

THE PARABLE OF THE GREAT BANQUET: INSIGHTS FROM LABORATORY ECONOMICS

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ABSTRACT

The Parable of the Great Banquet tells the story of individuals who made a decision not to attend a large feast that, from the context of the story, would appear to have been of great value to them. We use the tools of laboratory experimental economics to gain more understanding of the economic or behavioral reasons behind such a decision. A central feature of our design is ambiguity aversion flowing from advice given by a conflicted expert. The results indicate a strong propensity of our subjects towards ambiguity aversion in this context. Nevertheless, the traditional economic features of the underlying individual choice problem also proved to be important in explaining behavior.

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I. INTRODUCTION

The methodology of experimental economics is employed by a variety of social sciences and frequently attracts interdisciplinary study. But, never before has experimental methodology partnered with theological studies. Other familiar economic skill sets, such as optimization theory and econometrics, have attempted to explain or offer insight into religious themes. One of the most notable is Brams' (1980) exposition of game theory and the Hebrew Bible which received mixed reviews from Biblical scholars, some of whom objected to the fact the game-theoretic structure led Brams to make assertions about the nature of God and his relationship with man. The praiseworthy portion of his study was his ability to offer a glimpse into the thought processes of characters in a given story (Ackerman, 1982). This paper seeks to walk the line between insightfulness and inflated ramifications with an individual choice study of The Parable of the Great Banquet (Luke 14:15 – 23; the parable is presented in full in Appendix I).

The Parable of the Great Banquet possesses terrific richness, layers, and is commonly interpreted as the End Times Messianic Banquet prophesied in the Book of Isaiah (Isaiah 25:6 - 9; Bailey, [2008]). But, the advantage of the laboratory lies in our ability to narrow the focus of such a layered story to a small slice in a controlled environment. So what part of human nature would have been important, even if unstated, in Jesus' story about a decision to skip a potentially profitable evening at the home of a wealthy man? Our experimental design attempts to capture the behavior of the invitees in the parable. We examine the decisions of "invitees" to choose to participate either in a

known payment (per task completed) or in a “banquet” whose payoff is conveyed by the banquet host (by electronic chat) with principal-agent incentives. The experiment also features an unknown and random ending time between 0 and 900 seconds. With this experiment we hope to gain perspective about which rational or behavioral components of individual choice models best describe the decision of the invitees in our experiment.

The choice situation in the parable can be viewed as a version of a two-armed bandit problem. In a classic version of the problem, the probability distribution of returns to one arm is known to the individual. However, the knowledge about the distribution of returns to the second arm is only gained by experience with using the second arm, with the opportunity cost of forgoing the returns from the first arm (DeGroot, 1970; Anderson, 2001).

Our design captures a different manifestation of the bandit problem. The “known” arm is a computer screen with access to a repeated, incentivized task, allowing an individual to earn money at a known rate. The “unknown” arm (called the “separate screen”) yields a payoff of \$15 dollars *provided the individual is logged onto the separate and not the first screen at the time the experiment ends (T)*. The central design feature of the experiment is that invitees only learn the value of being on the separate screen through representations made by the banquet host. Furthermore, the banquet host faces incentives that may lead him to incorrect representations. As both Anderson (2001) and Maccheroni, et al. (2006) have suggested, this transfer of information through potentially conflicted experts is a process that may introduce ambiguity into otherwise

straightforward economic calculations.¹ It is the ambiguity of the payoff to the separate screen that is at the heart of our investigation, and which leads our inquiry into the behavioral realms of ambiguity aversion as classically proposed by Ellsberg (1961). Our investigation will be consistent with Camerer's (1995) definition that ambiguity exists when known-to-be-relevant information is not, in fact, known. However, our manifestation of ambiguity is different than in the traditional urn-and-ball experiments (surveyed in Camerer), lottery choice preference elicitation mechanisms (Moore and Eckel [2006]), or market experiments with ambiguous probabilities over states induced by the experimenters (Ahn, et al., [2009]).

We must reiterate that the purpose of this experiment is not to make theological statements about God. A tantalizing part of the parable as told in Luke is that the reader is not told explicitly why the invitees chose to skip the great banquet. It is well recognized that the reasons stated by the invitees in the parable are embarrassing excuses. So what part of human nature would have been important, even if unstated, in Jesus' story about a decision to skip a potentially profitable evening at the home of a wealthy man? Our experimental design attempts to capture the behavior of the invitees in the parable. We examine the decisions of the invitees to choose to participate either in the known payment (per task completed) of the first screen or in the "banquet" (the separate screen) whose payoff is conveyed by the banquet host with principal-agent incentives. (In our experiment, the host communicates to the invitees through the experimental chat interface). The next section (Section Two) describes the experimental design with greater thoroughness. In Section Three, we present results consistent with both rational-choice

¹ There is nothing in the Parable of the Great Banquet that suggests any conflicted, principal-agent incentives on the part of the banquet host. However, inducing such incentives is our way of creating in the laboratory a context of ambiguity that we believe is relevant to the decisions of the invitees in the parable.

and behavioral models: on the one hand, subjects clearly display behavior that is consistent with ambiguity aversion. On the other hand, in explaining why some subjects chose the separate screen in the presence of ambiguity conventional economic concepts (time remaining, ability, opportunity earnings, etc.) predict remarkably well. We were surprised when none of the explicit characteristics of the “host/invitee” chat related to issues of trust or credibility added anything significant to the predictive power of the model. Finally, the paper concludes with discussion about what insights may be gleaned from this analysis and discussion of future extensions for the design.

