Delegated bargaining in a competitive agent market: An experimental study

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Abstract

We examine a variant of ultimatum bargaining in which principals may delegate their proposal decision to agents hired from a competitive market. Contrary to several prior studies, we find that when principals must use agents, the resulting proposals are significantly higher than when principals make proposals themselves. In reconciling our results with prior findings, we conclude that both the rejection power afforded to responders and the structure of principal-agent contracts can play significant roles in the nature of outcomes under delegated bargaining.

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1. Introduction

Many decisions in economic and organizational settings are delegated, making it important to understand how delegation influences outcomes. In this paper, we investigate changes in bargaining outcomes when principals hire agents for a fixed wage in a competitive agent market. Fershtman and Gneezy (2001, henceforth FG) previously examined delegated ultimatum bargaining using explicit incentive contracts tying the agent's earnings to those of the principal. FG find that delegation under such a contract leads to lower proposals and lower conditional rejection rates; specifically, they find that responders are less likely to reject low proposals. FG conclude that one reason for this is that rejection would also harm the agent's earnings. Thus, the explicit incentive contracts appear to allow principals to hold the agent "hostage."

The above work leaves open the question of whether different contract types will yield results similar to those observed by FG. For example, does a contract that does not directly reward the agent for maximizing the principal's earnings also produce lower proposals than under no delegation? We explore this issue in an ultimatum game by adopting the competitive fixed-wage agent market used in Hamman, Loewenstein, and Weber (2010, henceforth HLW). In HLW's agent market, each agent signals his intention (the amount he would propose to the responder) with a non-binding message, and each principal selects an agent based on the messages. Each agent receives a fixed wage when hired by a principal, thus incentivizing agents to be hired, rather than to make any specific kind of proposal.

It is not straightforward to predict the outcome of delegated bargaining in our setting. In the absence of an explicit incentive contract that ties agents' earnings to those of principals, responders could be more willing to reject low proposals, since doing so will not harm the agents' earnings. This could eliminate the negative effect of delegated bargaining on proposals, or even lead to higher proposals.

On the other hand, it is possible that delegated bargaining will yield lower proposals and rejections, as in FG. Even though there is no explicit incentive contract, HLW show that competition to be re-hired in the agent market is sufficient to drive agents to make low transfers in a dictator game. In addition, responders may be more reluctant to reject low proposals because delegation leads to indirect interaction between principals and responders. Prior studies show that removal of principals from decision making often reduces the degree to which they are blamed and punished for resulting outcomes (Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013).

In contrast to the results of both FG and HLW, we find that proposals are significantly higher when principals must delegate the proposal decision to agents. We attribute this finding to two factors: (1) principals do not always select those agents who signal the lowest proposal, as they did in HLW, and (2) agents do not always stick to their message: once selected they often propose more than they signalled. As a consequence, responders' earnings are significantly higher when the proposal decision is delegated.

We also find that when the use of agents is optional, principals choose to delegate 38% of the time. Since agent use does not improve the outcomes for principals, it is not surprising that many principals forgo this option.[1]

2. Experimental Design

[1] FG find that principals using delegation are significantly better off. While we also find that principals earn more under compulsory delegation, mainly due to lower rejections, the difference is not significant.

All sessions were conducted at the xs/fs laboratory at Florida State University. Subjects were recruited by email from a student database using ORSEE (Greiner 2015). Each subject received a $10 show-up fee plus earnings from the experiment. Sessions lasted 45-60 minutes and average total earnings were $16.80 per subject. The experiment was conducted using z-Tree (Fischbacher, 2007).

Each session consisted of a repeated ultimatum game over 12 rounds, with random matching and re-matching between six principals and six responders. Roles were assigned randomly and subjects were seated by role. Once assigned, each subject's role remained the same. The experimenter read instructions (see Appendix) aloud and answered procedural questions publicly. Each subject received a randomly assigned ID number, which was fixed throughout the experiment, to preserve anonymity.[2]

The experiment had three conditions: the *baseline ultimatum game* (UG), an *ultimatum game with agent* (UGA), where principals used an agent, and an *ultimatum game with agent choice* (UGAC), where principals had the option of using an agent or making the proposal to the responder directly. In all three conditions, in each round, $14 was divided, in ten-cent increments, between a principal and a matched responder.

