More research into participation is indeed warranted given the voluminous literature which points towards female's relative abstention from competitive situations (e.g. Niederle and Vesterlund 2007).

[22] We asked subjects to answer the question "How important do you consider religion in your daily life?". Subjects then chose one of the following answers: (1) very important, (2) somewhat important, (3) a little important, and (4) not at all important. We code the variable *religiosity* as 1 if subject's answer was (1) and 0 otherwise.

with the gender effect, the religiosity effect (i.e., 26%) is bigger than the treatment effects (i.e., 11-16%) and is persistent throughout the duration of the experiment (see Figure 4). Furthermore, when we control for demographic variables in specification (3), including religiosity and gender, we find that treatment effects are not significant, suggesting that a large portion of treatment effects are subsumed by the demographic effects. We further investigate religiosity and gender in more detail in the next sections.[23]

4.4. Gender and Religiosity

To more fully understand the differences in religiosity and gender we consider the fully interacted model in Table 7. In particular, for every independent variable included in specification (2) of Table 6, we interact that variable with either *gender* or *religiosity*. Specifications (1) and (2) in Table 7 show the estimation results of the fully interacted model with the *gender* indicator variable. Specifications (3) and (4) show the estimation results of the interacted model where *religiosity* is interacted with other independent variables. The interaction term, if significant, indicates that there is a difference in how the dependent variable affects either group in the *religiosity* or *gender* indicator variables.

First, we focus on specifications (1) and (2) where the pertinent interaction variable is the *gender* indicator. In particular, specification (1) in Table 7 shows that there are two significant (although marginally) interaction variables: *treatment* and *religiosity*. The significant interaction of *gender* and *treatment* suggests that treatments do little to mitigate the overbidding of women

[23] For a robustness check, we also re-estimated Table 6, including the lagged variables as in Table 4 along with interaction terms on gender and religiosity. Similarly to when comparing Table 3 and Table 4, the main variables of interest in Table 6 remain significant even after controlling for different lag variables. The only significant result that is new is that women's response to *otherbid-lag* is different than men's response: men's bids in period *t* are negatively correlated with *otherbid-lag*, whereas women's bids are positively correlated. The estimation results are available from authors upon request.

in the experiment. Moreover, this implies that the effect of gender partially mitigates the treatment effects in the experiment. The interaction term of *gender* and *religiosity* is also significant and positive, suggesting that women who identify themselves as very religious bid more than women who do not identify themselves as very religious.[24] Examining specification (2), which includes interaction terms only for the variables which were not included on the demographic questionnaire, we see that the *treatment* variable is significant. This suggests that the treatment effects are at least partially mitigated by the inclusion of the demographic variables.

Next, we focus on specifications (3) and (4) where the interaction variable is the *religiosity* indicator. When we consider the interaction of *religiosity* and other dependent variables in specification (3), we see that there are three significant interaction variables: *safe*, *class*, and *gender*. The results for the interaction of *safe* and *religiosity* imply that for a given level of risk tolerance those subjects who identify as being highly religious bid significantly less than those who do not identify as being highly religious. This is interesting in light of the fact that research has documented that religious people are usually more risk averse (Hilary and Hui, 2009; Kumar et al., 2011). However, our findings do not necessarily suggest that religious people are more risk averse but that they bid lower in contests relative to others who have similar measured risk preferences.[25]

Since we discussed the interaction of *religiosity* and *gender* previously, the last difference to discuss is the interaction of the number of economics *classes* taken and *religiosity*. In

[24] It should be noted that the females who identify as highly religious (religiosity = 1) do not drive the gender result. In particular, if we remove the subjects who identify as highly religious and rerun the regression from Table 6 we see that the female indicator is still significant albeit with a the higher p-value of 0.08. These results are available by request.

[25] Based on a simple OLS regression with robust standard errors, we do not find a significant difference between risk preferences by religious and non-religious subjects (p-value = 0.39)

particular, the results suggest that those subjects who identify themselves as highly religious bid significantly more the more economics courses they take. Although interesting, this result does not come as a surprise. It is well recognized that students studying business and economics have more profit maximizing positions (Rubinstein, 2006) and they act more selfishly (Meier and Frey, 2004).

In summary, it appears that both gender and religiosity significantly impact individual behavior in contests. In the concluding section we discuss application of these findings in relation to the extant literature and suggest directions for future research.