II . EXPERIMENTAL DESIGN

This experimental design is not intended to replicate the characters excusing their absence from the banquet. Indeed, the experimental context is incapable of capturing all of the nuances of this parable. For example, there are two large cultural omissions from this experiment:

- 1) In the context of the laboratory experiment subjects never previously promised the host they would attend the banquet², and
- 2) There was no social cost to lab participants for not attending the event³.

²The story begins, "A man was preparing a great banquet and invited many guests." Cultural Biblical scholar Kenneth Bailey writes, "In a traditional Middle Eastern village the host of a banquet invites a group of his friends. On the basis of the number of people who accept the invitation, he decides how much and what kind of meat he will serve." (Bailey, 2007). The characters of the parable therefore already accepted the invitation of the great banquet prior to their excuses. The remainder of the story therefore speaks to their excuses on the day of the banquet. This doesn't change the economic representation of our design. However, we recognize that if laboratory subjects had previously committed to attendance there could be a behavioral effect in which they felt obligated to return.

³ Absence from the banquet without a valid excuse was seen as an insult. Such an insult could incur major social costs such as the severance of relationships.

This weakness however is also a strength. By reducing a complex environment to a simpler form we can study how small alterations influence decisions to attend or miss the banquet. The treatments in our experiment include differences in payment per completion of the task as well as variation in the credibility of the banquet host.

All experiments were conducted at Florida State University in the Experimental Social Science Laboratory (XS/FS) on campus. Subjects were Florida State University students paid a ten dollar "show-up fee" and each subject earned additional money for decisions made during the experiment. The experimental interface was fully computerized and the software was programmed using z-Tree (Fischbacher, 2007).

For this study the experimental design involves four participants in each experimental session (Participants A, B, C, and D), with two role types and a random end time between 0 and 15 minutes. Each participant is assigned one of the two roles at random upon entry to the laboratory. Participants A, B, and C play the role of the invitee. Participant D plays the role of the banquet host, inviting those other participants to the banquet (called the "separate screen").

Participants A, B and C may participate in an incentivized task with a known payment per completion of a task. Incentivized tasks are commonly used in labor economics experiments to simulate real effort in the laboratory (Falk and Fehr, 2003). The incentivized task used in this experiment includes counting how many numbers are contained within a string of twenty randomly generated letters and numbers.⁴ The earnings per completion of the task are 5 or 10 cents depending on the treatment. Participants may also forgo the task screen in order to receive an unknown payment on

⁴ An example of such a string would be "EA1BC2PQ34679QRS9TUV". This task was introduced to us by our colleague Dmitry Ryvkin.

the separate screen, however, they will only receive this unknown payment if they are on the separate screen when the experiment ends⁵, which is randomly determined before the start of the experiment. Participants that choose the separate screen will be able to play the game of tic-tac-toe, which mitigates boredom (Engel [2007]), but are unable to earn money via the incentivized task while they are on the separate screen. Invitees may switch between the incentivized task screen and the separate screen as frequently as they choose without penalty.

Participant D utilizes the chat feature in the experimental program to invite the other participants (individually) to the separate screen. It is common information that the experimenters have informed Participant D of the true size of the payment to the invitees for ending the experiment on the separate screen. Moreover, Participant D can make a representation about this information to other participants through the chat interface. Also, it is common information that the host receives five dollars in payment for each participant that ends the experiment on the separate screen as opposed to \$2.50 for each person that does not. Therefore, invitees may view the host's representations as "cheap talk." Thus, this task captures a motivation of ambiguity as information coming from a potentially non-credible expert.⁶

The treatment with complete credibility does not include Participant D at all. Instead, in the complete credibility treatment the experimenter announces the fact that \$15 will be paid to those ending the experiment on the separate screen with certainty. The

⁵ Receipt of the payoff to the banquet if and only if the participant ends the experiment on the separate screen is similar to a theology in which "back-sliding" out of salvation can occur. The experiment is not intended to debate election as opposed to these views; however, the possibility of back-sliding does create tension in the decision to forgo a known payment for some unknown payment.

⁶ Note, this feature of the design may be considered to simulate those without social status and outsiders being brought to the banquet, but, it also has methodological importance in the sense that it curbs feelings of duty from the participants to ensure Participant D receives some money during the experiment.

only unknown remains the timing of the end of the experiment. Because the participants are told in advance that the ending of the experiment is uncertain and between 0 and 900 seconds, viewing the separate screen is not informative to subjects with regards to the p.d.f. of T.

Other details of the experiment include the two practice periods prior to the start of the experiment and an end-of-experiment survey. The first practice period familiarized subjects with the computer interface and was unpaid. The second practice period lasted two minutes and each correct response earned the same amount of money as the main experiment. This enabled us to measure a notion of ability for each subject. Finally, at the end of the experiment there was an unincentivized questionnaire that included two questions: "Have you ever heard of this experiment before?" and "What do you think this experiment was about?"⁷

Table 1 displays a matrix of the treatments. Included in the table are the number of sessions per treatment and the number of hosts and invitees. The randomly drawn times for these sessions were 92, 215, and 491 seconds and those are listed at the top of Table 1.

TREATMENT	92 SECONDS	215 SECONDS	491 SECONDS
Low Payment with Ambiguity	5 hosts, 15 invitees (5 sessions)	5 hosts, 15 invitees (5 sessions)	5 hosts, 15 invitees (5 sessions)
High Payment with Ambiguity	5 hosts, 15 invitees (5 sessions)	4 hosts, 12 invitees (4 sessions)	5 hosts, 15 invitees (5 sessions)
High Payment with Complete Credibility	8 invitees (all in one session)	4 invitees (all in one session)	7 invitees (all in one session)

TABLE 1: Roadmap of Experimental Sessions

⁷ Subject responses indicated that they had not heard about the experiment. Also, significant amounts of subjects believed the experiment was about either risk, trust, or both.

