

## **Bargaining Power and Observability in a Repeated Managerial Control Setting**

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## **1. Introduction**

In theory, repeated interaction affords agents the opportunity to signal their intended future behavior through their prior actions. That is, agents can form reputations for acting in a particular manner given a particular set of circumstances. The attempt to form reputations is considered an important explanatory factor of organization and individual behavior and has been studied in contexts such as interactions among firms, among employees within a firm and between a firm and its customers.

Since the basis of reputation is the use of prior actions as a signal of future intent, the degree to which prior actions are observable is a key determinant of the nature of reputation formation (Kreps 1990). However in practice, rarely if ever are agents' actions observed perfectly by others. Such settings, which feature repeatedly interacting agents with limited ability to infer each other's actions, have particular relevance to the study of managerial control. Superiors wish to induce subordinates to take actions that do not maximize the subordinate's short-term utility. Superiors and subordinates typically interact repeatedly within the firm. Obfuscation of subordinates' actions is inherent, because monitoring is costly to the organization.

The standard approach for investigating managerial control issues is that of theoretical analysis. However, such analyses are of lesser value in repeated interaction settings, because repeated interaction often gives rise to non-unique equilibrium solutions. Therefore, the use of empirical techniques such as a controlled laboratory investigation seems particularly useful in obtaining an understanding of the role of observability in repeated interaction settings. However, despite the pertinence of observability and repeated interaction to the study of managerial control, and the limited

role that analytics can play in furthering our understanding, there is a dearth of empirical evidence on this issue. In fact, we are aware of no laboratory study that addresses repeated superior-subordinate interactions while manipulating the degree to which each participant can observe the actions of the other.<sup>1</sup> We address this deficiency in the empirical data by designing a simple, focused experiment that provides evidence on the effect of perfect *ex post* verification in a managerial control setting. Additionally we manipulate the payoff structure of the game in a manner designed to proxy for relative bargaining power of superior and subordinate. This latter manipulation provides a robustness check and, more importantly, allows us to explore evidence on the interaction of observability and bargaining power.

The setting we employ is parsimonious, yet captures several important attributes of managerial control: (1) the diffusion of information within the firm, (2) the owner's inability to contract on all possible contingencies, and (3) the repeated nature of intra-firm relationships. Our design, which is derived from several theoretical analyses of firm structure (Aghion and Tirole 1997; Baker, Gibbons and Murphy 1999; James 2000), consists of an owner and a manager. The owner may either delegate or not delegate a project selection choice to the manager. If delegated the choice, the manager can respond by using his or her superior private information to the benefit the owner, or by acting selfishly, at the expense of the owner. In all treatments, the owner and the manager interact repeatedly for a randomly determined number of periods. In half the treatments, the owner becomes aware of the manager's private information, *ex post*. In the other half of the treatments, the owner never becomes aware of the manager's private information.

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<sup>1</sup> Sainty (1999) and Schwartz and Young (2002) manipulated observability of the actions of hierarchical equals in repeated settings.

This is our primary manipulation and provides evidence on the effects of greater observability in a repeated managerial control setting. We posit that greater observability, when combined with repeated play, will mitigate the efficiency loss to the owner attributable to the misaligned goals of the owner and manager.

We also manipulate the payoff structure of the setting. In one treatment, the owner receives a greater stage game payoff from not delegating than from delegating to a selfish manager.<sup>2</sup> In the other treatment, the owner receives a lesser stage game payoff from not delegating than from delegating to a selfish manager. Consistent with the literature on bargaining experiments, we interpret this second manipulation as determining the relative bargaining power of the owner and the manager (Binmore, Morgan, Shaked and Sutter 1991; Kahn and Murnighan 1993). Prior experimentation has shown that bargaining power may have a profound effect on behavior in a single period setting. In our hypothesis development, we derive predictions for both the main effect and interaction effect of our manipulation of bargaining power in our repeated setting. The most notable of these hypotheses is that the manipulation of bargaining power will have a lesser effect in the presence of *ex post* verification than in its absence.

We contribute to the literature in several ways. First, we examine the consequences of *ex post* verification in a management control setting. The effects of verification, although widely addressed in experimental market studies of auditing, thus far have received limited attention in intra-firm settings. The two studies that squarely addressed the role of verification in an intra-firm management control setting have produced inconsistent results. Sainty (1999) found noticeable efficiency gains with the introduction of a less noisy but still imperfect feedback mechanism, while Schwartz and

Young (2002) found very modest efficiency gains due to verification in their combined reporting and investing environment. The conflicting results may be due to the greater complexity of the Schwartz and Young (2002) setting. Further, both the Sainty (1999) and Schwartz and Young (2002) experiments were games played between subordinate managers, rather than between a superior (owner) and subordinate (manager). In contrast, we employ a simpler bilateral setting that is centered on an owner-manager interaction.

Second, we manipulate the relative bargaining positions of owner and manager in a repeated play setting. This secondary manipulation is motivated by the observation that bargaining power rarely resides entirely with the owner and relative bargaining power has rarely been manipulated in a repeated play setting.<sup>3</sup> We are particularly interested in how bargaining power interacts with verification in influencing attempts to form reputations and the ability to maintain them. Finally, to deepen our understanding of the role of verification in the reputation formation process itself, we examine the use of history contingent strategies in formulating implicit arrangements within owner-manager pairs.

