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OPTIMAL CONTRACTS WITH TEAM PRODUCTION AND HIDDEN INFORMATION: AN EXPERIMENT

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Abstract: It is standard in agency theory to search for incentive-compatible mechanisms on the assumption that people care only about their own material wealth. Yet it may be useful to consider social forces in mechanism design and contract theory. We devise an experiment to explore optimal contracts in a hidden information context. A principal offers one of three possible contract menus to a team of two agents of unknown types. We observe numerous rejections of the more lopsided menus, and approach an equilibrium where one of the more equitable menus is proposed and agents accept a contract, selecting actions according to their types. The consensus menu differs across treatments that vary the payoffs resulting from a rejection. We find that an agent is more likely to reject a contract menu if her teammate rejected a contract menu in the previous period, suggesting that agents may be learning social norms; in addition, low-ability agents have a particularly adverse reaction to reduced wage offers.

Keywords: Experiment, Hidden Information, Optimal contract, Production Team, Wage Rigidity

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1. INTRODUCTION

The classic 'lemons' paper (Akerlof 1970) illustrated the point that asymmetric information led to economic inefficiency, and could even destroy an efficient market. Since the seminal works of Vickrey (1961) and Mirrlees (1971), research on mechanism design has sought ways to minimize or eliminate this problem.[1] In an environment with *hidden information* (sometimes characterized as adverse selection), each agent knows more about her[2] 'type' than the principal does at the time of contracting. In the standard scenario, a firm hires a worker but knows less than the worker does about her innate work disutility. Other typical applications include a monopolist who is trying to price discriminate between buyers with different (privately known) willingness to pay, or a regulator who wants to obtain the highest efficient output from a utility company with private information about its cost.[3]

It has long been standard in agency theory to search for incentive-compatible mechanisms on the assumption that people care only about their own material wealth. However, while this assumption is a useful point of departure for a theoretical examination, economic interactions frequently are associated with social approval or disapproval. In dozens of experiments, many people appear to be motivated by some form of social preferences, such as altruism, difference aversion, or reciprocity. Recently, contract theorists such as Casadesus-Masanell (2004), Rob and Zemsky (2002), and MacLeod (2003) have expressed the view that contract theory could be made more descriptive and effective by incorporating some form of behavioral considerations into the analysis.

[1] Applications include public and regulatory economics (Laffont and Tirole 1993), labor economics (Weiss 1991, Lazear 1997), financial economics (Freixas and Rochet 1997), business management (Milgrom and Roberts 1992), and development economics (Ray 1998).
[2] Throughout this paper we assume that the principal is male and the agents are female.
[3] One-shot contracts are common in consumer transactions. In the public sector, government procurement is often conducted on a one-shot basis.

In this context, we conduct an experimental test of optimal contracts with hidden information. Our aim is to gather evidence about the determinants of behavior that could lead to a better understanding of how work motivation and performance are linked, and to thereby improve these through more effective contract and employment choices. In our design, there are two types of agents and it is common information that these types are equally prevalent. A principal selects one of three menus, each having two possible contracts, to a pair of agents of unknown types. Each individual agent, who knows her own type and the menus available to the principal, then independently selects one of the two contracts offered on the menu or rejects both. Pecuniary incentive-compatibility separates the types' optimal choices for every menu and no rejections should ever be observed. The menus are ranked with respect to how much they favor the principal.

If both agents accept a contract, the contracts are implemented; if either agent rejects, both the agents and the principal receive symmetric reservation payoffs (a treatment variable). By introducing contracts that must be accepted by both workers, we contemplate the common situation where contracts must be negotiated with a union and then approved by the workers.[4] Besides this feature, our environment (with 3-person groups and interactive preferences) leads to a more natural and realistic structure for the way in which subjects receive feedback, without (we will argue) otherwise distorting the contractual environment.

As people frequently do not act as pure money-maximizers in experiments, there is the immediate conjecture that the usual theoretical predictions will be rejected. However, the pattern of any such rejections should be informative. Interesting questions include the 'equilibrium'

[4] In essentially all of Europe, collective bargaining involving trade unions covers more than 75% of all workers (Layard, Nickell and Jackman 1994). Our design assumes that a contract structure that affects all workers needs to be approved by a supermajority rule.

contract menu (if any), whether there is a separation by type, and whether the level of the reservation payoffs affects behavior.[5] In our data we observe that whether or not the different types of agents get substantial rents (as well as the size of these rents) depends crucially on the available reservation payoff; this should not be true under the standard theory.

We observe that principals usually initially propose the theoretically-predicted contract, although it is intriguing that this is significantly more likely in the treatment with higher reservation payoffs. When these early-period contracts are rejected sufficiently often (how often depends very much on the individuals and on the reservation payoffs), the principals who were offering them instead choose progressively less self-favorable alternatives, until rejections cease and an 'equilibrium' menu is reached. This menu differs across the two treatments.

We calculate *ex post* optimal contract menus and expected receipts for firms using our estimated parameters for the Fehr and Schmidt (1999) model of utility. We also identify other factors that influence an agent's decision to reject a contract menu, such as whether the other paired agent rejected the menu in the previous period and whether the menu is less favorable than the one offered in the previous period. Finally, we analyze principal behavior, estimating prior beliefs, *ex ante* differences in expected utility for different menus, and discuss the evolution of menu choices in each treatment.

The remainder of the paper is organized as follows: Section 2 offers a brief review of the background and previous related literature, and we present the model in Section 3. We describe our experimental methodology in Section 4, and present our results in Section 5. In Section 6,

[5] Previous experimental studies (e.g., Fehr, Gächter, and Kirchsteiger 1997 and Fehr and Schmidt 2000) argue that an implicit contract is often more beneficial than an explicit contract, despite the theoretical predictions under the standard self-interest assumption. We feel that their point is well taken, but note that they compare complete contracting to incomplete contracting. Our concern is the optimal complete contract, as influenced by social preferences, in an environment where complete contracts are simple.

we estimate the Fehr-Schmidt model, and discuss determinants of agent and principal behavior. Section 7 concludes.

2. BACKGROUND AND RELATED WORK

Private information leads to inefficiency because it is effectively a form of monopoly power (of information). Sometimes it is possible to introduce competition (such as auctions) as a method of reducing informational rents. If competition is not a possibility, mechanism design can still effectively minimize the rents of the privately informed, provided that there are more dimensions in preferences than in the informational problem. If a principal knows workers care both about wages and the number of hours worked, he can devise a contract menu of hours and wages that induces more truthful revelation and reduced inefficiency.

In contracting under *hidden action (*sometimes characterized as moral hazard), the problem is how to induce the efficient action without being able to observe it. In principle, if outcomes are related to actions, we can induce efficiency by making the contract contingent on the outcome. Yet impediments such as risk preferences and limited liability may be present. For example, it may be necessary to have the agent incur some risk in order to induce the best action; however, this may conflict with other contractual objectives, such as providing insurance.

There is recent theoretical research about the impact of social forces in optimal contract design. Casadesus-Masanell (1999) studies a principal-agent problem with hidden action and assumes that an agent suffers disutility if her action differs from the social standard. Thus, the strength of extrinsic monetary incentives is lower than in standard theory, due to the trade-off between an agent's intrinsic and extrinsic incentives. The analysis is performed (with qualitatively similar conclusions) when the motivating factor is an ethical standard, similar to a social norm.

Rob and Zemsky (2002) study a problem in which agents working in a group (firm) must undertake both an individual task and a cooperative task. Effort devoted to the cooperative task is more productive than that devoted to the individual task, but the (noisy) performance measure is such that a worker receives only partial credit for her cooperative effort. Employees receive disutility from not cooperating, depending on the past cooperation levels in the group. The (dynamic) problem of the principal is to manage the group so as to maximize profits. As the solution has different steady state levels of cooperation ('corporate cultures') depending on the initial levels of cooperation, the incentive schemes vary across groups. Thus, this paper provides a theory for the observed heterogeneity in actual incentive schemes, and an operative definition of corporate culture.

Dufwenberg and Lundholm (2001) study an unemployment insurance situation in which there is hidden action (unobservable job search effort) and hidden information (privately known productivity of effort). The job search effort, although unobservable to the regulator, is observable to other members of society. Social pressure mitigates the hidden action problem, and effort is higher than under the absence of social concerns. However, individuals can pretend that the productivity of effort is lower than it really is; overall, the distribution of social respect is not clearly welfare improving. If one formulates an explicit utilitarian welfare function, the impact of social values on welfare is not monotonic, and welfare reaches a maximum for a positive but moderate social sensitivity.

Hidden action has been studied extensively in private-auction experiments (see Kagel 1995 for a review), and there are also some studies of the dynamic contracting problem.

Chaudhuri (1998) and Cooper, Kagel, Lo, and Gu (1999) study the problem of the *ratchet effect,* where the agent has an incentive to conceal her true type, as the principal may use this information to ratchet up the demands for performance in later periods. The theoretical prediction without pre-commitment is that types will remain hidden, although the laboratory results suggest otherwise.

Nevertheless, principal-agent interactions in the field are frequently one-shot affairs; furthermore, if the principal could commit to an *ex ante* contract, it would be optimal to implement the one-shot problem in the dynamic setting.[6] We are only aware of one experimental study of the static principal-agent problem with hidden information. Güth, Königstein, Kovács, and Zala-Mezõ (2001) conduct an experiment in which a principal faces two agents with unequal productivity functions. The principal offers each agent a separate two-part contract, which specifies both a fixed payment and a return share. The focus in this experiment is on "horizontal fairness" – how knowledge of the contract offered to the other agent affects effort choices. The principal finding is that making work contracts observable leads to a greater degree of pay compression. Effort choices differ systematically from the "rational" choices in relation to concerns of horizontal fairness.[7]

Papers such as Berg, Daley, Dickhaut, and O'Brien (1992), Keser and Willinger (2000), and Anderhub, Gächter, and Königstein (1999), and Königstein (2001) consider the behavioral issues present with individual contracting under hidden action, or moral hazard.[8] These studies

[6] In addition, even though a relationship may actually involve repeated play, a firm could choose to pre-commit to a contract, and perhaps cultivate a reputation for integrity by doing so.
[7] However, it is not clear from the paper whether an agent knew that the other agent had different marginal productivity.
[8] Other studies involving moral hazard include Bull, Schotter, and Weigelt (1987), who examine the incentive effects of piece rate and tournament payment schemes, and Nalbantian and Schotter (1997), who investigate group incentive contracts. The latter study finds that "relative performance schemes outperform target-based schemes," suggesting the relevance of social preferences to this context. Plott and Wilde (1982), DeJong, Forsythe, and

provide evidence that social forces are a consideration that affects the ability of the principal to reduce informational rents. Charness and Dufwenberg (2006) consider the hidden action problem in an experiment, and find that cheap-talk statements of intent *(promises)* help to achieve desirable outcomes (the Nash bargaining solution).