We can construct a straightforward, theoretical rational choice benchmark for this problem based upon the treatment with no ambiguity (that is, the situation in which the \$15 prize is known with certainty). With a) a known \$15 prize, b) the known uniform distribution on T of $[0,900]$ seconds, c) the known earnings per-correct-answer (5 cents or 10 cents), and d) a known ability rate for the subject, we can calculate the optimal switching point for an expected-income-maximizing subject. Furthermore, it can easily be seen that a risk-averse individual would switch no later than, and perhaps sooner than, one with the same ability but who was risk-neutral.⁸

Given the parameters of our design, these calculations might not be as formidable as might be expected. For example, consider subjects in the low payment condition (5 cents per correct answer). Unless their ability rate exceeds 20 correct answers per minute, such subjects should switch to the separate screen immediately (and, again, risk-averse subjects would require an even higher level of ability to justify entering the first screen). In the high payment condition, risk-neutral subjects would have to exceed 10 correct answers per minute to spend any time on the first screen.

III . RESULTS

We conducted a total of 29 sessions with ambiguity and differing payment per completion of task. There were also 3 control sessions conducted where subjects experienced no ambiguity and a high payment per completion of task. These sessions

⁸ The logic is as follows. The increasing hazard rate together with constant opportunity earnings implies that when an individual switches, he knows that he will not switch back. Let $T_i^*(r_i)$ be the optimal switching point for a risk-neutral individual with ability rate r_i . Then at $T_i^*(r_i)$ switching to the separate screen entails a \$15 payoff with certainty, while remaining on the first screen involves a lottery. By assumption, the expected payoff to switching to the separate screen is greater than the expected payoff to the lottery implied by remaining on the first screen (and switching at some later time). If the individual were risk averse, then clearly the expected utility of the lottery would be less than or equal to the expected utility of the \$15 with certainty. Together with the increasing hazard rate, this implies that a risk-averse individual with ability rate r_i would switch no later, and perhaps earlier, than $T_i^*(r_i)$.

yielded a total of 135 observations. Of those observations 116 were from the sessions with ambiguity. Because we are specifically studying the behavior of invitees we drop the observations of Participant D in those experiments resulting in 87 observations in the ambiguity condition. In the complete credibility experiments there were a total of 19 observations (none were dropped because there were no hosts). Sessions were all conducted with the same sequence of randomly generated end times (in the actual order: 215, 92, and 491 seconds).

As a preface, we report the distribution of abilities of the subjects as measured by the second (incentivized) practice round of the problem solving exercise. The frequency distributions for the high payment and low payment conditions (of the invitees who will be facing a banquet host) are presented in Figure 1.

Figure 1 About Here

The means of the two samples are 8.8 for the low wage individuals and 8.7 for the high wage individuals. The difference is highly insignificant ($t = 0.29$). A Kolmogorov-Smirnov test indicates that we cannot reject, at any usual level of significance, that the two distributions are drawn from the same population. This indicates that for the responding invitees the combination of the different draw of subjects and the difference in payment per correct score did not significantly alter the measured ability levels.

There is, however, an additional possible difference in abilities between the two treatments. For the low wage individuals, the highest measured ability is 13, which means that all low wage individuals should, if the \$15.00 payment is credible, switch immediately to the high screen. For the high wage individuals, the story is slightly different. All of the low wage individuals should switch before 215 seconds, but three of

the 15 individuals in the high wage, 92-second treatment have measured abilities that might suggest not switching before the 92 seconds are up.

The second comparison that should be examined is any difference between the measured ability of the high payment (ambiguity) invitees and the subjects in the individual choice (credibility) treatment. The relevant distributions are displayed in Figure 2.

Figure 2 About Here

The mean ability of the subjects facing ambiguity is 8.7 and of subjects in the credibility treatment it is 8.9. The difference in means is highly insignificant, as is a K-S test on the distributions. Also, all of the individuals in the individual choice (credibility) sessions with an ending time of either 215 or 491 should switch before their endpoints, just as in the ambiguity sessions. And the proportion of individuals in the 92-second condition who have abilities such that they might optimally not switch is almost identical between the ambiguity and credibility treatments (3 out of 15 versus 2 out of 8). Thus, despite the differences in the draw of subjects and the slightly different instructions, the profile of measured abilities across these two treatments is quite comparable.⁹

Before any econometric analysis of the results we present Table 2 with descriptive statistics regarding the different treatments. Obviously the main focus of our analysis will be those who switched to the separate screen and those who finished on the separate screen when time expired.

⁹ To reiterate, the statistical results in this paragraph compare the high payoff “ambiguity” subjects with the (high payoff) “credibility” subjects.

Treatment	Number of Subjects	Number Ended on Separate	Percentage Ended on Separate	Number Switched to Separate	Percentage Switched to Separate
Low Payment (92 Seconds)	15	1	6.7%	1	6.7%
Low Payment (215 Seconds)	15	4	26.7%	7	46.7%
Low Payment (491 Seconds)	15	8	53.3%	8	53.3%
High Payment (92 Seconds)	15	0	0%	2	13.3%
High Payment (215 Seconds)	12	2	16.7%	5	41.7%
High Payment (491 Seconds)	15	4	26.7%	7	46.7%
Credibility (92 Seconds)	8	3	37.5%	3	37.5%
Credibility (215 Seconds)	4	3	75%	3	75%
Credibility (491 Seconds)	7	7	100%	7	100%

TABLE 2: Experimental Results by Treatment

Given the descriptive statistics above we see that the ambiguity present in the two treatments with the banquet host appears to have an important impact in reducing the likelihood of subjects ending the experiment on the separate screen. We examine this more closely in Figure 3, which standardizes the comparison upon the two high payoff conditions.