Each session of UG consisted of six principals and six responders. In each round, principals saw the ID of the matched responder before making proposals. Once all proposals were made, responders saw the proposal and the matched principal's ID on their screen, and

[2] This fixed role, fixed ID procedure is identical to that used in HLW. Therefore any differences in outcomes between our findings and HLW are likely to be due to responders' rejection power. In FG, each subject was given a fixed ID, but their roles were not fixed and each subject only played the delegation game once, after playing a standard ultimatum game. While our general procedures differ from FG, our primary interest is in comparing the effects of delegation within a given environment. Hence, as with FG, we compare environments with and without delegation, with the major substantive difference in our designs being delegation through a fixed-wage competitive agent market in our study and explicit incentive contracts in FG. We cautiously attribute differences in results to the contract type. While we believe it unlikely, we cannot rule out that other differences between the designs may also play a role in outcome differences.

chose whether to accept or reject. Acceptance implemented the proposal, while rejection yielded zero payoffs for both players.

In UGA, principals were required to hire an agent, so in addition to six principals and six responders, three additional subjects had the role of agent. Each round started with each agent sending a non-binding message to all principals indicating the amount he intended to propose to the responder if selected. Once principals saw all three agents' messages, each selected an agent. While the principals saw all messages, each agent only knew his own message. Once selected by a principal, each agent learned the ID of the responder matched to the principal before making a proposal. Note that if an agent was hired by three principals, he made separate proposals to three different responders. After all agents entered their proposals, responders saw their proposals and the IDs of their matched principal and the agent who sent the proposal. Each responder then decided whether to accept or reject the proposal. As in UG, the responders' decision determined the payoff for the proposer-responder pair. Responders knew that agents received a fixed wage from the experimenter when hired by a principal.[3]

UGAC differed from UGA only in that, after viewing the agents' messages, principals could select an agent or choose to make the proposal themselves by clicking a "myself" button. Before deciding whether to accept or reject, responders knew whether the proposal was made by their matched principal or by an agent selected by the principal.

In all conditions, at the end of each round, each participant saw a screen summarizing decisions from all parties involved with that participant in that round: the matched counterpart's ID, the proposed amount, the responder's decision to accept or reject, payoffs for the principal

[3] Findings in McDonald, Nikiforakis, Olekalns, and Sibly (2013) suggest that merely introducing a third party may change behavior by shifting the reference group. Our payoff structure helps minimize this potential effect (though perhaps not remove it entirely) due to differences in earnings determination across roles. A principal's effect on an agent's payoff when by choosing that agent is small at $0.30 compared with the $14 endowment.

and responder, and ID of the agent if used. For example, if an agent was selected by three principals, his results screen summarized the decisions made in all three principal-responder pairs; if an agent was not selected in that round, his results screen was empty. No participant saw any information related to decisions not directly involving him- or herself. Participants were encouraged to record all information they received on record sheets before advancing to the next round.

At the end of each session, principals and responders saw a table showing their potential earnings in each round, the one round that was randomly selected to count toward their earnings from the experiment, and their total earnings including the $10 show-up fee. This was the only payment that principals and responders received for the experiment.[4]

Agents in the UGA and UGAC conditions received their total payment summed across all 12 rounds.[5] In each round, agents gained or lost money based solely on the number of principals who selected them. Specifically, agent *i*'s payoffs in a given round depended on the payoff function (identical to HLW),

$$\pi_i = -\$0.60 + \$0.30 n_i, \tag{1}$$

where n_i denotes the number of principals selecting agent i in a round. Agents incurred a fixed cost of $0.60, and received $0.30 for each principal who selected them.[6] In each round, an agent broke even when hired by two principals. This holds the net payoffs among the three agents

[4] See Azrieli, Chambers, and Healy (2015) for a discussion of the merits of this payment procedure.
[5] We follow the payoff function used in HLW to keep total agent pool earnings in each round constant. This minimizes other-regarding preference concerns when comparing UGA to UGAC: principals could not increase total agent earnings by hiring agents more frequently. Preserving constant agent pool earnings in a random round payment system would necessitate either large potential losses (to offset large earnings from frequently-chosen agents) or underincentivization. Our payment method makes the agent's incentives to be chosen salient.
[6] In the instructions, we explained agent payoffs in a slightly different manner. Subjects were told that each agent would receive $0.20 for each principal who selected and lose $0.10 for each principal who did not select them. In the UGAC condition, a principal who chose to make his or her own proposal did not affect any agent's payoffs. In this condition, the agent's payoffs were only affected by those principals who chose to delegate their proposal decision.

constant at $0. To minimize the likelihood of negative accumulated earnings, all agents began with a $6 initial endowment.[7] Agents therefore had incentives to be selected by as many principals as possible, creating a competitive agent market wherein agents benefited from inferring the principals' preferences and signaling accordingly. Agents' earnings did not depend on the amount they proposed to responders (unlike FS and Schotter, Zheng, and Snyder 2000).