3. THE MODEL

In this section we describe the theoretical model that serves as the basis for the experimental design. Imagine that a firm needs two workers in order to be able to operate. The profits for the firm when it is operating are:

$$\Pi = e^1 - w^1 + e^2 - w^2$$

where e^i, w^i are, respectively, the effort levels and wages of worker $i \in \{1,2\}$. Each worker i has a utility function which depends on her type $j \in \{H,L\}$, which is her private information:

$$u_j^i(e^i, w^i) = w^i - \frac{k_j}{2}(e^i)^2$$

where $k_H = 1$ and $k_L = k > 1$. That is, the high type of agent has a lower cost of effort than the lower type. Thus, only the individual agent knows j, but e is observable and contractible.

From the utility functions of the principal and the agents we have that the first-best efforts levels are:

$$\hat{e}_j = \frac{1}{k_j},\ j \in \{H, L\} \qquad (1)$$

Lundholm (1985), and DeJong, Forsythe, Lundholm, and Uecker (1985) consider moral hazard problems with multiple buyers and sellers. Güth, Klose, Königstein, and Schwalbach (1998) consider a dynamic moral hazard problem where trust and reciprocity issues impede obtaining the first-best outcome.

We call $\hat{e}_j$ the efficient level of effort.[9] If we denote by $\underline{U}$ the outside option of the worker (which we assume for simplicity to be type-independent) we can induce optimal effort, with:

$$\hat{w}_j = \underline{U} + \frac{1}{2k_j}, j \in \{H, L\}$$

If the (independent) probability that an agent is a high or low type is denoted respectively by p_H or p_L, then the expected (optimal) profits for the principal are given by:

$$\Pi^E = 2(\frac{p_L}{2k_L} + \frac{p_H}{2k_H} - \underline{U})$$

The second-best optimal contracts, when the types are private information of the agents result from the solution of the maximization program:

$$\max_{w_H, w_L, e_H, e_L} 2(p_H(e_H - w_H) + p_L(e_L - w_L))$$

subject to

$$w_H - \frac{k_H}{2}(e_H)^2 \geq \underline{U} \quad (\text{IR}_H)$$

$$w_L - \frac{k_L}{2}(e_L)^2 \geq \underline{U} \quad (\text{IR}_L)$$

$$w_H - \frac{k_H}{2}(e_H)^2 \geq w_L - \frac{k_H}{2}(e_L)^2 \quad (\text{IC}_H)$$

$$w_L - \frac{k_L}{2}(e_L)^2 \geq w_H - \frac{k_L}{2}(e_H)^2 \qquad (\text{IC}_\text{L})$$

[9] This is an appropriate terminology because in all the Pareto-efficient allocations of this problem (with complete information) the level of effort is always $\hat{e}_j$. This is so because of the quasi-linearity of the utility function of the agents, a common assumption in this field. Thus, the Pareto-efficient allocations only differ in the wages and profits of the principal and agent.

where (IR_j)and (IC_{ji}) are respectively the individual rationality and incentive compatibility constraints of an agent of type $j \in \{H,L\}$. As usual in these problems, it turns out that the active constraints in the optimal solution are (IR_L) and (IC_H), so that the solution is:

$$e_H^* = \frac{1}{k_H} = 1; \quad e_L^* = \frac{1-p_H}{k_L - k_H p_H}; \quad w_L^* = \underline{U} + \frac{k_L}{2}(\frac{1-p_H}{k_L - k_H p_H})^2; \quad w_H^* = \frac{1}{2} + w_L^* - \frac{1}{2}(e_L^*)^2 \quad (2)$$

The high type of agent provides the 'efficient' level of effort and obtains utility above $\underline{U}$. These informational rents (rents are defined here as the utility an agent gets above her reservation utility) are equal to:

$$w_H^* - \frac{1}{2} - \underline{U} = \frac{k_L - 1}{2}(\frac{1-p_H}{k_L - k_H p_H})^2$$

The effort of the low type of agent is 'inefficiently' low and she obtains no rents. This is a subgame-perfect equilibrium.[10]

We implemented the theoretical model in our experiment by choosing values for the parameters in the three permitted contract menus shown in Table 1. Each menu consisted of a choice of two (enforceable) effort levels and payments that depend on the type of agent involved; if neither choice seems attractive to the agent, she can veto the contract menu. While we thus limit the possibilities available to the principal, a continuous strategy space would make the data

[10] There is one slightly non-standard feature of this model that should be mentioned. Since the agents' decisions are simultaneous, and a rejection implies that both agents receive the outside option, there exist subgame-perfect equilibria of the game, whose outcomes are different than the one we have just described. If one agent expects the other to reject the contract menu, it is a best response to reject contracts that give her a higher utility than $\underline{U}$. This can be used to construct a variety of subgame-perfect equilibria. However, notice that any strategy that rejects a contract yielding a higher utility than $\underline{U}$ is *weakly dominated*. While such equilibria are subgame-perfect, they are not *trembling-hand* perfect (Selten 1965), and do not survive one round of deletion of weakly-dominated strategies (Dekel and Fudenberg 1990).

analysis problematic (even ignoring the increased complexity of the decisions of the experimental participants), without adding much insight.

We chose $k_L = 2$ for all menus, in order to give relatively large rents to the H type (under her preferred contracts). Menu 1 is the 'theoretically-predicted' menu; it is not first-best efficient (since $e_L \neq \frac{1}{2}$) and has the most unequal payoffs. Here the values for e_i, and w_i are obtained from equation (2).[11] An H agent could obtain moderate rents (if she chose the 'right' contract and one of the contracts was accepted by the other agent) and an L agent could receive very small rents.[12] In Menu 2 the effort choices were the efficient ones, computed from equation (1). The value for w_L is set so that the L agent could receive small rents, while the value for w_H provides the H agent with higher rents than in Menu 1. In Menu 3, both types of agents can receive substantial rents, and (as in Menu 1) the efforts of both types correspond to the optimal ones in the theoretical model.[13] The parameters, efforts, and wages for the different menus in the experiment are summarized below:

TABLE 1 – PARAMETER VALUES

Menu	k_L	p_L	e_H	e_L	w_H	w_L
1	2	1/2	1	0.33	0.69	0.24
2	2	1/2	1	0.50	0.88	0.39
3	2	1/2	1	0.33	0.94	0.36

[11] All payoffs were rounded to the nearest 25 units in our payoff table.
[12] In the theoretical model the rents for the L player are exactly zero. We chose to make the rents positive (but very small) to make acceptance strictly dominant while remaining very close to the "theoretical prediction."
[13] In Menu 2, the high-type agent is given a wage that respects incentive compatibility, and an extra .25 is added. This was done primarily to see if a low-type agent will refuse to reject an unfavorable menu for fear of hurting an innocent bystander who is getting a fair deal. Menu 3 is just like Menu 1, but each type of agent receives this gift of .25 to the wage. This is still incentive compatible, acceptable, and asymmetric.

One of the criticisms of models of optimal contract design in adverse selection contexts is that the theoretically-predicted contract menus are more ‘complex’ than one observes in reality. In an environment like ours, these often employ a nonlinear structure and a very large number of possible choices of pairs of wages and efforts. This would be quite complicated to design for the principal, and even the choice of the agent would not be simple. While we have selected a very simple structure (only two types), we feel that a ‘simple’ menu can serve as an approximation for the fully-optimal schedule. As Wilson (1993) points out (p. 146) in a representative example: “The firm’s profits from the 5-part and two-part tariffs are 98.8% and 88.9% of the profits from the nonlinear tariff.”

4. EXPERIMENTAL PROCEDURES

Six sessions were conducted at Universitat Pompeu Fabra in Barcelona. All participants knew that there were 12 people in each session, with four principals, four high-type agents, and four low-type agents. Groups of three (one principal and two agents) were matched randomly in each of the 15 periods, subject to the restriction that no group was ever repeated in consecutive periods. While there were few repeated 3-groups, each agent could expect to be matched with each principal several times during the experimental session. On average, each participant received about 13 euros, including a show-up fee of 4 euros. Sessions lasted less than 2 hours.

At the beginning of a session, the instructions and a decision sheet were passed out to each subject. The decision sheet stated the subject number and the role (principal, high-type agent, or low-type agent). Instructions (presented in Appendix 1) covered all rules used to determine the payoffs to each player in the group; these were read aloud to the entire room. We included a table showing the monetary payoffs for every possible combination of actions. We verbally reviewed every case, and then asked questions to ensure that the process was understood.

[Payoff table about here]

When the instructional phase was concluded, we proceeded with the session. In each period the principals first selected a menu on their decision sheets. Each matched agent could accept choice 1 or 2 from this menu, or reject both options. If both agents in the group accepted contracts, each obtained the corresponding payoff for an agent of her type. If either of the agents rejected both choices 1 and 2, then the payoffs for both the principal and the agents were the same (500 units or 250 units depending on the treatment).[14] Payoff units converted to euros at the rate of 125 units to a euro.

The experimenter went around the room collecting this information, with care taken to preserve the anonymity (with respect to experimental role) of the principals. Once the principals' menu selections were recorded, the experimenter again went around the room, this time providing the information about the menu to the agents (again preserving anonymity). The agents then made their choices and the experimenter collected this information; finally, the experimenter privately informed each participant about the choices and types (but not the identities) of both agents in the group.

Participants knew that there would be 15 periods in all. At the end of the session, participants were paid privately, based on the payoffs achieved in a randomly-selected round, as

[14] In a sense, our game can be viewed as a multi-period 3-person version of the classic ultimatum bargaining game (Güth, Schmittberger and Schwarze 1982), where a rejection results in positive material payoffs.

was indicated in the instructions.[15] As mentioned earlier, two types of sessions were conducted, and these differed only with respect to the reservation payoffs for a rejection. We conducted three sessions for each treatment.

5. RESULTS

We find that the incentive-compatibility mechanism is predominantly successful in inducing a separation by contract selection among the agents who do not reject the contract menu proposed. However, there are many rejections of unfavorable contract menus by both types of agents. We also see a substantial degree of convergence on a 'community consensus' by the end of 15 periods. If social utility is not a factor, one would expect principals to choose Menu 1 and agents to accept the appropriate contract. However, in Treatment 1 (Treatment 2), when Menu 1 is proposed, it is rejected by at least one of the two agents 68% (40%) of the time. We also see that, from period 10 on, Menu 1 is selected less than 20% of the time in each of Treatments 1 and 2 (19% and 18%, respectively).