Figure 3 About Here

A statistical test on difference in proportions ($z = 4.35$) between the ambiguity and credibility sessions in the high payment condition demonstrates that the obvious difference between treatments is not an optical illusion.¹⁰ Because the 92 second condition may contain some individuals who, given their ability, should not switch, we can conduct the some test including only the subjects from the 215 and 491 second conditions. We obtain a similar result ($z = 3.84$).

These results indicate that ambiguity aversion is alive and well in the sessions in which the information about the value of being on the separate screen is conveyed by a banquet host. However, some individuals in the ambiguity condition do make the switch to the separate screen, and some also end on the separate screen. Before conducting a logit analysis on what factors of the economic environment explain invitee decisions to switch or end on the separate screen we must define our variables.

End: Subject ended the experiment on the separate screen.

Switch: Subject was at any time logged onto the separate screen.

Time: The length of the experiment.

Ability: The number of correctly completed tasks per minute in the paid practice period.

Opportunity Cost: The ability of the individual subject (as measured by his per-minute completion of tasks in the paid practice period) multiplied by earnings per completion of task.

Each of the variables above is either set by us as a parameter or directly observed in the sessions. From these, we can construct a simple model of subject choices (either *ending*

¹⁰ This z statistic is adjusted for the slightly different proportions of stopping times in the two treatments. The raw statistic is virtually identical ($z = 4.23$).

on the separate screen or *switching* at any point during the session) as follows. The probability of (ending, switching) is defined by:

$$\beta_0 + \beta_1 * Time + \beta_2 * Opportunity\ Cost + \beta_3 * Ability$$

Implicit in this construction is the fact that, even though these individuals are not switching as frequently as expected as in the case with complete credibility, the same underlying economic factors, time available and opportunity earnings, are at work. We include *ability* as a separate variable as a measure of an individual's skill with the alpha-numeric calculations. We believe that their ability in one task might represent a good proxy for ability to solve the optimal switching problem. The results of the logit analysis are presented in Table 3.

Explanatory/Dependent	End	Switch
<i>Constant</i>	-2.792 (.048)**	-1.681 (.143)
<i>Time</i>	.006 (.002)***	.004 (.005)***
<i>Opportunity Cost</i>	-2.886 (.040)**	-.730 (.497)
<i>Ability</i>	.175 (.285)	.044 (.757)
<i>Pseudo R-Square</i>	.175	.080

(P-Values are in parentheses. Significance levels * = .10, ** = .05, *** = .01)

Table 3: Logit Analysis With a Structural Model

These results indicate that the components of the structural model predict rather well the probability that an individual ends up on the separate screen. All of the coefficients have the anticipated sign, and all but *ability* are statistically significant. The model does not work as well at predicting those individuals who ever switched to the separate screen.

To this point, only individual explanatory variables have been included in the model. Closer examination of the communication between participants may reveal relational explanations about what drives subjects to end the experiment on the separate screen. The chat was coded along several dimensions. For example: “Were invitees given the actual value of being on the separate screen?” “Did the invitees indicate that they didn't know whether they could trust the host?” Appendix II contains a list of the coded variables, their definitions, and the method used in the coding. We selected the following dummy variables as being potentially significant in their ability to help explain the results:

Value: The host revealed the true \$15 value of being on the separate screen at the end of the experiment.

Trust: The invitee communicated that she did not know whether the host was telling them the truth.

Tunnel Vision: This was not a variable constructed from what was said. Rather, this variable was constructed by a lack of communication. If, in the 215 and 491 second treatments, the host communicated to the invitees but the invitees did not respond they were coded as having "tunnel vision".

Never Lie: The host responds to the question of “trust” by the invitee with a statement along the lines of “I would never lie to you”.

Justification: The host proactively communicates his moral hazard problem to the invitee and seeks to justify his representations in the presence of the experimental incentives.

Table 4 presents the results of two logit regressions for each of the *end* and *switch* dependent variables. The first includes all three of the coded variables described above as regressors; the second drops those that showed no explanatory power. (We kept the *value* variable because it seemed structurally imperative to control for those subjects who were never told by their banquet hosts that the value of ending on the separate screen was \$15).

Explanatory/Dependent	End	End	Switch	Switch
<i>Constant</i>	-4.215 (.014)***	-4.130 (.014)***	-2.760 (.042)**	-2.540 (.050)**
<i>Time</i>	.006 (.026)**	.006 (.011)***	.006 (.006)***	.006 (.002)***
<i>Opportunity Cost</i>	-4.225 (.018)**	-4.152 (.014)***	-.605 (.639)	-1.174 (.333)
<i>Ability</i>	.398 (.046)**	.384 (.048)**	.155 (.354)	.168 (.287)
<i>Value</i>	1.083 (.211)	1.030 (.208)	-.066 (.922)	.027 (.965)
<i>Tunnel Vision</i>	-3.173 (.024)**	-3.210 (.011)***	-2.886 (.006)***	-2.860 (.002)***
<i>Trust</i>	.265 (.829)		.987 (.433)	
<i>Never Lie</i>	-.355 (.750)		-.868 (.464)	
<i>Justification</i>	.133 (.908)		-1.571 (.142)	
<i>Pseudo R-Square</i>	.338	.337	.244	.213

(P-values in parentheses.) Statistical significance: * = .10, ** = .05, *** = .01)

Table 4: Logit Analysis With Expanded Models

Among the variables derived from the chat communication, the most remarkable observation is the significance of "tunnel vision" in both estimations. We do not know the causal relationship, but there is certainly a strong correlation between ignoring the communication from the host and staying with the known payment per completion. Another notable observation is that when including variables from the chat communication, "ability" from the structural model (with dependent variable "end") becomes significant with the expected positive sign. "Opportunity cost", which is constructed by "ability" and payment rate, shows up as significant and with the expected negative sign.