5. Discussion and Conclusions

Our results contribute to several areas of research. First, our study contributes to the discussion on how to reduce overbidding in contests and enhance efficiency. Our findings indicate that an important contributing factor to overbidding is the fact that subjects receive windfall endowments (house money) before participating in contests. When subjects earn their endowments the bidding decreases by 11-16%. Therefore, our results suggest that one way to increase efficiency in contests is simply by making subjects earn their endowments before participating in contests. This is particularly useful information for managers who are tasked with allotting costly resources to departmental units within an organization.

Second, the results of our experiment can partially explain why subjects' behavior in contests is heterogeneous, with some subjects making very high and some making very low bids. A usual explanation for a high variance in individual bidding behavior is that subjects have heterogeneous preferences towards winning (Sheremeta, 2010, 2015) and risk (Sheremeta, 2011, 2015; Mago et al., 2013; Shupp et al., 2013). We also find that subjects' preferences towards winning and risk are significant predictors of individual bidding behavior. However, more importantly, we find that demographic differences, such as gender and religiosity, are significant sources of variation in subjects' bidding behavior. Specifically, we find that women make 25% higher bids than men and subjects who consider religion to be a very important part of their daily life make more than 26% lower bids in contests. Remarkably, demographic effects are even bigger than the treatment effects, suggesting that in addition to heterogeneous preferences, a significant part of differences in individual bidding may be attributed to differences in demographic characteristics. Indeed, our results show that the treatment effects are at least partially mitigated by controlling for the gender effect on bidding behavior with earning the endowment having substantially less impact on the overbidding of women relative to men.

This again is very important for the use and allocation of resources to departmental units within an organization. Just as the manager must allocate resources in a circumspect way, the manager must also anticipate how resources are to be used once they have been allocated within an organization. The evidence from this study suggests that an important aspect of this paradigm is to understand the preferences of those who make decisions about the use and allocation of costly resources.

Third, the results pertaining to gender contribute to the large literature on gender differences (Croson and Gneezy, 2009). Specifically, our results show that in a contest setting women bid significantly higher relative to men. As a result, women receive lower average earnings than men. This may affect women's propensity to enter freely into similar contests and contribute to the discussion of gender preferences for competition.[26] As outlined in Niederle and

[26] The fact that women perform in contests worse than men also suggests that there may be little demand for women in jobs where competition is present in wages. For details of this concern see Price (2012), where the author places subjects in an environment very similar to Niederle and Vesterlund (2007) except that one subject (a manager) chooses a compensation scheme for another (a worker).

Vesterlund (2007), the reluctance to commit to a competitive situation may be an important aspect of explaining the gender-wage disparity (Blau and Kahn, 2006) and low representation of women in top-level corporate management (Bertrand and Hallock, 2001).

Our findings concerning gender in contests are also consistent with research on gender effects in auctions. Ham and Kagel (2006) and Casari et al. (2007), for example, find that women are more susceptible to the winner's curse. Controlling for individual ability and other factors, the authors conjecture that women's overbidding in a common value auction may reflect a relative lack of familiarity with competitive market interactions. Ong and Chen (2012) find that women overbid more than men in all-pay auctions. Overall, it seems that overbidding by women in auctions is a robust phenomenon (Charness and Levin, 2009; Chen et al., 2013). Although contests are rather different than auctions, it is intriguing to find similar gender effects in both environments.

Lastly, the results regarding religiosity and bidding behavior in contests contribute to the growing literature on religion and economic behavior (Hoffmann, 2013). Our results show that subjects who consider religion to be a very important part of their daily life make 26% lower bids in contests than others. There are several possible explanations for the significant difference in behavior of religious subjects (Iannaccone, 1998). First, it is usually the case that most religions provoke people to care about and trust others (Tan and Vogel, 2008), as well as to be more pro-social (Ahmed, 2009; Benjamin et al., 2012). This may produce less competitive behavior in contests from more religious subjects. It is also documented in the literature that more religious people are more compassionate towards the disadvantaged (Batson et al., 1993; Regnerus et al., 1998). Therefore, one would expect that when confronted with other subjects in a contest, more religious subjects may yield the competition in favor of their counterparts.

In addition to the impact this study has on the current set of literature, this study also suggests advances in future research along two important dimensions. First, as noted above, we have investigated the impact of gender in relation to overbidding in contests. This study highlights a new avenue for studying gender differences in competition (e.g., Gneezy et al., 2009). In particular, future work should consider how previous results concerning women's preference to abstain from competition in wages is related to women's overbidding in the context of contests. Second, given the fact that more than 60% of Americans self-classify as religious (Joas, 2008), it is imperative to investigate how religiosity impacts individual behavior in competitive environments. From the results of our experiment, it appears that religiosity is as important as gender in explaining individual behavior in contests. It occurs to us that investigations of the impact of demographic factors, such as religiosity and gender, on decisions in competitive environments both inside and outside the laboratory are especially fruitful and insightful avenues for future research.