At the end of the last round in each session, agents saw their earnings in each round and their total earnings. After all subjects completed a short questionnaire, including several demographic questions, they were paid privately, by check, before leaving the lab.

3. Results

We conducted five sessions per condition totalling 210 subjects. We obtained observations for thirty pairs of principals and responders in each condition, as well as for fifteen agents in each of the UGA and UGAC conditions.

We begin by highlighting the differences between our UG and UGA conditions to see the direct effect of delegation on bargaining outcomes. We then compare these treatments to UGAC in section 3.2.

3.1. Bargaining with and without delegation: Comparing UG and UGA

Figure 1 shows the distribution of proposals in UG and UGA. The height of each bar indicates the frequency of proposals in that range. The bottom portion of each bar indicates the proportion of accepted proposals, whereas the top portion indicates the proportion of rejected proposals.

[7] Knowing that average earnings for principals and responders would be (weakly) less than $7, we used $6 as the initial endowment for the agent so that average earnings across all subject types would be comparable. No agents ended the experiment with negative earnings.

{Insert Figure 1 about here}

Result 1: We observe higher proposals, fewer rejections, and higher responders' earnings in UGA than in UG.

Average proposals were \$5.61 in UG and \$6.28 in UGA. This difference is significant based on two-tailed Mann-Whitney tests using a single session-averaged observation per subject ($p = 0.01$). Table 1 presents estimates of the treatment effect on proposals (first two columns in each panel) and the probability of rejection (right-most columns). Panel A shows the results comparing only UG and UGA. The binary variable *UseAgent* captures the treatment effect of UGA. To control for the lack of independence in our data, we cluster standard errors by subject with round controls (models 1 and 3), and use participants' session averages (models 2 and 4) to compare the treatment effect.[8] The regressions confirm the pairwise tests, with higher proposals in UGA (\$0.67 more than UG).[9]

{Table 1 about here}

As a result of the higher offers in UGA, there are significantly fewer rejections: 60 in UGA (17% of all proposals) versus 85 in UG (24%, $\chi^2_{(1)} = 5.40$, two-tailed p-value = 0.025). However, using logistic regressions with standard errors clustered by responder (model 3 in Table 1), we find that the likelihood of a proposal being rejected in UGA and in UG is not

[8] We also use these models to compare other treatment variables in the paper. In addition, for robustness, we re-estimate all dependent variables using both subject and sessoin random effect models with and without round controls. Our results are robust to these alternative specifications. We report OLS results with standard errors clustered by subject, which were the most conservative results, while the random effects GLS models reduced standard errors for nearly every right-hand-side variable. We also find similar results using a reduced data set that includes only the first time each principal was paired with each responder, i.e. repeated pairings were removed. We additionally compare treatments using one observation per subject in Mann-Whitney tests and find statistically similar differences between treatments.

[9] The 95% confidence interval on our treatment difference extends from 0.16 to 1.18, which means average proposal were from 1% to 8% larger in UGA. The PO game in FG found that proposals were on average 7.8% *lower* under delegation (43.3 to 35.5 out of 100 tokens), which lies well outside our confidence interval.

significantly different after controlling for the proposed amount. Thus, delegation itself does not seem to affect the responders' rejection rates.

Table 2 reports comparable regressions to those in Table 1, but with different dependent variables. Higher proposals in UGA resulted in more accepted proposals and significantly higher average earnings for responders in UGA ($5.48) than in UG ($4.74). However, the use of agents did not benefit principals significantly.[10]

{Table 2 about here}

Result 2: Principals did not choose agents who signaled the lowest proposals.

Excluding rounds in which the principals only had one choice due to identical agent messages, we find that the modal principal choice is the agent who signaled an intent to propose the *most* to responders (44.94% of cases).[11] Principals only selected agents who signaled to propose the least 28.57% of the time, and the median signals were selected 26.49% of the time. If the principals had selected only those agents who signaled to propose the least, the corresponding average proposal in UGA would have been $4.14, significantly lower than the average proposal made by principals in UG.[12]

Result 3: Agents proposed more than they signaled.

[10] Agents' proposals were higher in the last round (see Figure A2 in Appendix). When we re-analyze the data excluding the last round, the results do not change.
[11] This is not driven by principals favoring an equal split signal. Excluding cases of homogeneous signals, when the highest signal was $7 it was chosen 42.6% of the time (69/162).
[12] P-value < 0.001 using regression model with clustered standard errors by principal and with round controls. In HLW, principals tended to select the agent with the lowest message. Since our setting is identical to HLW except for the responders' rejection power, we attribute the sharp contrast in findings to the principals' fear of rejection by responders.