5.1 Principal behavior

In Treatment 1, Menu 2 is chosen in 40 of 180 cases (22%) and Menu 3 was chosen in 78 cases (43%). In Treatment 2, Menu 2 is chosen in 88 of 180 cases (49%) and Menu 3 was chosen in 29 cases (16%). The percentage of Menu 2 (Menu 3) contracts offered is lower (higher) in each and every Treatment 1 session than in each and every Treatment 2 session, and so the difference across treatments is significant at $p = 0.05$ using the Wilcoxon-Mann-Whitney test

[15] This was done in an effort to make payoffs more salient to the subjects, as this method makes the nominal payoffs 15 times as large as would be the case if payoffs were instead aggregated over 15 periods, and it also avoids possible wealth effects from accumulated earnings.

(see Siegel and Castellan 1988), even with the very conservative statistical approach of treating each of our sessions as being only one independent observation.[16]

Figures 1 and 2 show the patterns of menu proposals over time (Appendix 2 offers a chart of the aggregated proposals for each period):

[Figures 1 and 2 about here]

The rate of Menu 1 proposals drops over time in each treatment. If we look at the last 5 periods only, this rate is about 20% in each treatment. In contrast, the rate for Menu 3 increases to 63% in the last 5 periods of Treatment 1, and the rate for Menu 2 increases to 67% in the last 5 periods of Treatment 2. The trend for menu proposals over time seems clear in each case.

Principal choices also vary considerably across individuals. A chart showing each principal menu choice and the responses received is presented in Appendix 2.

TABLE 2- INDIVIDUAL PRINCIPAL CHOICES

Principal # - Treatment 1

	1	2	3	4	5	6	7	8	9	10	11	12	Total
Menu 1	5	8	3	3	6	6	9	4	3	5	4	6	62
Menu 2	6	4	1	3	5	1	5	3	1	1	5	5	40
Menu 3	4	3	11	9	4	8	1	8	11	9	6	4	78

Principal # - Treatment 2

	1	2	3	4	5	6	7	8	9	10	11	12	Total
Menu 1	0	4	15	0	9	2	2	12	6	3	3	7	63
Menu 2	15	10	0	0	6	8	12	3	5	9	12	8	88

[16] If we assume that each observation is independent, the difference across treatments in the distribution of proposals made is statistically significant at $p = 0.000$ ($\chi^2(2) = 40.45$). However, since there are 15 choices by each principal and there is also interaction through the agents, this approach overstates the degree of significance.

Menu 3	0	1	0	15	0	5	1	0	4	3	0	0	29

Another statistically clean test for differences in proposals across treatments is to examine only the first-period principal choices, as each of these should be independent. All 12 principals in Treatment 1 chose Menu 1 in the first period. By comparison, only seven of the 12 principals chose Menu 1 in Treatment 2, with three principals choosing Menu 2 and two principals choosing Menu 3. The Fisher exact test (see Siegel and Castellan 1988) finds that the difference in the number of Menu 1 choices in period 1 is significant across treatments ($p = 0.018$).

5.2 Agent behavior

Although agents who are concerned only with maximizing their own material reward should never reject a contract menu, rejections are quite common.[17] When Menu 1 is proposed, it is rejected by at least one of the two agents 68% (40%) of the time in Treatment 1 (2). Table 3 provides a summary of rejections by session, contract menu, and responder type:

TABLE 3 - REJECTIONS

	Menu 1		Menu 2		Menu 3	
Session	H	L	H	L	H	L
1	5/20	13/18	0/13	11/15	0/27	0/27
2	7/27	16/23	0/12	15/16	0/21	0/21
3	3/20	9/16	0/11	10/13	0/29	0/31
Treatment 1 total	*15/67*	*38/57*	*0/36*	*36/44*	*0/77*	*0/79*
4	0/21	6/17	0/21	2/29	0/18	0/14

[17] This contrasts with the results of the Chaudhuri (1998) study, which found few 'rejections' by the high productivity type firm in the 2nd (and final) period of his ratchet effect game.

5	10/26	0/24	0/28	0/30	0/6	0/6
6	7/17	5/21	0/36	1/32	0/7	0/7
Treatment 2 total	*17/64*	*11/62*	*0/85*	*3/91*	*0/31*	*0/27*

In Treatment 1, rejection rates of Menu 1 and Menu 2 are much higher for L types than for H types: 67% vs. 22% with Menu 1, and 82% vs. 0% with Menu 2. However, this is not the case for Menu 1 in Treatment 2, with rejection rates of 27% for H types and 18% for L types. Overall, we also see nearly 3 times (89 to 31) as many rejections in Treatment 1 as in Treatment 2. No H responder ever rejected Menu 2 and no responder of any type ever rejected Menu 3.[18]

As with principals, we find that there is considerable heterogeneity among the agents in the population; this can be seen in Appendix 3 and Appendix 4.[19] Overall, 16 of 24 L agents and 11 of 24 H agents rejected at least one proposed menu. In addition, three H agents who never rejected a menu chose 'low effort' at least once, sacrificing some money to reduce the principal's payoff. While most players rejected at some point, the distribution of the frequency of rejection is scattered.

We can examine whether rejection rates are stable over time. A supergame explanation for rejections would imply that rejection rates drop over time. Figures 3 and 4 show the rates for the cases with observed rejections, aggregated over three periods for smoothing:

[Figures 3 and 4 about here]

[18] Aside from rejections, the mechanism does successfully separate the types of agent in the types of contract accepted. Overall, of the 600 contract acceptances, 578 (96%) correctly mapped the agent to the predicted type. Low-type agents only chose high effort in three cases of 360, all in the first period; there were 19 cases of 360 where a high-type agent chose low effort.

[19] The average number of rejections and the standard deviation in Treatment 1 is 6.17 (2.62) for L types and 1.25 (2.01) for H types; in Treatment 2 these are 1.17 (1.90) and 1.41 (1.62) for L and H types, respectively.

Rejection rates of Menu 1 by H types are fairly stable in both treatments. Rates for L types increase where rejections seem to be effective - Menu 1 and Menu 2 in Treatment 1, as well as Menu 1 in Treatment 2.

6. DISCUSSION

Under the conventional assumption of own money-maximization, we should observe no rejections of any of the contract menus. However, given the vast body of research that people care about some notion of fairness, it is not surprising that agents sometimes reject lopsided contract offers and that principals respond by making more favorable offers. Given the multiple-period design and the likelihood that an agent will be (anonymously) paired with the same principal, a supergame notion might be suggested to explain the many rejections. Although this might explain rejections in early rounds, there is no evidence of decreases in rejection *rates* over time.[20] Strategic motivations alone do not provide an explanation for the observed behavior.

6.1 Estimating the Fehr-Schmidt model

One approach is to attempt to explain such behavior using a model of social preferences, and we do so below using the Fehr and Schmidt (1999) model,[21] which has the following form in our setup:

[20] One specific bit of evidence is that, in the very last round, seven principals tried Menu 1, perhaps thinking that rejections were only being made for strategic purposes; however, these were rejected by all L types (6/6) and 25% of H types (2/8).

[21] In the working-paper version of our paper, we also estimate the Charness and Rabin (2002) model, with similar results. However, this analysis is complex and is omitted here for expositional clarity.

$$v_i(\pi_1, \pi_2, \pi_P) = \pi_i - \alpha_i \frac{1}{2}\left(\sum_{j \neq i} \max\{\pi_j - \pi_i, 0\}\right) - \beta_i \frac{1}{2}\left(\sum_{j \neq i} \max\{\pi_i - \pi_j, 0\}\right)$$

Here (π_1, π_2, π_P) is the vector of monetary payoffs for agent 1, agent 2, and principal *P*. The critical parameters are α and β, which measure the degree to which one is averse to coming out behind or coming out ahead, respectively. In this model, it is assumed that α ≥ β and that 1 ≥ β ≥ 0; Fehr and Schmidt note that there is very little evidence about aversion towards a difference in favor of a player, so that β_i may well be a small number. In relation to rejecting a contract menu, a concern about coming out ahead could only be relevant for the high-type agent.[22] However, β does not seem to be important for high-type agents, since they never reject Menu 2, where the gap between agent payoffs is greatest. We focus exclusively on the agent's α, as the value of β = 0 for the agent fits best out of the several values we tried in the constrained range.[23]

We analyze our data using a multinomial random-parameters logit model (NLogit, version 3.0), where the expression

$$p(action\ 1) = \frac{e^{\gamma * \mathrm{U}(action\,1)}}{e^{\gamma * \mathrm{U}(action\,1)} + e^{\gamma * \mathrm{U}(action\,2)} + e^{\gamma * \mathrm{U}(action\,3)}}$$

is used to determine the values that best match predicted probabilities of play with the observed behavior; γ is a precision parameter reflecting sensitivity to differences in utility (see McFadden 1981). The higher that γ is, the sharper the predictions—when γ is 0, the probability of any of the three available actions must be 1/3; when γ is arbitrarily large, the probability of the action

[22] As is standard practice, we primarily focus such an analysis on the responders in the game, since the behavior of principals depends on expectations about the responses that will be made, and this confounds the social-preference analysis; we address strategic principal behavior in section 6.3.

[23] A regression also including β as an explanatory variable gives an estimate of –1.738 for β. This value is outside the permitted range for the model (and also seems suspect since β is not estimable for low-type agents, and is overwhelmed by α for high-type agents with Menu 1 or Menu 2).

yielding the highest utility approaches one. Random parameters accounts for multiple effects by assuming that sets of observations that belong to the same individual have some common structure that differs from individual to individual, and that observed behavior corresponds to individuals implementing their own preferences with error.[24] The likelihood of error is assumed to be a decreasing function of the utility cost of an error.

TABLE 4: FEHR-SCHMIDT REGRESSION ESTIMATES

Variable	Coefficient	t-statistic	p-value[25]
α	.0918	4.23	0.000
α_R	.0884	1.48	0.140
γ	.0046	23.71	0.000

N = 720; Log likelihood = -374.477

In this table, α is the Fehr-Schmidt α, and γ is the precision parameter. α_R is the coefficient of a dummy variable added to α, and has a value of 1 if the other agent rejected the contract menu in the previous period, but is otherwise equal to 0.[26]

We see that our agent population estimate for α is about .09 and is highly significant. It is also interesting to note that this parameter value nearly doubles when an agent has observed that the other agent in the group has rejected a contract menu in the previous period. While α_R is

[24] A random-effects model estimates $Y_{it} = a_i + b*X_{it} + e_{it}$, where a_i is a random variable. A random-parameters model takes this a step further, estimating $Y_{it} = a_i + b_i*X_{it} + e_{it}$, with b_i also being random.