The significant coefficients on “*time*” and “*opportunity cost*” suggest the power of a standard rational choice framework.¹¹ The positive coefficient of “*ability*” as a separate effect may intimate a deeper understanding of the optimal switching problem. Finally, “*trust*” (that is, whether the subject ever inquired as to whether he or she could trust the banquet host) is not significant in its explanatory power. Why is this? This suggests that there is no underlying difference between those people that verbalize their uncertainty with the host and those who do not.

IV. DISCUSSION AND CONCLUSIONS

This experimental partnership with religious studies sought to peel some testable propositions from a complex and layered story about the End Times Messianic Banquet. If there is a beneficial event, to which people have been extended an invitation, what factors influence their decision to attend? Perhaps the credibility of the banquet host with respect to the benefit of the event would matter. Likewise, the opportunity cost of the invitee could be significant in determining her choice. In fact, these were the two central treatments of this experiment.

In the ambiguity treatments nearly 22% of the invitees ended the experiment on the separate screen. When the ambiguity about the payoff was removed nearly 69% of invitees ended the experiment on the separate screen. There is a significant difference in the decisions made when invitees are operating under certainty as opposed to ambiguity. But, even amongst those invitees that ended the experiment on the separate screen when

¹¹ In retrospect, we realize that the instructions may not have gone far enough in making sure that subjects understood the nature of a uniform distribution. To the extent that they actually believed that extreme times (very early or very late) were less likely, their optimal switching time would be later than predicted, which is consistent with behavior in the sessions with no ambiguity.

ambiguity was present there were common characteristics. On average those invitees had lower opportunity costs, higher abilities on the decoding task, more time to switch, and they responded to the messages from the host.

We conjecture that these characteristics portray a reasonable model of the behavior of those who rejected the invitation of the banquet host in the parable of the Great Banquet. They had doubts about the value of attending, so they preferred those activities with relatively less ambiguity in returns. They were relatively preoccupied with other things in their life, perhaps to the point of simply tuning-out the messages from the host's servants. And, they were those who lacked a deeper understanding of what the choice they were making was ultimately about.

Unlike other research that is often known as the "economics of religion", we have not sought to apply our economic skill sets to modeling denominations, rituals, or behavior of members. Rather, we wish to broaden the horizon of understanding of an important story by analyzing a small slice of human nature in the laboratory.

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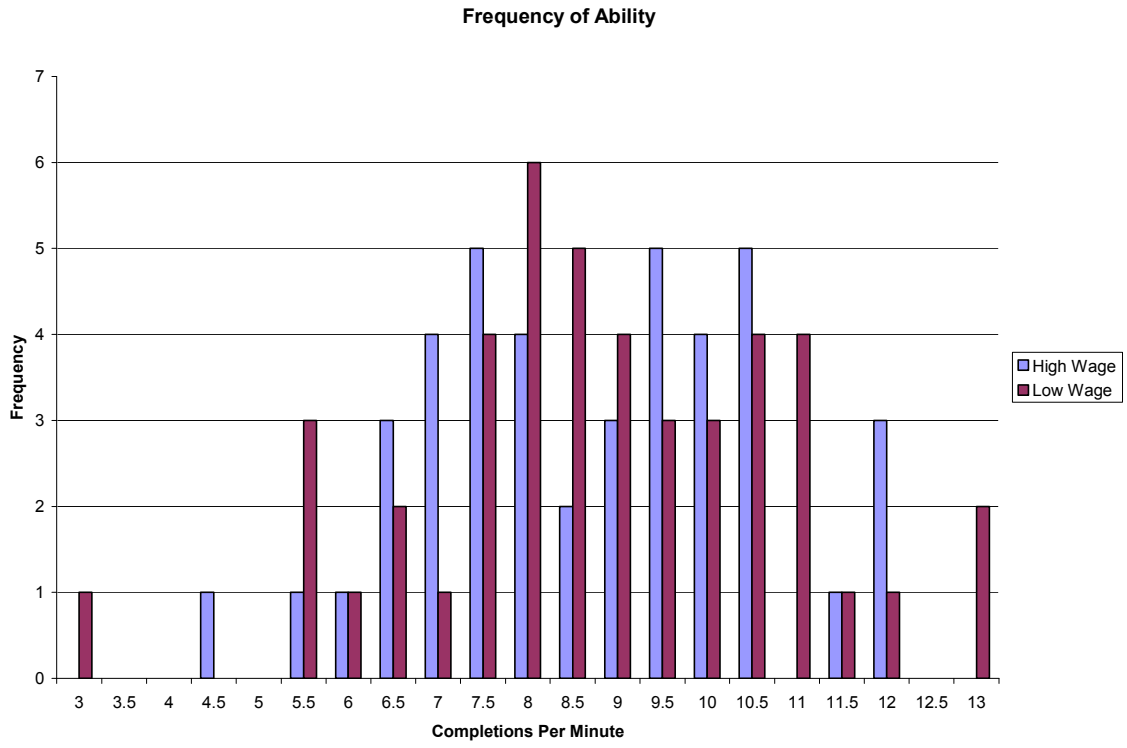