We find that responders receive higher earnings in UGA partly because selected agents often made significantly higher proposals than they signaled in their messages ($p<0.001$ for within subject two-tailed t-test). If agents in UGA had made proposals entirely consistent with their messages, the average proposal would have been \$5.83, significantly lower than the actual average proposal by 7.2% (See Table 2).[13]

These results suggest that the higher average proposals observed in UGA can be attributed both to the principals' willingness to select agents likely to make higher proposals and to agents proposing more than they signaled. Unlike in HLW, agents in our setting did not feel similar pressure to make low proposals to be hired.

Result 4: Principals' decisions to switch agents are driven mainly by prior-round rejections.

We find that principals in UGA switched agents 191 out of 330 possible times (Rounds 2-12). Principals commonly switched agents following rejected proposals. In UGA there were 57 total rejections in rounds 1-11. Principals switched agents in 40 of these cases (70.2%). When proposals were not rejected, principals switched agents significantly less frequently (151 / 273 = 55.3%; $\chi_{(1)} = 4.27$, two-tailed $p < 0.05$).

Using logistic regressions clustered by principal and controlling for round, we estimate the probability of switching in round t (*switch*) as a function of the lagged proposal ($Proposal_{t-1}$) and lagged rejection decision ($Rejected_{t-1}$), reported in model 1 of Table 3.[14] In models 2 and 3, we include variables to investigate whether agents proposing more ($PropMore_{t-1}$) or less ($PropLess_{t-1}$) than their signal to principals affected switch decisions in the next round. Model 3

[13] Principals' proposals in UG are not significantly different from their choices based on the agents' signals; this indicates that agents in the competitive market were able to infer the principals' preference in the absence of explicit communication.

[14] We include the proposed amount in all regressions, since principals may use switching to express their preferences for lower proposals. Such behavior is also found by HLW.

includes interaction terms to investigate principals' switching behavior when agents did not stick to their messages and the proposals were rejected. In model 4, we include a variable (ΔM_t) to control for agents changing their signal from the previous round. We find that rejection in the previous round is the key driver of switching in all models. The effect of agents not sticking to their messages seems to be marginal and appears largely mitigated by agents changing their signals (often to match the amount they previously proposed).

{Table 3 about here}

3.2 Optional delegation: Comparing UG, UGA and UGAC

Result 5: Agents did not improve outcomes for principals and were often not hired when use of an agent was optional.

In UGAC, principals delegated the proposal decision to an agent only 38% of the time and the frequency of delegation decreases significantly over time (See appendix Table A2 and Figure A1). Mann-Whitney tests show that proposals in UGAC are significantly lower than in UGA ($5.31 versus $6.28, two-tailed $p < 0.01$), though there is no significant difference between UG and UGAC ($p = 0.15$). The rejection rate in UGAC was 18%, lying between those in UG and UGA.

We examine UGAC proposals further by separating them into two subgroups: those made by principals (UGAC/*no-agent*) and by agents (UGAC/*agent*). We add the binary variable *OptionA* to regression models used in the previous section, to investigate whether having the option to use an agent and whether using one leads to different bargaining outcomes. *OptionA* equals 1 when the principal has the option of delegating, and 0 otherwise. We also include an interaction term *OptionA* × *UseAgent* for UGAC/agent to capture any incremental effect of principals delegating when they have an option.[15] The results found in UG and UGA extend to inclusion of the subgroups in UGAC; that is, the signs and significance of the coefficients for *UseAgent* do not change in Panel B of Table 1 (models 5 and 6). Principals who proposed to responders directly in UGAC proposed slightly less than those in UG, but the difference is not significant. Responders were less likely to reject a low proposal in UGAC (see models 7 and 8). That is, the fact that responders know that principals have the option of using an agent, rather than whether they actually use an agent, lowers rejection rates. Hence, principals' earnings are slightly, though not significantly, higher when they have the option of using an agent (see Table 2, models 11 and 12).

Principals' decisions to make their own proposals were not simply because agents failed to infer their preferences. When principals did not use an agent, the principals' proposals were within a dollar of at least one agent's signal over half the time (117 out of 223 cases). In 51 cases, the principal proposed an amount equal to at least one agent signal.