[25] The *p*-values reflect two-tailed test results. In some cases there is an argument for a one-tailed test, which would cut the p-value in half.

[26] A regression also including β as an explanatory variable gives an estimate of –1.738 for β. This value is outside the range of the permitted range for the model (and also seems suspect since β is not estimable for low-type agents, and is overwhelmed by α for high-type agents with Menu 1 or Menu 2). We implicitly set $\beta = 0$ by excluding it from the regression. Note that doing so means we are effectively estimating the Bolton (1991) model.

short of conventional statistical significance, it appears that agents' social preferences may be influenced by perceptions of the social preferences of others.

We can perform a simple calculation of the optimal contracts using the parameter values estimated for the Fehr-Schmidt model. These are the lowest values that would be accepted by 'representative agents' who have the estimated parameters; a more complete optimal contract calculation would need to take into account issues such as the principal's degree of risk-aversion and the considerable degree of heterogeneity across agents. We display these computed contract menus in Table 5:

TABLE 5 – *EX POST* OPTIMAL CONTRACTS

Parameter Values

Treatment	k_L	p_L	e_H	e_L	w_H	w_L
1 (500)	2	1/2	1	0.32	0.80	0.25
2 (250)	2	1/2	1	0.32	0.76	0.21

Induced Numerical Payoffs

Treatment 1 (500)	Principal	Agent 1	Agent 2
2 H agents	3375	1010	1010
1 H agent, I L agent	2668	1010	595
2 L agents	1961	595	595
Treatment 2 (250)	Principal	Agent 1	Agent 2
2 H agents	3774	810	810
1 H agent, I L agent	3067	810	396
2 L agents	2359	396	396

These values suggest that, if a principal were free to design the contract menu, he could do considerably better than the Menu 3 result in Treatment 1 and the Menu 2 result in Treatment 2 (the *ex post* most profitable menus in the respective cases). Of course, a principal wishing to take into account the agents' heterogeneity might choose to increase the offers to ensure fewer rejections.

In Treatment 1, Menu 1 provides both low-and high-type agents amounts less than the cutoff values calculated, while the available payoff for the low-type agent is too low with Menu 2. In Treatment 2, by contrast, while Menu 1 is still unattractive to high-type agents, both Menu 1 and Menu 2 are acceptable to the representative low-type agent. This is consistent with Menu

3 being the most common of the three permitted contracted menus in Treatment 1, and Menu 2 being the most common contract menu in Treatment 2.

The Fehr and Schmidt model requires substantial heterogeneity in the population to successfully explain many experimental results. Since we do have a great deal of heterogeneity, our point estimate may not reflect the richness of the model, although the random-parameters approach attempts to address this concern. While it is difficult to reliably estimate parameters for each agent, given the few observations for each individual, we can nevertheless examine individual rejection behavior in response to each menu, in relation to the rejection cut-off values. We call an agent a *rejector* of a given menu if she rejects it at least 50% of the time, and consider the proportion of rejectors in relation to the minimum parameter values that would induce a rejection of the menu:

TABLE 6: PROPORTIONS OF REJECTORS AND CUTOFF PARAMETERS

Menu-type-treatment	**Observed Rejection rate**	**FS cutoff**
M2 – L – 1	11/12	0.04
M1 – L – 1	9/12	0.02
M1 – L – 2	4/12	0.24
M1 – H – 1	3/12	0.21
M1 – H – 2	3/12	0.39
M2 – L – 2	0/12	0.26
M2 – H – 1	0/12	1.91
M2 – H – 2	0/12	2.42

When the cutoff value for rejecting a contract menu is very low, most agents reject the menu. Intermediate cutoff values lead to only a fraction of agents rejecting the contract menu, while no one rejects a menu when the cutoff value is very high. While the overall relationship is broadly consistent with the model's predictions, we see no evidence of the high parameter values required for the Fehr-Schmidt model to explain behavior in many experimental games.[27] No high agent ever rejected Menu 2 (and no agent ever rejected Menu 3), suggesting that other factors are also present.

6.2 Patterns in agent behavior

Our experimental design permits each agent to observe the behavior of the other agent in the team. Agents can observe the actions of other agents, and they also are exposed to a variety of contract menus since there is such a high degree of heterogeneity in principal behavior, particularly in the early periods. As a result, agents may update their beliefs about what constitutes acceptable principal and agent behavior in this experimental society.

We do observe that the behavior of individual agents often varies over the course of a session. In fact, fewer than half (23 of 48) of all agents respond in a consistent manner, rejecting or not rejecting, to each contract menu; the remaining agents are either pursuing some mysterious "mixed strategy" or are susceptible to influences during the session. We mentioned earlier that 27 of the 48 agents chose to reject a contract menu at least once; of these 27 agents, only six rejected a contract menu in the first period. What caused the other 21 agents to begin rejecting contracts later? We find that in 10 of these cases, the other agent rejected a contract menu in the period immediately preceding the observed initial rejection. Similarly, this initial rejection occurred eight times when an agent was offered a contract menu less favorable than the one in the previous period.

[27] For example, the observed rejections of 40% offers in the ultimatum game means that α must have a value of at least 2.00.

We also find that the likelihood of an agent rejecting a particular contract menu can depend on the menu the agent was offered in the previous period. In Treatment 1, Low agents rejected Menu 1 18/22 times (81.8%) in the period after being offered a better contract menu, compared to 20/35 times (57.1%) otherwise; the corresponding figures for Treatment 2 are 9/30 (30.0%) and 2/32 (6.2%). Low agents are also just slightly less likely to reject Menu 2 if they were offered Menu 1 in the previous period than if they were offered Menu 2 in the previous period, 7/42 times (16.7%) versus 12/60 times (20.0%) overall. High types do not appear influenced much by such considerations, rejecting Menu 1 14/53 times (26.4%) in the period after being offered a better contract menu versus 17/78 (21.8%) times otherwise, and never rejecting Menu 2 in any case.

Similarly, in the period after the other agent rejected a contract menu, agents reject the contract menu 26/112 times (23.2%); this compares to 94/608 (15.5%) rejections in periods when there was no rejection by the other agent. Here the effects are similar for low and high agents, with 19/60 (31.7%) rejections after an observed rejection versus 69/300 (23.0%) in the alternative for low agents and 7/52 (13.5%) versus 25/308 (8.1%) for high agents.

To investigate the influences of changes in menu and observed rejections by other agents while accounting for other factors, we perform a random-effect probit regression (with robust standard errors), with rejection as the dependent variable. In this regression, Lagged rejection = 1 if the other agent rejected the contract menu in the previous period and was otherwise 0; Menu_up = 1 if the contract menu was more favorable to the agent than the one in the previous period and was otherwise 0; Menu_down = 1 if the contract menu was less favorable to the agent than the one in the previous period and was otherwise 0; High_agent = 1 if the agent was a high type and was otherwise 0.

TABLE 7: DETERMINANTS OF REJECTION

Variable	Coefficient	Z-statistic	p-value[28]
Treatment 1	1.036	3.56	0.000
Menu	-0.916	-6.58	0.000
Lagged rejection	0.316	1.64	0.100
Menu_up	-0.180	-1.01	0.156
Menu_down	1.062	4.37	0.000
High_agent*Menu_down	-1.350	-3.80	0.000
Period	-0.017	-0.80	0.426
Constant	-0.025	-0.08	0.933

N = 672; Log pseudo-likelihood = -197.164

The regression confirms the strong treatment effect (more rejections in Treatment 1), the effect of the menu chosen on the rate of rejection (the higher the menu, the lower the rejection rate). We see that agents are influenced by the behavior of other agents, as an observed rejection in the previous period makes an agent more likely to reject, with a marginal significance level similar to that found for α_R in the Fehr-Schmidt estimation. If a given contract menu is more favorable than the previous menu offered, an agent is slightly less likely to reject, but this effect is not significant. On the other hand, getting a worse offer than in the last period (holding the menu constant) causes low agents to be more likely to reject; however, this effect is not present for high agents. Finally, we see a slight decline in the rejection rate over time, but this is small and insignificant.[29]

[28] The *p*-values reflect two-tailed test results. In some cases there is an argument for a one-tailed test, which would cut the p-value in half.
[29] We also tried other specifications with dummy variables, with similar results.

So it appears that low agents become particularly unhappy when the wage offer is decreased, even holding the final wage offer in question constant; on the other hand, high agents, who receive more pay, do not. Perhaps this is an artifact of the fact that participants who had been selected to be low agents had already experienced a bad draw and were more prone to take offense. But this same phenomenon could easily be present in the field, with less able workers unhappy about their endowment of ability. There also appears to be a modest amount of "social learning" in this setting, in that both types of agents are more likely to reject a contract menu after seeing another agent rejecting a contract menu in the previous period.

6.3 Determinants of principal behavior

Principals do not change their behavior in a vacuum, but appear to respond to rejections. Table 6 presents the data concerning whether or not a principal changed the contract menu after observing either joint acceptance or a rejection by at least one agent (14 observations for each principal):

TABLE 8 – MENU CHANGES BY PRINCIPALS

No rejection in prior period

	Higher Menu	Same Menu	Lower Menu
Treatment 1	10 (10%)	66 (65%)	25 (25%)
Treatment 2	16 (11%)	99 (70%)	26 (18%)

Rejection in prior period

	Higher Menu	Same Menu	Lower Menu
Treatment 1	37 (55%)	18 (27%)	12 (18%)
Treatment 2	12 (44%)	13 (48%)	2 (7%)

Principals are substantially more likely to select a higher-numbered menu after a rejection than after no rejection, with the likelihood of a change to a more ‘generous’ menu being four or five times greater. A chi-square test comparing the distribution of choices across lagged rejection conditions shows strongly significant differences for each of Treatment 1 and Treatment 2. However, this assumes that each observation is independent, which is clearly not the case here.

We use random-effects ordered-probit regressions to account for the 15 observations for each participant; we also include period dummies to account for possible time trends.[30] We consider whether the principal was more likely to choose a higher-numbered menu depending on whether there was a rejection in the previous period, and also whether the principal was more likely to choose a lower-numbered menu after a non-rejection in the previous period. The full regression results are shown in Appendix 5 for Treatment 1 and Treatment 2 data separately and pooled. We find that a principal is significantly more likely to choose a less aggressive menu after a rejection in the previous period, for both the pooled data and the separate treatments ($p < .0.0001$ in all cases). After a non-rejection, a principal is more likely to make a more aggressive menu choice; while this effect is not quite significant when treatments are considered separately, it is significant ($p < 0.016$) with the pooled data. It appears that rejections drive the changes in principal behavior over time.[31]

We also perform a more complex analysis, estimating principals’ prior beliefs that a given contract menu would be rejected, and how these beliefs are affected by rejections. In the

[30] We choose period 8 as the baseline, as this seemed most likely to identify any period effects (early exploration is largely finished and any potential unraveling should not yet be a factor).