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Table 1: Average Bids and Payoffs

Treatment	Average Endowment	Average Bid	Average Payoff
Equilibrium		22.5	7.5
Gift	$20.00 (0.0)	43.2 (0.8)	-13.2 (1.1)
Yardstick	$20.00 (0.0)	36.4 (0.7)	-6.4 (1.1)
Earn	$18.53 (0.1)	38.6 (0.8)	-8.6 (1.1)

The standard errors are in parentheses.

Table 2: Between-Subjects and Within-Subject Variation

Treatment	Gift	Yardstick	Earn
	Between-Subjects Variation		
Periods 1-15	30.4 (0.7)	32.0 (0.7)	33.8 (0.7)
Periods 16-30	31.9 (0.7)	28.6 (0.7)	31.7 (0.7)
	Within-Subject Variation		
Periods 1-15	18.5 (0.8)	16.3 (0.8)	16.7 (0.8)
Periods 16-30	15.0 (0.8)	12.1 (0.7)	13.9 (0.8)

The standard errors are in parentheses. The between-subjects variation measure is calculated as the absolute difference between the individual bid in period t and the average group bid in period t, averaged over all periods of the experiment. The within-subject variation measure is calculated as the absolute difference between the individual bid in period t and period t-1, averaged over all

Table 3: Treatment Effects

Specification	(1)	(2)	(3)	(4)
Dependent variable, *bid*	Gift vs. Yardstick & Earn	Gift vs. Yardstick	Gift vs. Earn	Yardstick vs. Earn
gift	5.70**	6.80**	4.60	
[1 if Gift]	(2.83)	(2.97)	(2.95)	
yardstick				-2.20
[1 if Yardstick]				(1.61)
period	-0.30***	-0.29***	-0.26***	-0.36***
[period trend, *t*]	(0.07)	(0.09)	(0.08)	(0.09)
constant	42.17***	40.84***	42.61***	44.15***
	(1.63)	(2.23)	(1.91)	(1.96)
Observations	6480	4320	4320	4320

* significant at 10%; ** significant at 5%; *** significant at 1%. The standard errors in parentheses are clustered at the session level. All models include a random effects error structure, with the individual subject as the random effect, to account for the multiple decisions made by individual subjects.

Table 4: Treatment Effects and Learning

Specification	(1)	(2)	(3)	(4)
Dependent variable, *bid*	Gift vs. Yardstick & Earn	Gift vs. Yardstick	Gift vs. Earn	Yardstick vs. Earn
gift	2.36**	2.64**	1.91	
[1 if Gift]	(1.17)	(1.18)	(1.25)	
yardstick				-0.98
[1 if Yardstick]				(0.68)
period	-0.16***	-0.15***	-0.14***	-0.18***
[period trend, *t*]	(0.04)	(0.04)	(0.04)	(0.05)
win-lag	1.00	0.19	0.64	2.22**
[1 if win in *t*-1]	(1.08)	(1.37)	(1.47)	(1.08)
bid-lag	0.63***	0.63***	0.63***	0.63***
[bid in period *t*-1]	(0.03)	(0.03)	(0.03)	(0.04)
otherbid-lag	-0.01	0.00	-0.01	-0.02**
[sum of opponents' bids in period *t*-1]	(0.01)	(0.01)	(0.01)	(0.01)
constant	16.81***	15.50***	17.20***	18.57***
	(1.62)	(1.89)	(2.00)	(2.00)
Observations	6264	4176	4176	4176

* significant at 10%; ** significant at 5%; *** significant at 1%. The standard errors in parentheses are clustered at the session level. All models include a random effects error structure, with the individual subject as the random effect, to account for the multiple decisions made by individual subjects.