4. **Conclusion**

The existing literature on delegation in allocation tasks, including FG and other studies of non-strategic delegation (HLW 2010, Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013) shows that delegation typically improves the outcomes for principals.

We find that such a result is not universal. Specifically, when agents are hired in a competitive market for a fixed wage, delegated bargaining may improve the outcomes for the other party. In our ultimatum game setting, earnings for responders and combined earnings of principals and responders are higher when delegation is compulsory. Our results do suggest

[15] Thus, the matrix (0, 0, 0), (0, 1, 0), (1, 0, 0), and (1, 1, 1) for (*OptionA*, *UseAgent, OptionA* × *UseAgent*) represents UG, UGA, UGAC/no-agent, and UCAG/agent, respectively.

indirect support for FG's conclusion that explicit incentive contracts are key for principals benefiting from delegation, through the ability to hold agents hostage.

Since we use the same competitive agent market as in HLW, the difference in our findings suggests that responders' rejection power is critical to ensure a more favorable outcome for the responders. Unlike Coffman (2011), Bartling and Fischbacher (2012), and Oexl and Grossman (2013), principals in our setting did not use delegation to misbehave: the modal principal choice is the agent who intends to propose the *most* to responders.

Finally, we find that when delegation is optional, the benefits of delegation to responders largely disappear because most principals elect not to hire an agent. Principals see little advantage from using an agent, particularly given the possibility that an agent's proposal may deviate from his non-binding signal. The higher proposals observed when delegation is compulsory are primarily due to agents proposing more to responders than they indicated in their messages to the principal, and these higher proposals largely disappear when principals opt not to use agents.[16]

Our results leave unanswered questions that warrant future study. For instance, why does knowing that principals have a choice of using agents lower responders' rejection rates? There is no evidence that agents were seen as fair arbiters, since the likelihood of rejecting a low proposal does not depend on whether an agent was used. While this result might be spurious, further experiments designed to investigate responders' behavior when confronted with delegated bargaining would be valuable. Another important question involves whether different forms of communication from agents would affect bargaining outcomes. For example, requiring agents to

[16] Moreover, when agents are not hired as frequently, they have fewer opportunities to learn about responders' rejection thresholds and therefore may have less of an information advantage. This also potentially reduces their usefulness to principals.

send only binding commitments of what they intend to propose or to reveal prior proposals they have made could potentially affect principals' selection decisions and the resulting proposals and outcomes. This would contribute to other recent interesting research investigating how the nature of communication interacts with features of a bargaining context to influence outcomes (Kriss, Nagel and Weber, 2013; Baranski and Kagel, 2015).

Broadly, our results indicate that delegation to intermediaries without explicit financial ties to the immediate bargaining outcome does not yield more aggressive and egoistic behavior on behalf of the principal; if anything, it seems to favor the responders. Hence, when contrasted with prior work, our findings suggest that identifying the effects of delegation on outcomes may require looking closely at the features of the context in which delegation occurs, including the nature of contracts employed.

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Table 1: Regressions of Proposals and Rejection likelihood, by treatment

Panel A: Comparing UG to UGA

	Proposal		Rejection	
VARIABLES	(1)[a]	(2)[c]	(3)[b]	(4)[d]
UseAgent	0.668***	0.668**	0.077	-0.045
	(0.254)	(0.254)	(0.368)	(0.045)
Proposal			-0.945***	-0.036
			(0.145)	(0.030)
Round	Yes	--	Yes	--
Constant	5.448***	5.609***	3.694***	0.438**
	(0.421)	(0.180)	(0.863)	(0.172)
Observations	720	60	720	60
Clusters	60		60	
R-squared	0.049	0.106	0.281[f]	0.071

Panel B: Comparing UG, UGA, and UGAC

	Proposal		Rejection	
VARIABLES	(5)[a]	(6)[e]	(7)[b]	(8)[f]
UseAgent	0.668***	0.668**	0.148	-0.012
	(0.253)	(0.255)	(0.398)	(0.043)
OptionAgent	-0.474	-0.376	-0.733*	-0.090**
	(0.390)	(0.341)	(0.433)	(0.042)
*OptA*UseA*	-0.206	-0.429	-0.222	0.0401
	(0.443)	(0.429)	(0.507)	(0.050)
Proposal			-1.097***	-0.085***
			(0.131)	(0.017)
Round	Yes*	--	Yes***	--
Constant	5.330***	5.610***	4.460***	0.715***
	(0.360)	(0.223)	(0.815)	(0.143)
Observations	1,080	114	1,080	120
Clusters	90	90	90	90
R-squared	0.062	0.087	0.322[g]	0.175