[31] We also consider whether a lagged rejection (or the lack thereof) plays a role in whether a principal chooses a lower-numbered menu (i.e., makes a more aggressive menu choice. We find, for the pooled data, that a principal is significantly more likely to make the aggressive change when there is no lagged rejection. However, this effect is

process, we develop an explanation for the treatment effect that we observe. We hold fixed the Fehr-Schmidt and precision parameter(s), and we derive the values for priors and rejection effects for each parameter value.[32] It turns out that our estimates are robust over a range of Fehr-Schmidt parameters. We assume that the principal chooses from among the three contract menus by evaluating the predicted utility, using the multinomial logit model described above. We also assume that the (correct) prior for Menu 3 rejection is zero.

We note that it is β that is the relevant parameter, since the principal almost always comes out ahead of the agents when there is no rejection. We assumed that $\beta = 0$ for an agent thinking about another agent's payoffs, but this may also be a function of the fact that an agent need not feel responsible for protecting the other agent's interests, since the other agent can always choose to reject the contract menu himself. In any case, β should not be larger than the .09 estimated for α (or the .18 for $\alpha + \alpha_R$) for the agents, since the model presumes that α is no less than β. We report estimates for four values of β that span this range, with *Z*-statistics in parentheses:

TABLE 9 – PRINCIPAL PRIORS AND UPDATING

Treatment 1	β = 0	β = .05	β = .10	β = .20
Menu 1 Rejection Prior	.528 (17.8)	.549 (18.3)	.570 (18.7)	.615 (19.7)
Menu 2 Rejection Prior	.442 (12.2)	.457 (12.1)	.474 (12.0)	.512 (11.9)
Effect of a Lagged Rejection	.038 (2.23)	.038 (2.23)	.038 (2.23)	.038 (2.23)

not quite significant when treatments are considered separately, although it does achieve 5% significance with the Treatment 1 data, using an (appropriate) one-tailed test.

[32] We do this because of an identification problem – we have two estimated parameters (the menu-specific constants in the estimation), but there are four desired parameters that jointly determine the constant terms.

Treatment 2	β = 0	β = .05	β = .10	β = .20
Menu 1 Rejection Prior	.475 (18.1)	.495 (18.6)	.515 (19.1)	.558 (20.2)
Menu 2 Rejection Prior	.258 (8.86)	.266 (8.79)	.274 (8.72)	.294 (8.59)
Effect of a Lagged Rejection	.082 (1.84)	.082 (1.84)	.082 (1.84)	.082 (1.84)

In this table, the rejection prior for a menu is a probability, as is the effect of one rejection during the previous five periods (so that multiple past rejections have a correspondingly greater effect).[33] We were not able to estimate separate effects of a rejection on Menu 1 and Menu 2 priors. The principals in Treatment 1 think that rejection is more likely for both Menu 1 and Menu 2 than in Treatment 2; while this difference is significant in both cases, the difference is much larger with respect to Menu 2. Lagged rejections have a significant impact on beliefs about rejection in the current period; the coefficient is larger in Treatment 2, where rejections are less frequent and costlier.

Note that rejection priors are substantially lower in Treatment 2 than in Treatment 1, for both Menu 1 and Menu 2. This seems natural, given that rejection in Treatment 2 is more costly, so that principals anticipate that rejection rates will be lower. Costlier rejections would also be expected to have more effect on principal beliefs.

Our estimation process yields *ex ante* differences in expected utility for different contract menus, as well as how much this changes when rejections are experienced:

[33] Implicit in the specification is a decrease in expected probability in the absence of lagged rejections, which is 0.2 of an acceptance. This has been imposed, rather than estimated, due to an identification problem. Having separate variables for rejection and non-rejection would lead to perfect multi-collinearity, so that we cannot perform a separate estimation for each coefficient. While the choice of the 0.2 value is somewhat arbitrary, it is roughly consistent with the probability of rejection and produces sensible results.

TABLE 10: INITIAL ADVANTAGE ESTIMATES FOR MENUS

Variable	Coefficient	Z-statistic
Menu 1, T2 constant	-0.303	-1.38
Menu 1, T1 dummy	1.063	3.01
Menu 3, T2 constant	-1.458	-5.28
Menu 3, T1 dummy	1.752	5.08
GA1	0.00019	2.23
GA2	0.00022	0.95

Recall that we interpret choice probabilities as being induced by (random) utilities in a logit context. We normalize the Menu 2 constant to zero.[34] Thus, the first line gives the prior expected payoff for Menu 1 in Treatment 2, relative to Menu 2; similarly, the third line gives the prior relative expected payoff for Menu 3 in Treatment 2. To obtain the prior expected relative payoff for Treatment 1, add the coefficients of the first two rows for Menu 1 and add the coefficients of the middle two rows for Menu 3. GA1 is the coefficient on a variable that, in Treatment 1, adds the rejections of the last five periods experienced by the menu under consideration (times the payoff if accepted minus payoff if rejected). GA2 is a dummy for Treatment 2, so that the applicable coefficient for Treatment 2 is GA1+GA2.

Thus, Menu 1 has a relative initial advantage of 0.76 over Menu 2 in Treatment 1 and Menu 3 has an initial advantage of 0.29 over Menu 2. However, the perceived advantage dissipates at the rate of 0.49 per rejection,[35] so that this advantage of Menu 1 over Menu 2 is gone after 1.5 rejections, and so Menu 1 becomes unattractive by period 4 or 5. Since Menu 2 starts

[34] In logit models, because of the exponential form, one can estimate alternative specific constants for one less than the number of alternatives.

[35] GA1*(payoff if accepted minus payoff if rejected) = (0.00019)*(3068.75)=0.49.

with an initial disadvantage relative to Menu 3 and this increases over time, it is not surprising that Menu 2 seems to be mainly a transition state in Treatment 1.

For Treatment 2, Menu 2 actually starts with a (not statistically significant) 0.3 advantage over Menu 1, so it should be played more often initially than in Treatment 1; one might still expect a fair degree of Menu 1 play initially, to the extent that a principal might wish to experiment in early periods. Since Menu 1 is rejected more often than Menu 2, soon Menu 2 becomes strongly preferred. The large initial advantage of 1.458 for Menu 2 over Menu 3 is enough so that Menu 2 is still preferred over Menu 3, since Menu 2 is rejected so rarely in Treatment 2.

7. CONCLUSION

We explore the problem of optimal contract menus with hidden information and team production in a laboratory experiment matching a principal with two agents of unknown types. As standard contract theory does not consider social forces, the theoretically-optimal contract menu is often rejected and more agent-favorable contract menus are soon chosen as a result. After the principals learn the (evolving and heterogeneous) standard for menu acceptability, the production team functions in a relatively efficient manner, with agents choosing contracts in accordance with their types. It is interesting that changing the reservation payoffs leads to a different menu becoming predominant after a number of periods, even though standard theory would predict no differential effect. Rejection rates are much higher in Treatment 1, where the reservation payoffs are higher. This difference in reservation payoffs also leads to a different prevailing contract menu in our two treatments, as low agents are reluctant to veto Menu 2 in

Treatment 2. There is a substantial degree of heterogeneity in the behavior of both principals and agents.

The simple Fehr and Schmidt (1999) model of social utility captures much of the observed behavior, although we not see evidence of high inequality-aversion parameters in the population and no one ever rejects the most favorable menu, even though it favors the principal. We calculate *ex post* optimal contract menus for representative agents, where the principals extract less than in standard models, but still make substantial profits.

We also find that the history experienced by an agent has an affect on rejection behavior. Agents are more likely to reject a contract menu if they have observed a rejection from the other agent in the previous period, perhaps updating their views about the social norms and adjusting their values accordingly. The socially-appropriate action is not always obvious and so it seems reasonable that some people look to their peers for guidance.[36] Low agents are more likely to reject a particular menu when it offers a lower wage than the menu offered in the previous period, although we don't see this effect for high agents. Thus, we see downward rigidity in wages for the poorly-endowed agent, who perhaps resents the bad draw that has placed her in this position. Principal behavior is driven by experienced rejections, and we estimate priors and the marginal effects of rejections.

Since more effective contracts are likely to lead to better economic outcomes, we echo the view (Güth, Königstein, Kovács, and Zala-Mezõ 2001: 85) that "there is a need for behavioral contract theory, based on empirical findings." It is clear that further evidence on contracts, worker behavior, and social forces is needed.

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[36] Falk, Fischbacher, and Gächter (2003) find that individuals who are simultaneously linked to two separate 'communities' allocate different proportions of their endowments to the public good in the different communities,

their behavior depending on the behavior of the other people in the group. Charness, Corominas-Bosch, and Frechette (2007) find evidence of social learning in a bilateral network bargaining game.

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APPENDIX 1 - INSTRUCTIONS

Thank you for participating in this experiment. The experiment will consist of a series of 15 decision periods. In each period you will be randomly and anonymously matched with two other persons; the action you choose and the action chosen by the persons with whom you are matched will jointly determine your payoffs in each period.

You have been assigned a subject number. Please retain this number, as we will need it to pay you at the end of the experiment.

Process: There are two classes of players: *proposers* and *responders*. The responders can be one of two types: HIGH or LOW. The class to which the player is assigned (proposer or responder) and the type of the players (in the case the player is a responder) are chosen randomly at the beginning of the game. Each responder has an equal initial probability to be of either type HIGH or type LOW. Half of all responders will be of each type. Each responder knows her type, but no other participant does. Your role (class and type) *will not change during the experiment*. Your subject number, class and type (if you are a responder) are printed on your decision sheet.

In each period you will be randomly-matched in groups of three players, according to subject numbers. All groups will be composed of a proposer and two responders of any combination of types; *ex ante*, there is a 25% chance that both responders are HIGH, a 50% chance that one is HIGH and the other is LOW, and a 25% chance that both are LOW.[37] The identity of the other players in the group is unknown to you and the composition of the groups will change randomly every period. While you will not know the matching process, we would be happy to show you (at the end of the experiment) how the matches were created.