Table 5: Summary of Demographic Characteristics and Preferences

Variable	Description	Mean	Std. Dev.	Min	Max
winning	bid for the prize of 0	7.85	22.50	0	120
quiz	number of correct quiz answers	7.13	1.01	2	8
safe	number of safe options	9.35	2.47	1	15
gender	woman or man	0.35	0.48	0	1
religiosity	religion is very important	0.19	0.39	0	1
denomination	Christian or other	0.48	0.50	0	1
major	business or econ major	0.30	0.46	0	1
classes	number of econ classes taken	3.50	4.13	0	25
age	participant's age	20.85	1.80	18	33

Table 6: Determinants of Bids

Specification	(1)	(2)	(3)	(4)
Dependent variable	*bid*	*bid*	*bid*	I(*bid* > 1)
earn	-5.59**	-5.59**	-5.39	-0.33
[1 if Earn]	(2.83)	(2.85)	(3.38)	(0.32)
yardstick	-3.58	-3.64	-2.77	-0.12
[1 if Yardstick]	(2.92)	(2.90)	(3.10)	(0.33)
period	-0.30***	-0.30***	-0.36***	-0.02***
[period trend, *t*]	(0.07)	(0.07)	(0.08)	(0.00)
winning	0.15**	0.14**	0.11	0.02***
[bid for the prize of 0]	(0.07)	(0.06)	(0.09)	(0.01)
quiz	-5.41***	-5.64***	-4.95***	0.09
[number of correct quiz answers]	(1.71)	(1.65)	(1.84)	(0.13)
safe		-2.14***	-1.92***	-0.12**
[number of safe options]		(0.56)	(0.62)	(0.06)
gender			7.14***	0.51*
[1 if woman]			(2.76)	(0.29)
religiosity			-7.90***	-0.57
[1 if religion is very important]			(2.84)	(0.35)
denomination			-3.83	-0.33
[1 if Christian]			(2.41)	(0.27)
major			-1.74	0.38
[1 if business or econ major]			(4.18)	(0.35)
classes			-0.28	-0.04
[number of econ classes taken]			(0.57)	(0.04)
age			-1.20*	0.06
[participant's age]			(0.68)	(0.09)
constant	84.50***	106.25***	134.71***	1.50
	(13.14)	(14.26)	(18.26)	(1.92)
Observations	6480	6480	5400	5400

* significant at 10%; ** significant at 5%; *** significant at 1%. Specifications (1)-(3) use random effects models, with the individual subject as the random effect, to account for the multiple decisions made by individual subjects. The standard errors in parentheses are clustered at the session level. Specification (4) uses a random effects probit model.

Table 7: Interaction Models

Specification	(1)	(2)	(3)	(4)
Dependent variable, *bid*	I = *gender*	I = *gender*	I = *religiosity*	I = *religiosity*
gender	-28.53	10.12	5.15*	5.24**
[1 if woman]	(59.36)	(32.58)	(2.76)	(2.58)
I × *gender*			19.40***	24.22***
[interaction term]			(6.30)	(6.38)
religiosity	-11.06***		-9.01	-59.86
[1 if religion is very important]	(2.87)		(53.45)	(41.05)
I × *religiosity*	14.46*			
[interaction term]	(8.34)			
treatment	-6.13	-7.80**	-4.66	-4.43
[1 if Earn or Yardstick]	(3.75)	(3.52)	(3.32)	(3.65)
I × *treatment*	8.19*	10.11**	-6.39	-2.28
[interaction term]	(4.57)	(4.60)	(6.01)	(7.01)
period	-0.37***	-0.29***	-0.31***	-0.31***
[period trend, *t*]	(0.09)	(0.08)	(0.10)	(0.10)
I × *period*	0.03	-0.05	-0.26	-0.26
[interaction term]	(0.26)	(0.25)	(0.30)	(0.30)
winning	0.03	0.09	0.09	0.09
[bid for the prize of 0]	(0.09)	(0.08)	(0.11)	(0.09)
I × *winning*	0.19	0.10	0.23	0.42**
[interaction term]	(0.21)	(0.18)	(0.20)	(0.17)
quiz	-4.17*	-4.96**	-5.51***	-6.13***
[number of correct quiz answers]	(2.32)	(2.18)	(1.92)	(1.64)
I × *quiz*	-2.56	-1.67	6.39	10.35**
[interaction term]	(3.59)	(3.32)	(4.11)	(4.18)
safe	-1.73*	-2.33***	-1.40*	-1.48*
[number of safe options]	(1.00)	(0.69)	(0.79)	(0.76)
I × *safe*	-0.59	0.27	-2.97**	-2.60*
[interaction term]	(1.29)	(1.07)	(1.31)	(1.50)
denomination	-1.46		-2.87	
[1 if Christian]	(3.17)		(2.93)	
I × *denomination*	-5.73		-3.42	
[interaction term]	(4.13)		(7.28)	
major	-0.15		-1.91	
[1 if business or econ major]	(6.08)		(4.14)	
I × *major*	-1.38		-9.58	
[interaction term]	(11.58)		(8.66)	
classes	-0.04		-0.59	
[number econ classes taken]	(0.93)		(0.48)	
I × *classes*	-0.49		4.40***	
[interaction term]	(1.04)		(1.27)	
age	-1.86**		-0.90	
[participant's age]	(0.82)		(0.70)	
I × *age*	2.63		-1.21	
[interaction term]	(2.50)		(1.76)	
constant	134.00***	102.99***	121.49***	103.47***
	(26.98)	(19.36)	(16.63)	(14.90)
Observations	5400	5400	5400	5400