Figure 1

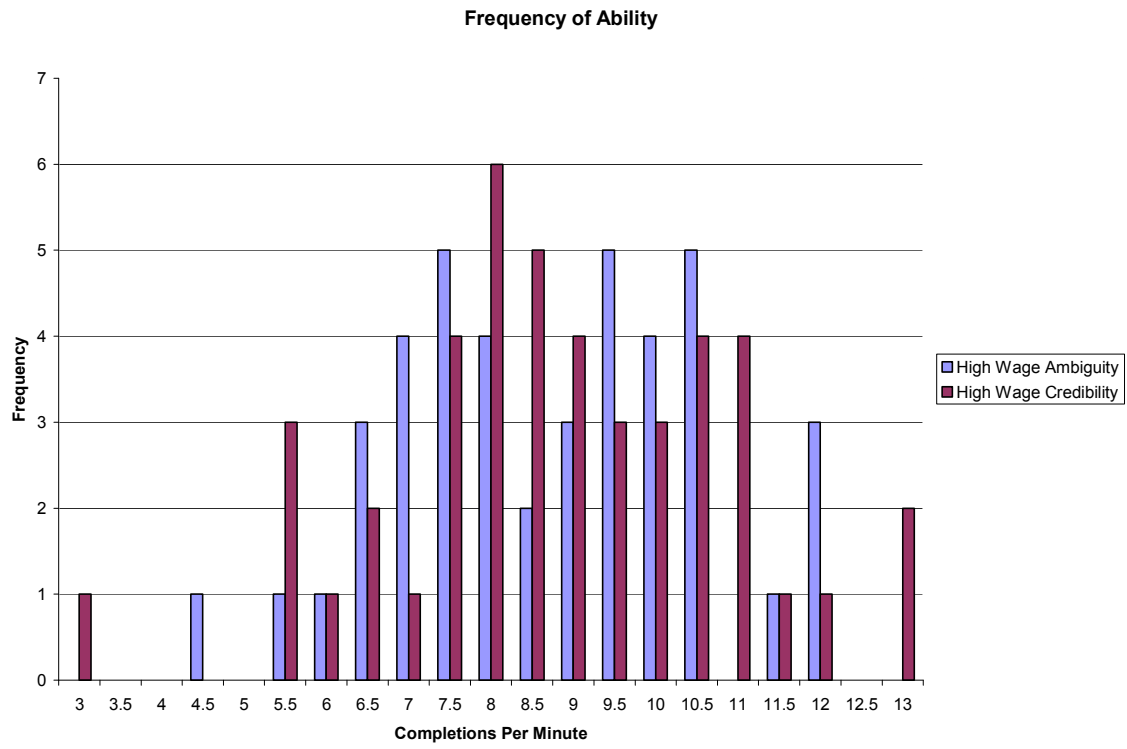


Figure 2

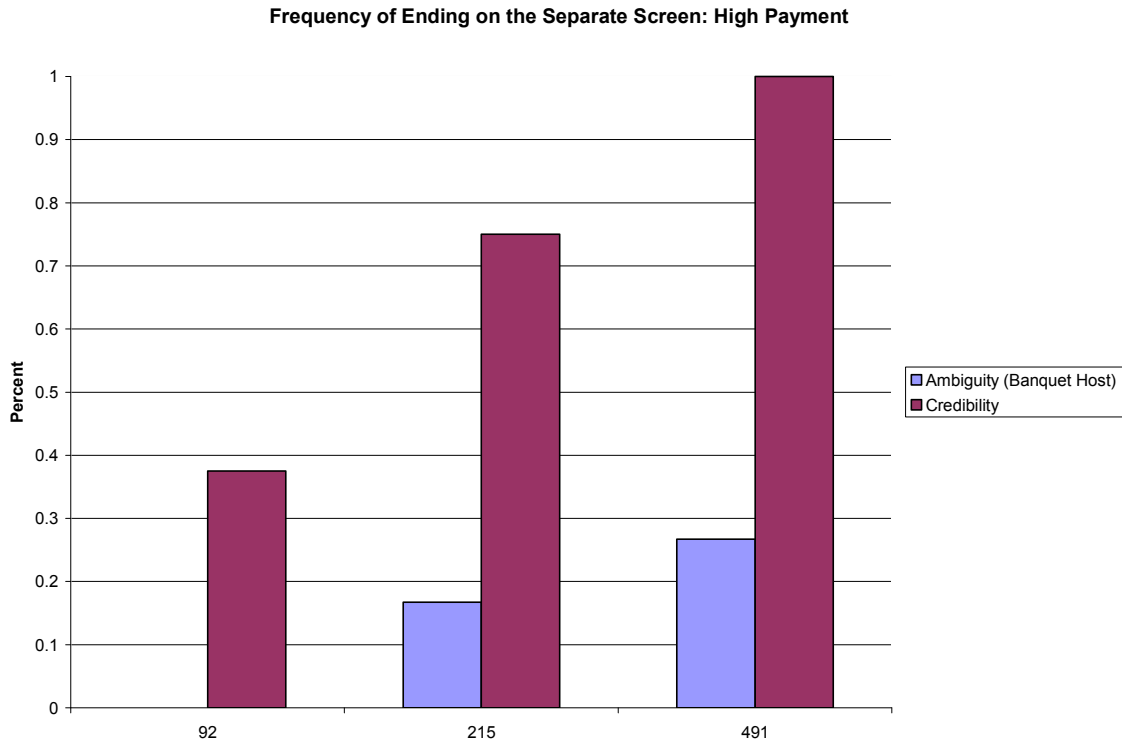


Figure 3

APPENDIX I

The Parable of the Great Banquet:

When one of those who reclined at table with him heard these things, he said to him, "Blessed is everyone who will eat bread in the kingdom of God!" But he said to him, "A man once gave a great banquet and invited many. And at the time for the banquet he sent his servant to say to those who had been invited, 'Come, for everything is now ready.' But they all alike began to make excuses. The first said to him, 'I have bought a field, and I must go out and see it. Please have me excused.' And another said, 'I have bought five yoke of oxen, and I go to examine them. Please have me excused.' And another said 'I have married a wife, and therefore I cannot come.' So the servant came and reported these things to his master. Then the master of the house became angry and said to his servant, 'Go out quickly to the streets and lanes of the city, and bring in the poor and crippled and blind and lame.' And the servant said, 'Sir, what you commanded has been done, and still there is room.' And the master said to the servant, 'Go out to the highways and hedges and compel people to come in, that my house may be filled. For I tell you, none of those men who were invited shall taste my banquet.' "

The Holy Bible: English Standard Version (Crossway Bibles, 2001), as transcribed on BibleGateway.com.

APPENDIX II

The following was our original coding protocol.

Observations on invitees

Observations on messages received by invitees from hosts.

- 1) 1 if invitee receives a direct statement of V (value to separate screen) and the initial observation on V is CORRECT (e.g. V = \$15.00).
- 2) 1 if invitee receives any initial representation of V that is FALSE Examples: V= \$20.
V > \$16.00.
- 3) 1 if condition 2) above is met (statement of V initially received is FALSE) and if the invitee then receives a correcting statement on V that reveals the host's initial statement to have been false. EXAMPLE: OK, V is really \$15.00.
- 4) 1 if invitee receives from host any other factually or logically FALSE statement.
Examples: "If you stay on the initial screen, you won't receive your \$10 show-up fee".
Or "I get the same payment regardless of whether or not you are on the separate screen."
- 5) 1 if invitee receives from a host a message that says anything disparaging about the task on the initial screen. Examples: boring, tedious, you won't earn very much money.
- 6) 1 if invitee ever receives a message from the host which denies engaging in the moral hazard incentives of the task: "I would never lie to you." "I'll make enough money regardless, so why should I lie to you." "You can trust me to look out for your interests."

Observations on messages sent by the invitees to the hosts.

- 7) 1 if invitee ever sends a message to the host that uses words like "This is all about whether or not I can trust you" "trust" "faith" "good faith" "good intentions", etc.
- 8) 1 if invitee ever specifically mentions the host's moral hazard incentives.
- 9) 1 if invitee ever specifically charges a host with lying, misrepresentation, etc.
- 10) 1 if invitee ever points out to the host that \$0.00 is a possible outcome.
- 11) 1 if invitee engages in tunnel vision, that is, invitee receives a message from the host but never responds in any way.

During the first sessions, we added an additional category which we called "justification" which existed if an invitee received a message from a host that represented reasons (other

than the “I would never lie to you” code of number 6, above) that the host would not engage in opportunistic behavior.

The coding was done independently by each of the two authors and a graduate student. The coding results (0 or 1) were entered into a spreadsheet. The three coders then met to discuss the non-unanimous cells. In most of these cases a consensus was reached. In the few cases in which a consensus could not be formed, majority rule prevailed.

APPENDIX III: AMBIGUITY (WITH BANQUET HOST) INSTRUCTIONS

Introduction

This is an experiment in the economics of decision making. If you follow the instructions carefully you have an opportunity to earn some money beyond the participation fee. From this time forward I ask that you do not talk to anyone. And, please make sure that your cell phones are turned off. If you do have any questions please raise your hand and one of the experimenters will assist you.

You are one of four participants in an experimental group. Even though there may be more than four people in the computer lab, there are no more than four people per group. The role of each participant is assigned at random and there are two possible roles. Three group members will have the same role (Participants A, B, and C) while the other group member (Participant D) will have a different role. The following paragraphs will describe the two possible roles. Your role in the experiment (whether you are Participant A, B, C, or D) will appear in the lower left corner of your computer screen.

How will Participants A, B, and C earn money?

Participants A, B, and C have the opportunity to earn money in two ways. They will be discussed in turn. First, Participants A, B, and C to earn money is by correctly typing how many numbers appear in a string of 20 randomly generated letters and numbers. For example,

E A 1 B C 2 P Q 3 4 6 7 9 Q R S 9 T U V

may appear on the screen. Participants A, B, and C would count how many numbers are within the given string (in this case 8) and type that number into the appropriate box and click on the “submit” button. Then the computer will respond “correct” or “incorrect” and another random string will appear on the screen. Each participant’s earnings are independent from the typing speed of other participants. This is an individual task. For each correct answer a participant completes he or she will receive 10 cents.

Secondly, Participants A, B, and C may or may not earn money on a different screen called the separate screen. Any time during the experiment Participants A, B, and C may click the “switch” button at the top of their task screen to leave that screen and enter into the separate screen. Unlike the task screen participants can play tic-tac-toe. But, playing tic-tac-toe does not earn Participants A, B, and C money.