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

[a] OLS with proposal as the dependent variable and standard errors clustered by principal;
[b] Logit with rejection as the dependent variable and standard errors clustered by responder;
[c] OLS with one session-avg. observation (average proposal) per principal;
[d] OLS with one session-avg. observation (rejection frequency) per responder; *Proposal* is average proposal;
[e] OLS with one session-avg. observation per principal per treatment variable and standard errors clustered by principal; there are 114 observations because in UGAC, two principals always used agents, four never used agents, and twenty four sometimes make their own proposals and sometimes use an agent;
[f] OLS with one session-avg. observation per responder per treatment variable and standard errors clustered by responder; there are 120 observation because every responder receives proposals from principals and from agents;
[g] Pseudo-R-squared reported for Logit models.

Notes: The *UseAgent* dummy in Panels A and B denotes the treatment effect of UGA. The UGAC treatment in Panel B is identified by both *OptionAgent* and the *OptA*UseA* interaction term.

Table 2: Regression results of treatment effects on additional variables

Panel A: Ultimatum game (UG) and ultimatum game with agent (UGA)

	Intended Proposal		**Principal earnings**		**Responder earnings**		**Total earnings**	
Variables	(1)[a]	(2)[c]	(3)[a]	(4)[c]	(5)[b]	(6)[d]	(7)[a]	(8)[c]
Constant	5.440***	5.610***	4.785***	5.959***	4.529***	4.736***	9.314***	10.694***
	(0.389)	(0.223)	(0.515)	(0.264)	(0.486)	(0.180)	(0.882)	(0.419)
UseAgent	0.216	0.217	0.233	0.233	0.739***	0.739***	0.972*	0.973**
	(0.235)	(0.235)	(0.309)	(0.309)	(0.266)	(0.266)	(0.475)	(0.475)
Round	Yes***	--	Yes***	--	Yes**	--	Yes**	--
Observations	720	60	720	60	720	60	720	60
Clusters	60		60		60		60	
R-squared	0.037	0.014	0.038	0.010	0.037	0.117	0.036	0.067

Panel B: Comparing UG, UGA, and UGAC treatments

	Intended Proposal		**Principal earnings**		**Responder earnings**		**Total earnings**	
Variables	(9)[a]	(10)[e]	(11)[a]	(12)[e]	(13)[b]	(14)[f]	(15)[a]	(16)[e]
Constant	5.450***	5.610***	4.965***	5.958***	4.496***	4.736***	9.461***	10.694***
	(0.338)	(0.223)	(0.459)	(0.264)	(0.395)	(0.259)	(0.761)	(0.420)
OptionA	-0.483	-0.376	0.685	0.801*	-0.172	-0.014	0.512	0.707
	(0.390)	(0.341)	(0.490)	(0.440)	(0.241)	(0.249)	(0.852)	(0.776)
UseAgent	0.216	0.217	0.233	0.233	0.739***	0.739***	0.972**	0.972**
	(0.234)	(0.235)	(0.308)	(0.310)	(0.265)	(0.267)	(0.473)	(0.476)
OptA×UseA	-0.322	-0.484	0.023	-0.619	-0.298	-0.344	-0.275	-1.072
	(0.434)	(0.482)	(0.614)	(0.723)	(0.391)	(0.424)	(1.024)	(1.269)
Round	Yes***	--	Yes***	--	Yes*	--	Yes***	--
Observations	1080	114	1080	114	1080	120	1080	114
Clusters	90	90	90	90	90	90	90	90
R-square	0.054	0.070	0.037	0.031	0.030	0.061	0.028	0.016

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

[a] OLS with standard errors clustered by principal;
[b] OLS with standard errors clustered by responder;
[c] OLS with one session-avg. observation per principal;
[d] OLS with one session-avg. observation per responder;
[e] OLS with one session-avg. observation per principal per treatment variable and standard errors clustered by principal; there are 114 observations because in UGAC, two principals always used agents, four never used agents, and twenty four sometimes make their own proposals and sometimes use an agent;
[f] OLS with one session-avg. observation per responder per treatment variable and standard errors clustered by responder; there are 120 observation because every responder receives proposals from principals and from agents;
Notes:

1. The *UseAgent* dummy in Panels A and B denotes the treatment effect of UGA. The UGAC treatment in Panel B is identified by both *OptionA* and the *OptA*UseA* interaction term.
2. Intended Proposal = proposals made by principals and messages selected by principals if agents were used. Total earnings = principals' earnings + responders' earnings.