* significant at 10%; ** significant at 5%; *** significant at 1%. The standard errors in parentheses. All models include a random effects specification on subject and clustered errors at the session level.

Figure 1: Average Bid Over 30 Periods

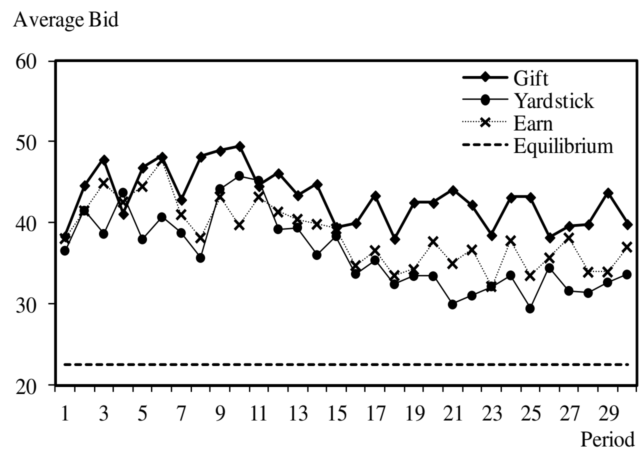
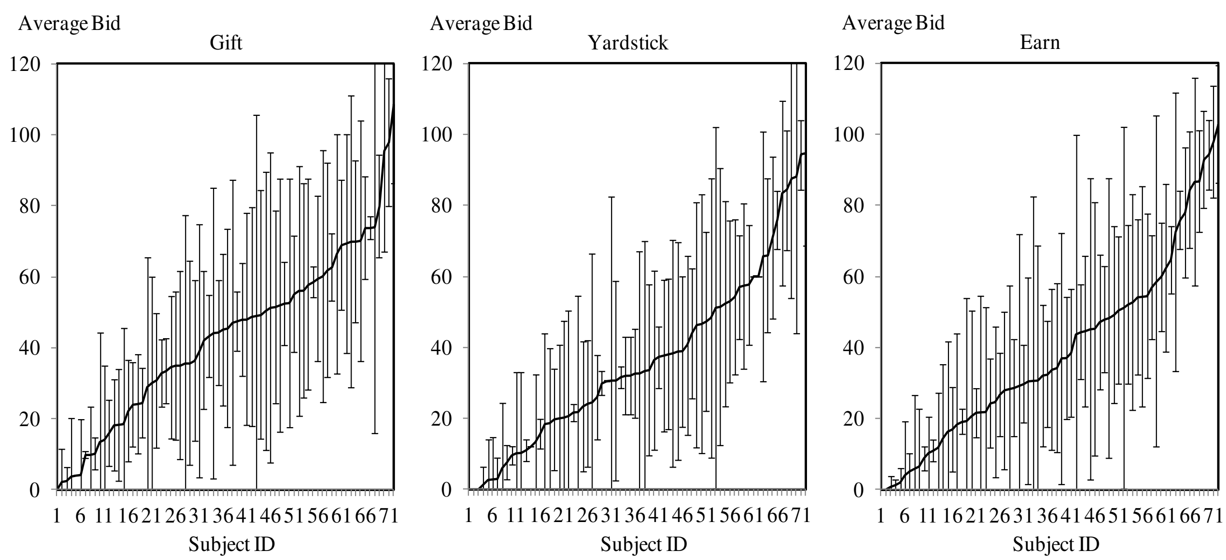


Figure 2: Distribution of the Average Bid by Subject in Each Treatment



Note: Subjects are sorted in increasing order by the average bid, which is indicated by solid line. The error bars represent standard deviations of bids for each subject over all 30 periods.