Participants are able to switch between the separate screen and task screen as often as they choose. Each participant keeps their earnings from tasks already completed and cannot lose money already earned by switching. But, once again, you cannot earn money by playing tic-tac-toe.

Participants will receive a payoff to the separate screen, if and only if, they are on the separate screen at the end of the experiment. The payoff to the separate screen can be any non-negative number (note: this also includes zero). But, the payoff will be the same for anybody who finishes the experiment on the separate screen at the end of the experiment.

The length of the experiment was determined by a random number generator between 0 and 900 seconds prior to the start of the experiment. The envelope taped to the whiteboard contains the randomly determined number of seconds. Nothing you do in the practice rounds or experiment will change the time.

Again, Participants A, B, and C can earn money either by correctly typing how many numbers are in a random string of 20 letters and numbers and/or being on the separate screen at the end of the experiment.

Are there any questions before we proceed?

Practice

Before discussion about how Participant D will earn money there will be two practice periods. The first practice period is unpaid. The purpose of this practice period is to familiarize each of you with the interface of the experiment. This practice period will last for 1 minute. Begin now.

The second practice period is a paid practice period. This practice period is 2 minutes in length. All participants, even Participant D who has a different task in the main experiment, will perform the typing task during the practice period. The amount paid per task completed is the same as the main experiment: ten cents. Begin now.

How will Participant D earn money?

Participant D will communicate individually with Participants A, B, and C via electronic messaging to invite them to the separate screen. For each participant that ends the experiment on the separate screen Participant D earns 5 US Dollars. For each participant that does not end the experiment on the separate screen Participant D will still receive 2.50 US Dollars.

Participant D is told the size of the payoff to Participants A, B, and C of being on the separate screen at the end of the experiment. Note also, the experimenter will not verify whether Participant D is correctly or incorrectly representing the separate screen payoff in their electronic messages.

Are there any questions before we proceed?

Mechanics of the Chat Feature

To utilize the chat feature click the text line, type your message, and press the “Enter” button. This will send the message. Note, the submit button serves a different function. When a message has been typed to you an exclamation mark will appear to indicate that there is a message awaiting response.

Summary

In summary, there are two possible roles for participants. Participants A, B, and C earn 10 cents for each correctly completed typing task and/or some non-negative number (zero is included as a possible non-negative number) by ending the experiment on the separate screen.

Participant D uses the chat feature to invite other participants to the separate screen. Participant D earns 5 US Dollars for each participant that ends the experiment on the separate screen and receives 2.50 US Dollars for each participant that does not end the experiment on the separate screen. Participant D is told by the experimenter about the size of the payoff to Participants A, B, and C, but, the experimenter will not verify whether Participant D is correctly or incorrectly representing the information.

The end of the experiment was determined by a random number generator between 0 and 900 seconds prior to the start of the experiment.

Are there any final questions before the experiment begins?

APPENDIX IV: INDIVIDUAL CHOICE INSTRUCTIONS

Introduction

This is an experiment in the economics of decision making. If you follow the instructions carefully you have an opportunity to earn some money beyond the participation fee. From this time forward I ask that you do not talk to anyone. And, please make sure that your cell phones are turned off. If you do have any questions please raise your hand and one of the experimenters will assist you.

This is an individual choice experiment in which participants have the opportunity to earn money in two ways. The first opportunity to earn money involves correctly typing how many numbers appear in a string of 20 randomly generated letters and numbers. For example,

E A 1 B C 2 P Q 3 4 6 7 9 Q R S 9 T U V

may appear on the screen. Then, participants would count how many numbers are within the given string (in this case 8) and type that number into the appropriate box and click on the “submit” button. Then the computer will respond “correct” or “incorrect” and another random string will appear on the screen. Each participant’s earnings in the experiment are independent from the typing speed of other participants. This is an individual task. For each correct answer a participant completes he or she will receive 10 cents.

The second opportunity to earn money is by being on the separate screen at the end of the experiment. Any time during the experiment participants may click the “switch” button at the top of their task screens to leave that screen and enter into the separate screen. Unlike the task screen participants can play tic-tac-toe. But, playing tic-tac-toe does not earn participants money.

All participants are able to switch between the separate screen and task screen as often as they choose. Each participant keeps their earnings from tasks already completed and cannot lose money already earned by switching. But, once again, participants cannot earn money by playing tic-tac-toe.

Participants will receive a payoff to the separate screen, if and only if, they are on the separate screen at the end of the experiment. The payoff to the separate screen will be \$15. This is the same for anybody who finishes the experiment on the separate screen.

The length of the experiment was determined by a random number generator between 0 and 900 seconds prior to the start of the experiment. The envelope taped to the whiteboard contains the randomly determined number of seconds. Nothing you do in the practice rounds or experiment will change the time.

Again, participants earn money either by correctly typing how many numbers are in a random string of 20 letters and numbers and/or being on the separate screen at the end of the experiment.

Are there any questions before we proceed?

Practice

The first practice period is unpaid. The purpose of this practice period is to familiarize each of you with the interface of the experiment. This practice period will last for 1 minute. Begin now.

The second practice period is a paid practice period. This practice period is 2 minutes in length. The amount paid per task completed is the same as the main experiment: ten cents. Begin now.

Summary

In summary, there are two possible ways to earn money for participants. Participants will earn 10 cents for each correctly completed typing task and/or \$15 by ending the experiment on the separate screen.

The end of the experiment was determined by a random number generator between 0 and 900 seconds prior to the start of the experiment.

Are there any final questions before the experiment begins?