Once the period begins each proposer must make a selection from one of 3 possible choices {1,2,3} and will do so by checking a box for that period on the decision sheet provided. We will come around the room and record each proposer selection. Next we will go around the room and mark the proposer selections on the decision sheets of the responders in the appropriate groups. At this point, the two responders in each group must each choose one of the three available options {1,2,VETO} by checking the corresponding box on the decision sheet. (For

[37] In fact, these were the actual probabilities, given our matching scheme (see Appendix 2). The actual *ex ante* probabilities are 3/14, 4/7, and 3/14.

both proposers and responders, we ask that you do not fill in the spaces clearly marked as EXPERIMENTER.) We will then record these choices. Finally, we will once again go around the room and mark the responder decisions (and the type of responders) for each group on the decision sheets for all members of that group. At this point, you can calculate your payoff from the period from the table.

How choices depend on points: The payoffs will be a function of the proposer's choice and the responders' responses. The conversion rate from payoff units to euros is 125 units to one euro. Please refer to the table provided and we will offer some examples of how this process works. [This Table is at the end of Appendix 1.]

First, you should understand that, unless one of the responders chooses to VETO the proposer's choice, the payoff for any responder depends **only** on the proposer's choice and the responder's choice. No person will ever receive a negative payoff *unless she chooses it herself.*

If **either** responder chooses to VETO the proposal, then the VETO payoffs (shown in the columns shaded in gray on the payoff table in your packet) would result.

If you are a Responder, you may be wondering how you can tell if you are Responder 1 or Responder 2. There is an algorithm you can use which will make your task easier: if you are a Responder of the HIGH type, simply consider yourself to be Responder 1; if you are a Responder of the LOW type, simply consider yourself to be Responder 2. In all cases, this will ensure that your payoffs correspond to your choices.

Suppose the proposer chooses option 1 and faces responders who are both type HIGH. Suppose further that both responders choose option 1. First, find the rows corresponding to Proposer Choice 1. Next, find the 5 columns corresponding to the case where both responders are HIGH. The column that is relevant in this case is headed by “11”. As you can see, the Proposer would receive 3950 units, Responder 1 would receive 775 units and Responder 2 would receive 775 units. Suppose instead that Responder 1 chooses option 1 and Responder 2 chooses option 2. The column that is now relevant is headed by “12”. In this case the Proposer would receive 3075 units, Responder 1 (who chose option 1) would receive 775 units, and Responder 2 (who chose option 2) would receive 725 units. If instead Responder 1 chooses option 2 and Responder 2 chooses option 1, the column that is now relevant is headed by “21”. In this case the Proposer would receive 3075 units, Responder 1 (who chose option 2) would receive 725 units, and Responder 2 (who chose option 1) would receive 775 units. If instead Responder 1 chooses option 2 and Responder 2 chooses option 2, the column that is now relevant is headed by "22". In this case the Proposer would receive 2175 units, Responder 1 would receive 725 units, and Responder 2 would receive 725 units. Suppose instead that **either** Responder chooses to VETO the proposer's choice. In this case, the Proposer would receive 500 units and each Responder would receive 500 units.

Suppose the Proposer chooses option 2 and faces two LOW Responders. First, find the rows corresponding to Proposer Choice 2. Next, find the 5 columns corresponding to the case in which both responders are LOW. Suppose further that both responders choose option 1. The column that is relevant in this case is headed by "11". As you can see, the Proposer would receive 2500 units, Responder 1 would receive -550 units and Responder 2 would receive -550 units. Suppose instead that Responder 1 chooses option 1 and Responder 2 chooses option 2. The column that is now relevant is headed by "12". Then the Proposer would receive 2400 units, Responder 1 (who chose option 1) would receive -550 units, and Responder 2 (who chose option 2) would receive 550 units. If instead Responder 1 chooses option 2 and Responder 2 chooses option 1, the column that is now relevant is headed by "21". Then the Proposer would receive 2400 units, Responder 1 (who chose option 2) would receive 550 units, and Responder 2 (who chose option 1) would receive -550 units. If instead Responder 1 chooses option 2 and Responder 2 chooses option 2, the column that is now relevant is headed by "22". Then the Proposer would receive 2300 units, Responder 1 would receive 550 units, and Responder 2 would receive 550 units. Suppose instead that **either** Responder chooses to VETO the proposer's choice. In this case, the Proposer would receive 500 units and each Responder would receive 500 units.

Suppose the Proposer chooses option 3 and faces one HIGH responder and one LOW responder (by the way the table is written, the type HIGH is necessarily Responder 1 and the type LOW is necessarily Responder 2). First, find the rows corresponding to Proposer Choice 3. Next, find the 5 columns corresponding to the case where one responder is HIGH and the other is LOW. Suppose further that both responders choose option 1. The column that is relevant in this case is headed by "11". As you can see, the Proposer would receive 2050 units, Responder 1 would receive 1725 units and Responder 2 would receive -325 units. Suppose instead that Responder 1 chooses option 1 and Responder 2 chooses option 2. The column that is now relevant is headed by “12”. Then the Proposer would receive 1625 units, Responder 1 (who chose option 1) would receive 1725 units, and Responder 2 (who chose option 2) would receive 1000 units. If instead Reponder 1 chooses option 2 and Responder 2 chooses option 1, the column that is now relevant is headed by “'21”. Then the Proposer would receive 1625 units, Responder 1 (who chose option 2) would receive 1225 units, and Responder 2 (who chose option 1) would receive -325 units. If instead Reponder 1 chooses option 2 and Responder 2 chooses option 2, the column that is now relevant is headed by “22”. Then the Proposer would receive 1175 units, Responder 1 would receive 1225 units, and Responder 2 would receive 1000 units. Suppose instead that **either** Responder chooses to VETO the proposer's choice. In this case, the Proposer would receive 500 units and each Responder would receive 500 units.

Payment: Each person will be paid individually and privately. Only one of the 15 periods will be chosen at random for actual payment, using a die with multiple sides. In addition, you will receive 4 euros for participating in the experiment. If, in the period selected your payoff is negative, it will be deducted from the 4 euro show-up fee; however, no one will receive a net payoff less than 0.

If you have questions raise your hand and one of us will come and answer your question. Direct communication between participants is strictly forbidden. Please ask questions if you do not fully understand the instructions. Are there any questions?

PAYOFF TABLE

	2 HIGH responders						1 HIGH, 1 LOW responder						2 LOW responders				
	11	**12**	**21**	**22**	**VETO**		**11**	**12**	**21**	**22**	**VETO**		**11**	**12**	**21**	**22**	**VETO**
Proposer	3950	3075	3075	2175	500		3950	3075	3075	2175	500		3950	3075	3075	2175	500
Responder 1	775	775	725	725	500		775	775	725	725	500		-1275	-1275	525	525	500
Responder 2	775	725	775	725	500		-1275	525	-1275	525	500		-1275	525	-1275	525	500
Proposer	2500	2400	2400	2300	500		2500	2400	2400	2300	500		2500	2400	2400	2300	500
Responder 1	1450	1450	1050	1050	500		1450	1450	1050	1050	500		-550	-550	550	550	500
Responder 2	1450	1050	1450	1050	500		-550	550	-550	550	500		-550	550	-550	550	500
Proposer	2050	1625	1625	1175	500		2050	1625	1625	1175	500		2050	1625	1625	1175	500
Responder 1	1725	1725	1225	1225	500		1725	1725	1225	1225	500		-325	-325	1000	1000	500
Responder 2	1725	1225	1700	1225	500		-325	1000	-325	1000	500		-325	1000	-325	1000	500

APPENDIX 2 - INDIVIDUAL PRINCIPAL CHOICES AND RESPONSES

A matching scheme was randomly determined (subject to no group repeating in two consecutive periods) and was used in all sessions. One can track the entire history of the sessions, given the matching scheme below and the results presented in previous Tables. Principals were # 1, 2, 11, and 12; H types were # 3, 4, 9, and 10; L types were # 5, 6, 7, and 8:

Period	Group 1			Group 2			Group 3			Group 4		
1	1	10	6	2	4	3	11	9	5	12	7	8
2	1	4	3	2	5	6	11	9	7	12	10	8
3	1	9	7	2	4	5	11	3	10	12	8	6
4	1	3	6	2	9	5	11	10	7	12	4	8
5	1	6	5	2	4	8	11	3	7	12	10	9
6	1	9	4	2	3	7	11	8	5	12	10	6
7	1	10	7	2	6	8	11	3	4	12	9	5
8	1	6	7	2	10	3	11	5	8	12	4	9
9	1	3	7	2	9	6	11	10	5	12	4	8
10	1	4	5	2	7	6	11	10	9	12	3	8
11	1	10	5	2	9	7	11	8	6	12	3	4
12	1	6	8	2	5	7	11	3	9	12	10	4
13	1	7	5	2	4	10	11	3	6	12	9	8
14	1	6	8	2	4	9	11	3	5	12	10	7
15	1	10	4	2	8	5	11	3	6	12	9	7

TREATMENT 1

Prop.	Period														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	1*/	2	2	1*	2**	2	3	2**	1*	2*	1*	3	3	3	1*
2	1	2	1	1*/	1*	2*	1**	1*	3	2*	3	3	1	2	1**
3	1	1*	1*	2*	3	3	3	3	3	3	3	3	3	3	3
4	1*	1**	1*	2*	3	2*	2*	3	3	3	3	3	3	3	3
5	1*	1	1**	2*	1**	3	2	2**	1*	3	1*	3	2**	3	2
6	1	1*	1	1**	2*/	1*	3	1	3	3	3	3	3	3	3
7	1/	1**	1	1*	2*	1**	1	2**	2*	1*	2**	1*	2*	3	1*
8	1	1*	1*	3	2	2*	2*	3	3	3	3	3	3	3	1**
9	1	1/	1**	3	2*	3	3	3	3	3	3	3	3	3	3
10	1/	1*	1*/	2*	1*/	3	3	3	3	3	3	3	1/	3	3
11	1*	1*	2	3	2*	3	3	2**	3	3	1*	2	2	1*	3
12	1*	1*	2	3	3	1	2*	1	3	2*	3	1/	2*	2*	1*

TREATMENT 2

Prop.	Period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	2	2	2	2	2*	2	2	2	2	2	2	2	2	2	2
2	1	2	3	2*	1	2	2	1	2	2	2	1**	2	2	2
3	1/	1	1	1	1	1*	1	1*	1	1	1*	1	1	1	1*
4	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
5	1	1	1	1	1	1*	1*	1	1/	2	2	2	2	2	2
6	2	1	3	2	2	3	2	1*	2	2	3	2	3	2	3
7	3	2	2	2	2	1	1*/	2	2	2	2	2	2	2	2
8	1	1*	1	1*	1	1	1*	1*	1/	1*	1//	2	1*	2	2
9	1	3	2	1*	1	3	2	3	2	1*	2	1*	3	2*	1
10	2	2	2	1	1**	3	3	3	2	2	1	2	2	2	2
11	1	2	1*	2	2	2	2	1*	2	2	2	2	2	2	2
12	1*	1	1	1*	2	2	1	1*	2	1*	2	2	2	2	2

* means a rejection, ** means both agents rejected.
/ means a low play by an H type. // means two low plays by H types.