Figure 3: Average Bid by Gender (All Treatments Combined)

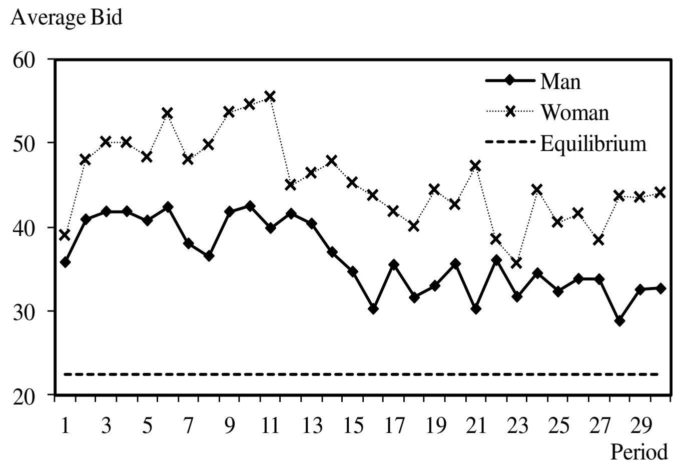
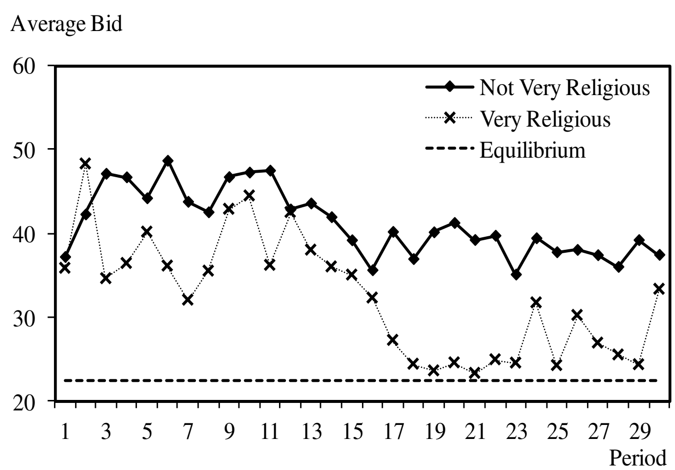


Figure 4: Average Bid by Religiosity (All Treatments Combined)



Appendix (Not for publication) – Instructions for Treatment Earn

GENERAL INSTRUCTIONS

This is an experiment in the economics of strategic decision making. Various research agencies have provided funds for this research. The instructions are simple. If you follow them closely and make appropriate decisions, you can earn an appreciable amount of money.

The experiment will proceed in four parts. The currency used in Part 1 and Part 2 of the experiment is U.S. Dollars. The currency used in Parts 3 and 4 of the experiment is francs. At the end of the experiment, francs will be converted to U.S. Dollars at a rate of **_60_** francs to **_1_** dollar. Your earnings today will be calculated as the sum of your earnings in each part of the experiment. At the end of today's experiment, you will be paid in private and in cash. **12** participants are in today's experiment.

It is very important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your cooperation.

At this time we will proceed to Part 1 of the experiment.

INSTRUCTIONS FOR PART 1
YOUR DECISION

In this part of the experiment you will be asked to make a series of choices in decision problems. How much you receive will depend partly on **chance** and partly on the **choices** you make. The decision problems are not designed to test you. What we want to know is what choices you would make in them. The only right answer is what you really would choose.

For each line in the table in the next page, please state whether you prefer option A or option B. Notice that there are a total of **15 lines** in the table but just **one line** will be randomly selected for payment. You do not know which line will be paid when you make your choices. Hence you should pay attention to the choice you make in every line. After you have completed all your choices a token will be randomly drawn out of a bingo cage containing tokens numbered from **1 to 15**. The token number determines which line is going to be paid.

Your earnings for the selected line depend on which option you chose: If you chose option A in that line, you will receive **$1**. If you chose option B in that line, you will receive either **$3** or **$0**. To determine your earnings in the case you chose option B there will be second random draw. A token will be randomly drawn out of the bingo cage now containing twenty tokens numbered from **1 to 20**. The token number is then compared with the numbers in the line selected (see the table). If the token number shows up in the left column you earn $3. If the token number shows up in the right column you earn $0.

Are there any questions?