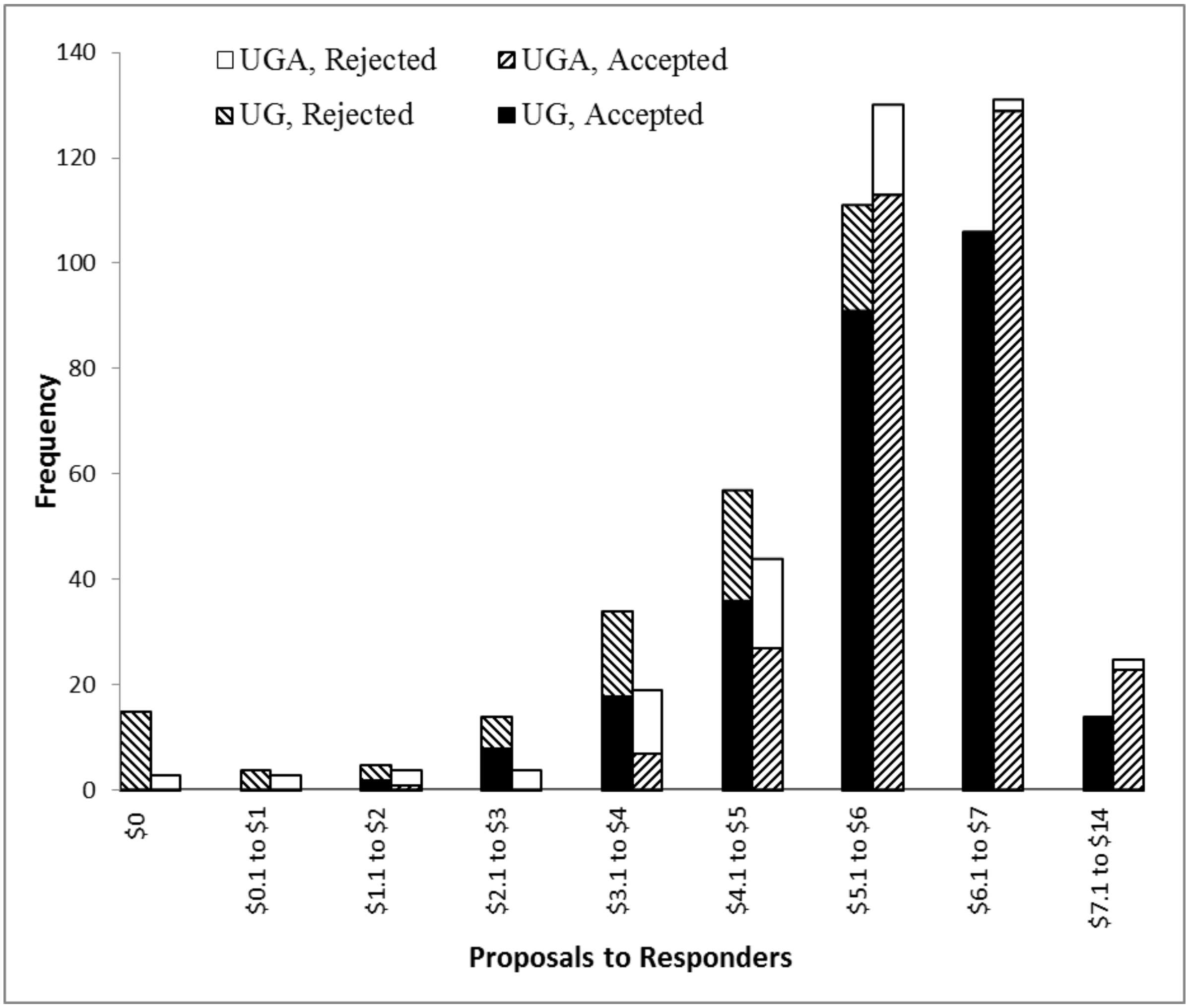
Table 3: Probability of principals switching agent in round t, logistic regressions