Principals 1-4 were in the first session in the treatment, 5-8 were in the second session, and 9-12 were in the third session.

AGGREGATED MENU PROPOSALS BY PERIOD

	Period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Treatment 1															
Menu 1	12	10	9	4	3	3	2	3	2	1	3	2	2	1	5
Menu 2	0	2	3	4	6	4	4	4	1	3	1	1	4	2	1
Menu 3	0	0	0	4	3	5	6	5	9	8	8	9	6	9	6
Treatment 2															
Menu 1	7	5	5	6	6	4	5	7	3	4	3	3	2	1	2
Menu 2	3	5	4	5	5	4	5	2	8	7	7	8	7	10	8
Menu 3	2	2	3	1	1	4	2	3	1	1	2	1	3	1	2

PRINCIPAL % OF TOTAL EARNINGS

	Period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Treatment 1	66	51	53	36	37	41	43	46	37	37	37	41	46	38	38
Treatment 2	63	57	54	55	58	53	55	52	55	55	53	54	51	53	53

APPENDIX 3 – INDIVIDUAL AGENT CHOICES BY PERIOD

TREATMENT 1 (500)

	Period														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
L1	1/2	2/2	1/2	1/2	2/3	3/2	2/3	3/2	3/2	2/3	1/3	3/2	3/2	3/2	1/3
L2	1/3	2/2	1/2	1/3	2/3	2/3	1/3	2/3	3/2	2/3	3/2	3/2	3/2	3/2	3/2
L3	1/2	1/3	2/2	2/3	3/2	2/3	3/2	2/3	1/3	2/2	3/2	3/2	3/2	3/2	3/2
L4	1/3	1/3	1/3	2/3	1/3	3/2	1/3	3/2	3/2	3/2	3/2	3/2	3/2	3/2	1/3
L5	1/2	1/2	1/2	1/3	1/3	1/3	2/3	2/3	2/3	3/2	1/3	3/2	2/3	3/2	3/2
L6	1/3	1/3	1/3	2/3	1/3	2/3	3/2	2/3	3/2	3/2	2/3	3/2	2/3	3/2	1/3
L7	1/2	1/3	1/3	1/3	2/3	1/3	2/2	2/3	1/3	3/2	3/2	3/2	2/3	3/2	1/3
L8	1/2	1/2	1/2	3/2	2/3	1/3	3/2	2/3	3/2	3/2	2/3	3/2	3/2	3/2	3/2
L9	1/2	1/3	1/3	2/3	2/3	3/2	2/3	2/3	3/2	3/2	3/2	3/2	3/2	1/3	3/2
L10	1/1	1/2	2/2	3/2	2/2	1/2	3/2	3/2	3/2	3/2	1/2	3/2	2/2	3/2	3/2
L11	1/3	1/2	1/3	3/2	2/3	3/2	3/2	3/2	3/2	3/2	3/2	3/2	3/2	2/3	1/3
L12	1/2	1/3	2/3	3/2	1/3	3/2	3/2	2/3	3/2	2/3	1/3	3/2	2/3	3/2	3/2
H1	1/2	2/1	1/1	1/1	3/1	2/1	3/1	1/1	1/1	3/1	3/1	3/1	3/1	3/1	3/1
H2	1/1	2/1	1/1	2/2	1/1	2/1	3/1	3/1	3/1	2/1	3/1	3/1	1/1	2/1	1/1
H3	1/1	1/1	2/1	1/3	3/1	2/1	2/1	3/1	3/1	3/1	3/1	3/1	3/1	2/1	3/1
H4	1/2	1/3	1/3	2/1	3/1	2/1	3/1	1/3	3/1	3/1	1/1	3/1	1/1	3/1	1/3
H5	1/1	1/1	1/1	2/1	2/2	1/1	1/1	1/1	1/1	3/1	3/1	1/1	2/2	3/1	1/1
H6	1/1	1/1	1/1	3/1	2/2	3/1	1/1	3/1	3/1	3/1	3/1	3/1	3/1	3/1	3/1
H7	1/2	1/3	1/3	1/3	2/1	3/1	2/1	3/1	3/1	1/3	3/1	1/3	3/1	3/1	1/3
H8	1/1	1/3	1/1	1/1	2/1	2/1	2/1	1/1	2/1	1/1	1/1	3/1	3/1	3/1	2/1
H9	1/1	1/1	2/1	3/1	2/1	3/1	3/1	3/1	3/1	2/1	3/1	2/1	2/1	1/1	3/1
H10	1/2	1/2	1/2	3/1	1/2	3/1	3/1	1/1	3/1	3/1	3/1	1/2	1/2	3/1	3/1
H11	1/3	1/3	1/3	2/1	3/1	3/1	2/1	1/1	3/1	3/1	3/1	2/1	2/1	3/1	1/1
H12	1/1	1/1	2/1	3/1	3/1	1/1	3/1	3/1	3/1	3/1	3/1	1/1	1/1	2/1	3/1

In this table, "x/y" indicates Menu x and response y, where 1 means "high effort", 2 means "low effort" and 3 means rejection.

TREATMENT 2 (250)

	Period														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
L1	1/2	2/2	3/2	2/3	2/3	1/3	3/2	1/3	1/2	2/2	2/2	1/3	2/2	1/2	2/2
L2	2/2	2/2	3/2	2/2	2/2	3/2	2/2	2/2	2/2	2/2	1/3	2/2	1/2	2/2	1/3
L3	3/2	1/2	2/2	1/2	1/2	2/2	2/2	2/2	2/2	2/2	2/2	1/3	2/2	3/2	3/2
L4	3/2	3/2	3/2	3/2	1/2	1/2	2/2	1/2	3/2	3/2	1/2	2/2	3/2	2/2	2/2
L5	3/2	1/2	3/2	2/2	1/2	1/2	1/2	2/2	2/2	2/2	2/2	2/2	2/2	2/2	3/2
L6	1/1	1/2	1/2	1/2	1/2	1/2	2/2	1/2	2/2	2/2	2/2	2/2	2/2	2/2	2/2
L7	1/2	2/2	1/2	2/2	2/2	3/2	1/2	1/2	1/2	2/2	3/2	2/2	2/2	3/2	2/2
L8	1/2	1/2	1/2	1/2	2/2	1/2	2/2	2/2	1/2	1/2	2/2	2/2	1/2	2/2	3/2
L9	1/2	2/2	2/2	1/2	1/2	2/2	1/2	1/2	2/2	1/2	2/2	2/2	3/2	2/2	2/2
L10	1/1	2/2	1/2	1/2	1/2	2/2	3/2	3/2	2/2	2/2	2/2	1/2	2/2	2/2	2/2
L11	1/3	2/2	2/2	2/2	2/2	3/2	2/2	3/2	2/2	2/2	1/2	2/2	3/2	2/2	2/2
L12	1/2	1/2	1/2	1/2	1/3	2/2	3/2	1/3	2/2	1/3	2/2	1/3	2/2	2/3	2/2
H1	1/1	2/1	1/1	2/1	1/1	2/1	1/1	1/1	2/1	3/1	3/1	1/1	1/1	1/1	1/1
H2	1/1	2/1	3/1	3/1	1/1	2/1	1/1	3/1	3/1	2/1	3/1	3/1	2/1	2/1	2/1
H3	1/2	1/1	2/1	2/1	3/1	2/1	3/1	3/1	2/1	1/1	2/1	1/1	3/1	2/1	3/1
H4	2/1	3/1	1/1	1/2	3/1	3/1	2/1	1/1	1/1	1/1	2/1	3/1	2/1	3/1	2/1
H5	2/1	1/1	2/1	1/1	2/1	3/1	1/2	1/3	1/2	1/3	1/2	2/1	2/1	2/1	2/1
H6	2/1	1/1	3/1	1/3	2/1	1/3	1/3	1/3	1/2	2/1	1/2	2/1	3/1	2/1	2/1
H7	3/1	2/1	1/1	2/1	1/1	1/1	1/3	1/1	2/1	2/1	3/1	2/1	1/3	2/1	2/1
H8	1/1	1/3	2/1	2/1	1/1	1/1	1/3	1/1	2/1	2/1	2/1	2/1	3/1	2/1	2/1
H9	2/1	3/1	1/3	1/3	2/1	3/1	2/1	3/1	2/1	1/3	2/1	2/1	2/1	2/1	2/1
H10	2/1	3/1	2/1	1/3	1/3	3/1	2/1	1/3	2/1	1/3	2/1	2/1	2/1	2/1	1/1
H11	1/1	2/1	2/1	1/1	2/1	3/1	1/1	1/1	2/1	2/1	1/1	2/1	2/1	2/1	2/1
H12	1/1	1/1	1/1	2/1	2/1	2/1	2/1	3/1	2/1	2/1	2/1	2/1	2/1	2/1	1/1

In this table, “x/y” indicates Menu x and response y, where 1 means “high effort”, 2 means “low effort” and 3 means rejection.

APPENDIX 4 - INDIVIDUAL AGENT BEHAVIOR

L types

	L1		**L2**		**L3**		**L4**	
Session	M1	M2	M1	M2	M1	M2	M1	M2
1	2/5	3/4	3/4	4/5	2/3	3/5	6/6	1/1
2	4/7	4/4	5/5	5/5	6/7	3/4	1/4	3/3
3	3/4	4/4	0/4	0/3	3/4	2/2	3/4	4/4
4	3/6	2/7	2/3	0/10	1/4	0/8	0/4	0/4
5	0/4	0/8	0/7	0/8	0/5	0/8	0/8	0/6
6	0/6	0/8	0/5	0/8	1/2	0/10	4/8	1/6

H types

	H1		**H2**		**H3**		**H4**	
Session	M1	M2	M1	M2	M1	M2	M1	M2
1	0/5	0/2	0/5	0/5	1/3	0/4	4/7	0/2
2	0/9	0/3	0/4	0/1	6/7	0/2	1/7	0/5
3	0/3	0/5	0/7	0/0	3/5	0/4	0/5	0/2
4	0/9	0/4	0/3	0/6	0/4	0/6	0/5	0/5
5	2/7	0/7	4/7	0/6	2/6	0/7	2/6	0/8
6	3/3	0/9	4/5	0/8	0/5	0/9	0/4	0/10

X/Y in each cell refers to # of times the agent chose rejection/# of times menu was offered.