Decision no.	**Option A**	**Option B**		Please choose A or B
1	**$1**	**$3** never	**$0** if 1,2,3,4,5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
2	**$1**	**$3** if 1 comes out of the bingo cage	**$0** if 2,3,4,5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
3	**$1**	**$3** if 1 or 2	**$0** if 3,4,5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
4	**$1**	**$3** if 1,2,3	**$0** if 4,5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
5	**$1**	**$3** if 1,2,3,4,	**$0** if 5,6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
6	**$1**	**$3** if 1,2,3,4,5	**$0** if 6,7,8,9,10,11,12,13,14,15,16,17,18,19,20	
7	**$1**	**$3** if 1,2,3,4,5,6	**$0** if 7,8,9,10,11,12,13,14,15,16,17,18,19,20	
8	**$1**	**$3** if 1,2,3,4,5,6,7	**$0** if 8,9,10,11,12,13,14,15,16,17,18,19,20	
9	**$1**	**$3** if 1,2,3,4,5,6,7,8	**$0** if 9,10,11,12,13,14,15,16,17,18,19,20	
10	**$1**	**$3** if 1,2,3,4,5,6,7,8,9	**$0** if 10,11,12,13,14,15,16,17,18,19,20	
11	**$1**	**$3** if 1,2, 3,4,5,6,7,8,9,10	**$0** if 11,12,13,14,15,16,17,18,19,20	
12	**$1**	**$3** if 1,2, 3,4,5,6,7,8,9,10,11	**$0** if 12,13,14,15,16,17,18,19,20	
13	**$1**	**$3** if 1,2, 3,4,5,6,7,8,9,10,11,12	**$0** if 13,14,15,16,17,18,19,20	
14	**$1**	**$3** if 1,2, 3,4,5,6,7,8,9,10,11,12,13	**$0** if 14,15,16,17,18,19,20	
15	**$1**	**$3** if 1,2, 3,4,5,6,7,8,9,10,11,12,13,14	**$0** if 15,16,17,18,19,20	

INSTRUCTIONS FOR PART 2

For this part of the experiment you will be asked to calculate the sum of five randomly generated two-digit numbers. You will be given **10 minutes** to calculate the correct sum for a series of these problems. You cannot use a calculator to determine this sum, however you are welcome to use the supplied scratch paper. You submit an answer by clicking the submit button with your mouse. When you enter an answer, the computer will immediately tell you whether your answer is correct or not and supply another summation problem. I will give notice when 30 seconds remain.

YOUR EARNINGS

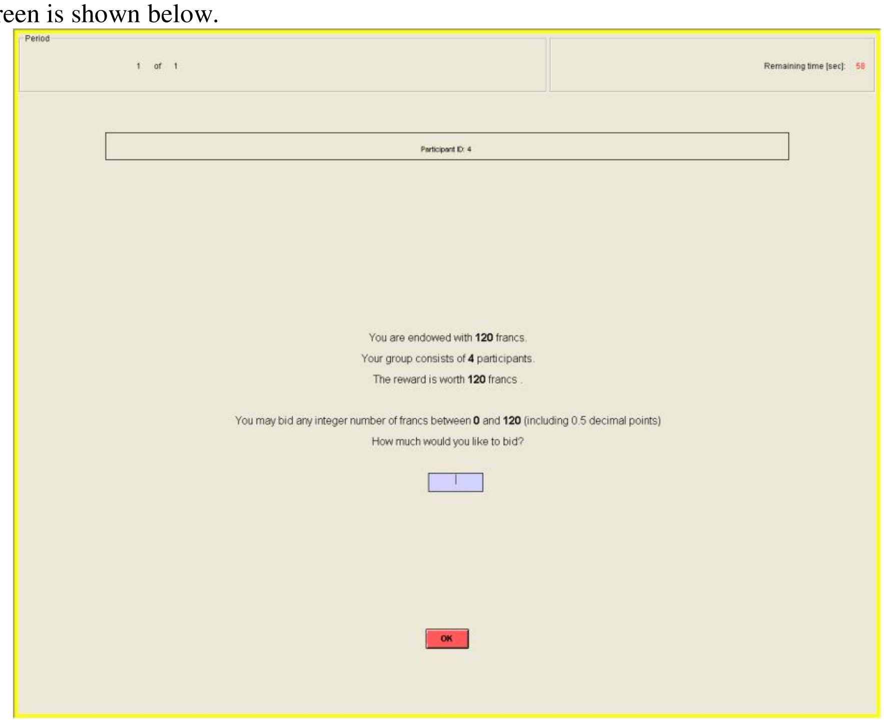
You will receive $1 per problem that you correctly solve within the 10 minutes. Your payment does not decrease if you provide an incorrect answer to a problem.