VARIABLES	(1) Switch=1	(2) Switch=1	(3) Switch=1	(4) Switch=1
$Proposal_{t-1}$	0.281***	0.201**	0.221**	0.086
	(0.102)	(0.100)	(0.109)	(0.075)
$Rejected_{t-1}$	1.112***	1.044***	1.016***	1.006**
	(0.394)	(0.368)	(0.376)	(0.453)
$PropMore_{t-1}$		0.737*	0.679*	0.760
		(0.446)	(0.403)	(0.552)
$PropLess_{t-1}$		0.560	0.837	-0.179
		(0.491)	(0.696)	(1.120)
PropMore × Rejected			0.357	-0.545
			(1.445)	(1.540)
PropLess × Rejected			0.000	0.000
			(0.000)	(0.000)
Proposal × Rejected			0.022	0.035
			(0.071)	(0.087)
ΔM_t				-0.193*
				(0.116)
Round	-0.067*	-0.060	-0.065*	-0.094*
	(0.039)	(0.039)	(0.038)	(0.050)
Constant	-1.114	-0.785	-0.869	0.181
	(0.792)	(0.751)	(0.740)	(0.646)
Observations	330	330	328	263
Clusters	30	30	30	24

Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Notes: All models are logit regressions with standard error clustered at principal level. The dependent variable is whether the principal switched agents in the current round or not. *PropMore* = 1 if proposal > agent's message to principals and 0 otherwise; *PropLess* = 1 if proposal < agent's message to principals and 0 otherwise; ΔM_t = [agent's message this round] – [the same agent's message last round].

Figure 1: Distribution of Proposals in UG and UGA Conditions



Online Appendix A: Additional Statistical Analyses

Table A1: Principals' switching behavior

Panel A: Grouped by whether agents stick to their messages

	Proposal > Message	Proposal = Message	Proposal < Message
Switch	39	143	9
Not switch	12	123	4
Last round	6	24	0
Percentage Switch	76.5%	53.8%	69.2%
Chi Square, compared to Proposal = message	$\chi_{(1)} = 9.03^{***}$		$\chi_{(1)} = 1.20$

Panel B: Grouped by whether the proposal was rejected

	Proposal rejected	Proposal not rejected
Switch	40	151
Not switch	17	122
Last round	3	27
Percentage Switch	70.2%	55.3%
Chi Square, compared to Proposal not rejected	$\chi_{(1)} = 4.27^{*}$	

Table A2: Delegation over time in UGAC

VARIABLES	(1) UseAgent
Round	-0.101***
	(0.030)
Constant	0.156
	(0.264)
Observations	360

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Logit with likelihood of using an agent as dependent variable and standard errors clustered by principal. Round is used as a discrete variable to capture changes in delegation over time.

Figure A1: Percentage of agent use over time, UGAC

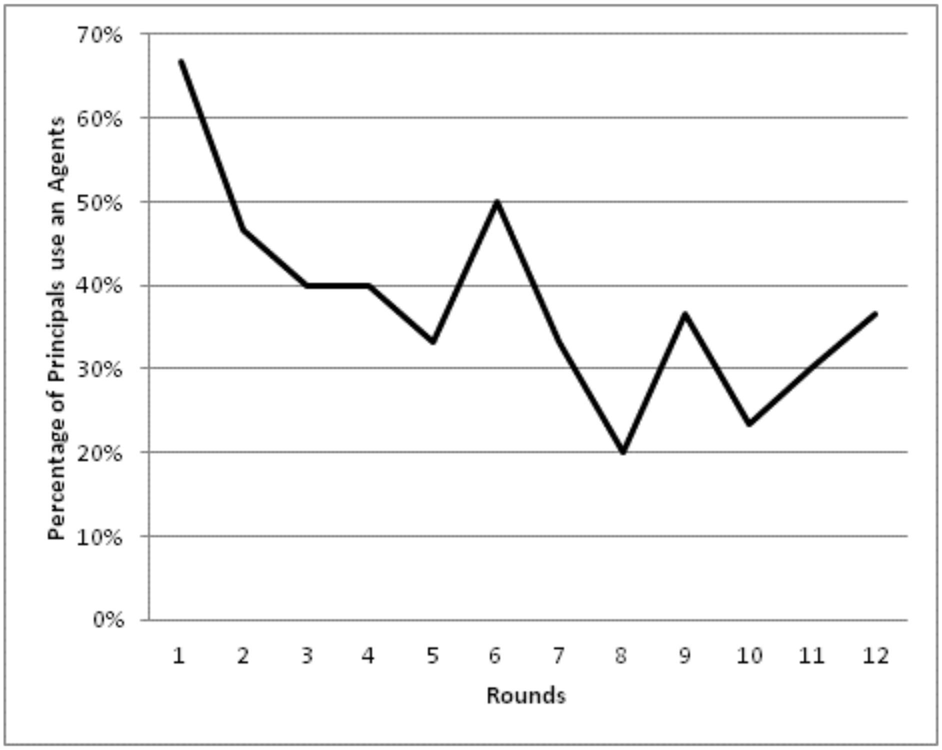


Figure A2: Average Proposals over time