APPENDIX 5

Lagged Rejections and menu choice

Dependent Variable

Independent variables	Menu Up (All Data) (1)	Menu Up (T1 only) (2)	Menu Up (T2 only) (3)	Menu Down (All Data) (4)	Menu Down (T1 only) (5)	Menu Down (T2 only) (6)
Lagged rejection	**1.360** **(7.06)**	**1.539** **(5.71)**	**1.467** **(3.78)**	**-.549** **(-2.41)**	-.504 (-1.67)	-.648 (-1.39)
Period 2	.083 (0.19)	-.407 (-0.63)	.617 (0.95)	-.925 (-1.88)	-8.152 (-0.00)	-.404 (-0.62)
Period 3	-.328 (-0.71)	-.894 (-1.35)	.253 (0.37)	-.672 (-1.43)	-1.000 (-1.40)	-.414 (0.63)
Period 4	.509 (1.17)	1.020 (1.64)	-.401 (-0.49)	-.310 (-0.71)	-1.041 (-1.47)	.252 (0.40)
Period 5	-.230 (-0.51)	-.031 (-0.05)	-.931 (-1.15)	.033 (0.08)	.427 (0.77)	-.621 (-0.86)
Period 6	.379 (0.89)	-.021 (-0.04)	.800 (1.23)	-.164 (-0.39)	.210 (0.37)	-.823 (-1.14)
Period 7	-.345 (-0.74)	-.048 (-0.08)	-8.75 (-0.00)	-.322 (-0.74)	-.582 (-0.95)	-.059 (-0.09)
Period 9	.259 (0.61)	.212 (0.36)	.378 (0.57)	-.389 (-0.88)	-.498 (-0.83)	-.251 (-0.37)
Period 10	-.139 (-0.29)	-.289 (-0.42)	.075 (0.10)	-.352 (-0.82)	-.289 (-0.51)	-.447 (-0.68)
Period 11	-.091 (-0.20)	-.054 (-0.08)	.262 (0.39)	-.440 (-1.00)	-.251 (0.44)	-.749 (-1.03)
Period 12	.140 (0.31)	-.062 (-0.10)	.444 (0.67)	-.333 (-0.77)	-.553 (-0.92)	-.103 (-0.16)
Period 13	.230 (0.50)	.174 (0.27)	.398 (0.56)	-.508 (-1.16)	-.382 (-0.66)	-.681 (-0.97)
Period 14	.188 (0.42)	.488 (0.81)	-.279 (-0.36)	-.762 (-1.62)	-1.061 (-1.56)	-.479 (-0.70)
Period 15	-.477 (-0.89)	-.599 (-0.73)	-.371 (-0.47)	-0.59 (-0.14)	.424 (0.77)	-0.871 (-1.18)
N	336	168	168	336	168	168
LL	-141.7	-71.7	-58.1	-151.4	-75.8	-64.4

Z-statistics are in parentheses. **Bold** indicates significance at the 5% level, two-tailed test.

FIGURE 1
Proposals over Time (Treatment 1)

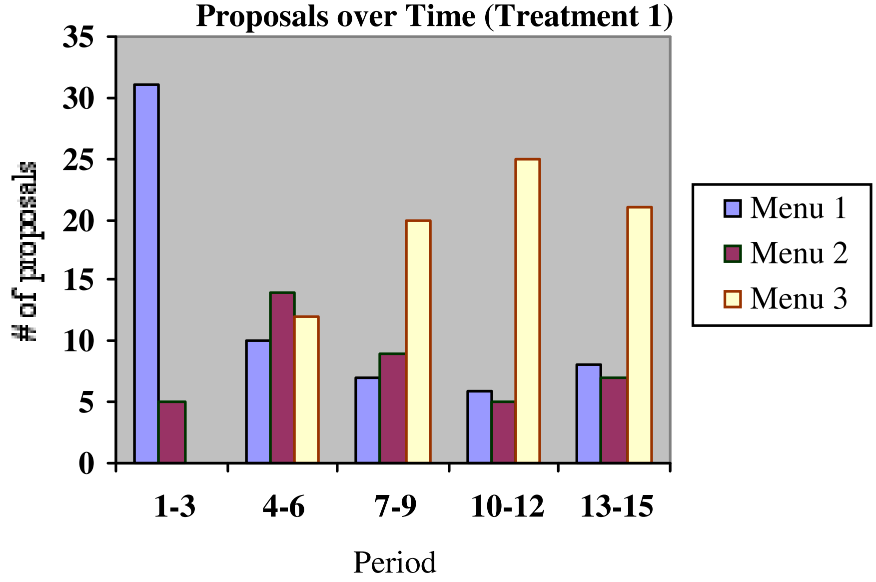


FIGURE 2
Proposals over Time (Treatment 2)

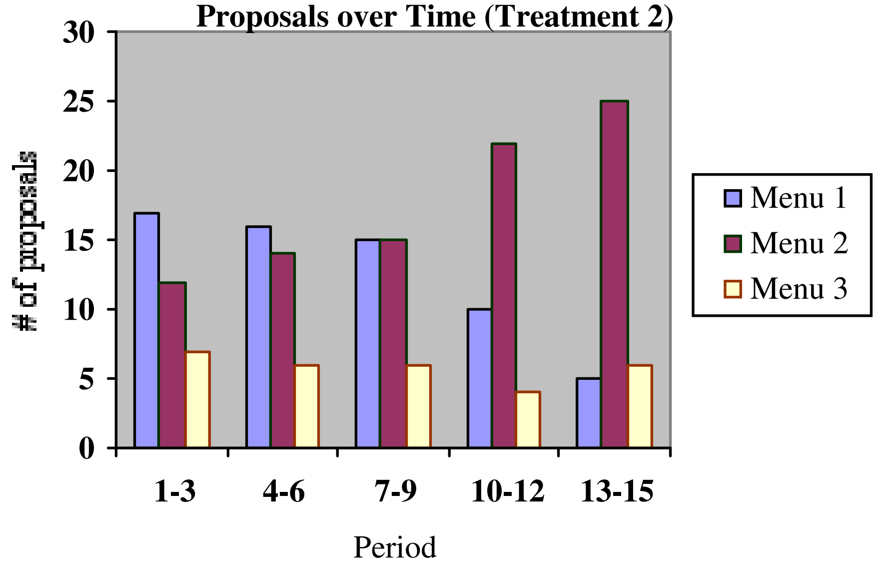
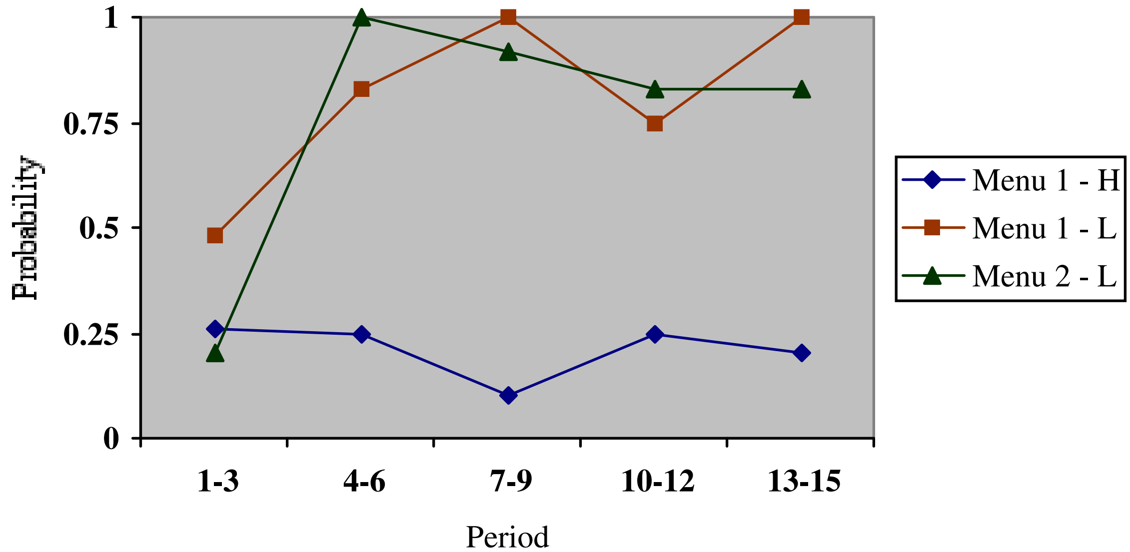
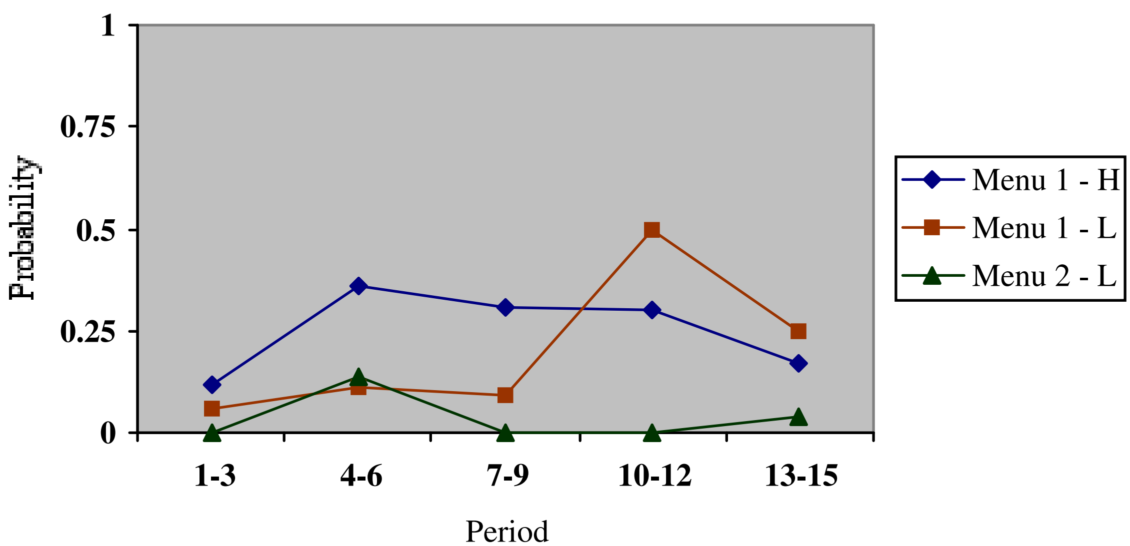


FIGURE 3
Rejection Rates over Time (Treatment 1)



Rejection rates were always 0% for Menu 2 – H, Menu 3 – H, and Menu 3 – L.

FIGURE 4
Rejection Rates over Time (Treatment 2)



Rejection rates were always 0% for Menu 2 – H, Menu 3 – H, and Menu 3 – L.