INSTRUCTIONS FOR PART 3

YOUR DECISION

You may have already earned some money from the first part of the experiment, although we will determine how much at the conclusion of the experiment. In addition to this amount, you have also earned some money from the second part of the experiment. In part 3 of the experiment, you may receive either positive or negative earnings.

The third part of the experiment consists of **30** decision-making periods. At the beginning of each period, you will be randomly and anonymously placed into a group of **4 participants**. The composition of your group will be changed randomly every period. The reward is worth **120** francs to you and the other three participants in your group. You may bid any integer number of francs between **0** and **120** (including 0.5 decimal points). An example of your decision screen is shown below.



YOUR EARNINGS

After all participants have made their decisions, your earnings for the period are calculated. Regardless of who receives the reward, all participants will have to pay their bids. Thus, your period earnings will be calculated in the following way:

If you receive the reward: Earnings = Reward – Your Bid = 120 – Your Bid

If you do not receive the reward: Earnings = No Reward – Your Bid = 0 – Your Bid

Remember, in the first and second part of this experiment you have earned money. In this part of the experiment, depending on a period, you may receive either positive or negative earnings. At the end of the experiment we will randomly select 5 out of 30 periods for actual payment. You will sum the total earnings for these 5 periods and convert them to a U.S. dollar payment. If the earnings are negative, we will subtract them from your earnings. If the earnings are positive, we will add them to your earnings.

The more you bid, the more likely you are to receive the reward. The more the other participants in your group bid, the less likely you are to receive the reward. Specifically, for each franc you bid you will receive one lottery ticket. At the end of each period the computer **draws randomly** one ticket among all the tickets purchased by **4 participants** in the group, including you. The owner of the drawn ticket receives the reward of 120 francs. Thus, your chance of receiving the reward is given by the number of francs you bid divided by the total number of francs all 4 participants in your group bid.

$$\text{Your chance of receiving a reward} = \frac{\text{Your Bid}}{\text{Sum of all 4 Bids in your group}}$$

If all participants bid zero, the reward is randomly assigned to one of the four participants in the group.

<u>Example of the Random Draw</u>

This is a hypothetical example used to illustrate how the computer makes a random draw. Let's say participant 1 bids 10 francs, participant 2 bids 15 francs, participant 3 bids 0 francs, and participant 4 bids 40 francs. Therefore, the computer assigns 10 lottery tickets to participant 1, 15 lottery tickets to participant 2, 0 lottery tickets to participant 3, and 40 lottery tickets for participant 4. Then the computer randomly draws **one lottery ticket out of 65** (10 + 15 + 0 + 40). As you can see, participant 4 has the **highest chance** of receiving the reward: **0.62 = 40/65.** Participant 2 has **0.23 = 15/65** chance, participant 1 has **0.15 = 10/65** chance, and participant 3 has **0 = 0/65** chance of receiving the reward.

After all participants make their bids, the computer will make a random draw which will decide who receives the reward. Then the computer will calculate your period earnings based on your bid and whether you received the reward or not.

At the end of each period, your bid, the sum of all bids in your group, whether you received the reward or not, and the earnings for the period are reported on the outcome screen as shown below. Once the outcome screen is displayed you should record your results for the period on your **Personal Record Sheet** under the appropriate heading.

IMPORTANT NOTES

You will not be told which of the participants in this room are assigned to which group. At the beginning of each period you will be randomly re-grouped with three other participants to from a four-person group. You can never guarantee yourself the reward. However, by increasing your bid, you can increase your chance of receiving the reward. Regardless of who receives the reward, all participants will have to pay their bids.

At the end of the experiment we will randomly choose **5 of the 30** periods for actual payment in **Part 3** using a bingo cage. You will sum the total earnings for these 5 periods and convert them to a U.S. dollar payment. **Are there any questions?**

INSTRUCTIONS FOR PART 4

The fourth part of the experiment consists of only 1 decision-making period and only **one stage**. The rules for **part 4** are the same as the rules for **part 3**. At the beginning of the period, you will be randomly and anonymously placed into a group of **four participants**. You will bid in order to be **a winner**. The only difference is that in part 4 the winner **does not receive the reward**. Therefore, the reward is worth **0** francs to you and the other three participants in your group. After all participants have made their decisions, your earnings for the period are calculated.

Earnings = 0 – Your Bid

After all participants have made their decisions, you will learn whether you win or not. The computer then will display your earnings for the period on the outcome screen. Your earnings will be converted to cash and paid at the end of the experiment.