Our results indicate that the addition of verification induces owners to place greater trust in managers, but only in settings where owners have the superior bargaining position. Surprisingly, the main effect of verification on the level of cooperative behavior by managers is minimal. Further, analysis of history contingent strategies reveals that when owners had a favorable bargaining position, they tried to enforce an efficient, equal outcome. However, when owners had a less favorable bargaining position, they simply acquiesced to the managers, allowing them to take a larger share of the surplus. Thus, our most noteworthy finding is that the effectiveness of verification in mitigating

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<sup>2</sup> The manager always prefers the owner to delegate.

management control issues is highly contingent on the relative bargaining positions of owner and manager.

The paper is organized as follows. Section 2 presents the experimental design and equilibrium analysis. Hypotheses are developed in Section 3. Section 4 analyzes the data and section 5 discusses the results and concludes the paper.

## 2. Experimental Design and Game-Theoretic Analysis

The game consists of two individuals, an owner and a manager. The owner moves first by deciding whether to delegate a project choice (**Delegate**) or not (**Not Delegate**) to the manager. If the owner chooses **Not Delegate**, a fallback project with deterministic, non-transferable, payoffs to the owner and manager is chosen by default. If the fallback project is chosen, the game ends. If the owner chooses **Delegate**, the manager chooses between Project A and Project B. For simplicity, we assume that only Project B has state-contingent payoffs, and that there are only two (equally likely) states of nature, denoted  $s_1$  and  $s_2$ . Before choosing between Projects A and B, the manager alone obtains perfect information regarding a state realization that is payoff relevant to both parties. The distribution of the state is common knowledge. Figure 1 displays a one-shot version of the game (stage game) in extensive form.

*[Insert Figure 1 about here]*

Figure 1 indicates that the fallback payoff to the owner, labeled  $O_F$ , determines the subgame perfect equilibrium in the stage game. The manager has a dominant strategy in the subgame to choose Project A no matter the state. Anticipating this, the owner would optimally choose **Delegate** if  $O_F < 30$  and **Not Delegate** if  $O_F > 30$ . Therefore,

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<sup>3</sup> In a traditional contracting approach, the owner is effectively assigned all of the bargaining power by allowing him or her to move first and to fully commit.

relatively small changes in the fallback payoff to the owner can have substantial effects on social welfare. In the experiment, we set  $O_F$  to 20 and 40 in the *Low* and *High Fallback* treatments, respectively. When  $O_F = 20$ , the equilibrium total utility of the owner and the manager is  $30 + 90 = 120$ , which maximizes social efficiency. When  $O_F = 40$ , equilibrium total utility is  $40 + 20 = 60$ , which is quite inefficient.[R1]

It is easy to see the owner would wish to delegate the decision to the manager if he or she could be trusted to be obedient. By obedient, we mean choosing Project A in  $s_1$  and Project B in  $s_2$ . If the manager were obedient, the owner would receive an expected utility of  $.5(30) + .5(90) = 60 > O_F$  and the manager would receive  $.5(90) + .5(30) = 60$ . Of course, this is not equilibrium behavior in the stage game, because the best response of the manager is to choose Project A in both  $s_1$  and  $s_2$ .

The preceding analysis illustrates that under both of our parameterizations the owner would prefer to delegate to an obedient manager, but only in the low fallback treatment would the owner prefer to delegate to a selfish manager. It is for this reason that we interpret the level of the fallback payoff as indicating the relative bargaining power of the owner and manager.

In all treatments of the experiment, the game described above is played repeatedly between the same two individuals. The interaction lasts for a minimum number of rounds with certainty and then proceeds to each successive round after the minimum with probability  $q$ . The treatments differ with respect to: (1) whether the owner is informed of the state realization subsequent to the completion of a round and (2) the magnitude of the owner's payoff from the fallback project. We describe below the equilibrium implications of our treatment manipulations and the administration of the experiment.

### *Analysis of the Repeated Game with Ex Post Verification*

When there is *ex post* verification the owner learns, after the manager has chosen Project A or B, whether the state was  $s_1$  or  $s_2$ . In the experiment we set  $q = .9$ , which implies that there exist equilibria in which the owner and manager each receive 60 in expectation per period. These equilibria attain full efficiency (total utility of 120). An equilibrium strategy profile that achieves this level of efficiency is as follows: (1) the owner begins by choosing **Delegate**, (2) the manager selects Project A in  $s_1$  and Project B in  $s_2$  and (3) the owner punishes any deviations from this strategy by choosing **Not Delegate** for a sufficient number of periods.

This efficient equilibrium exists in both the *Low* and *High Fallback* treatment, but only when there is verification. It is important to note that this equilibrium is one of many under a verification treatment, and further that the level of the fallback payoff might influence the evolution of play. The potential influence of the fallback payoffs on equilibrium selection will be discussed in greater detail below.

### *Analysis of the Repeated Game without Verification*

Assume that the game proceeds as described above, except that in the setting without verification the owner never becomes aware of the state realization. Now the owner is uncertain whether the manager has chosen Project A in the owner's best interest ( $s_1$ ), or whether the manager has chosen Project A selfishly ( $s_2$ ). In this setting, there exist equilibria in which the owner chooses **Delegate** in some periods and the manager acts obediently in some periods. However, unlike the verification treatment, delegation by the owner and obedience by the manager cannot occur in *every* period in equilibrium. This characteristic is not an artifact of our experimental design. In any situation that does not

admit a history arising uniquely from manager disobedience, as in our *No Verification* treatments, the owner must be willing to “punish” (choose **Not Delegate**) in response to a history profile that may have arisen from manager obedience. Hence, use of the fallback project must occur along the equilibrium path in any equilibrium wherein the manager acts obediently in some periods. The choice of **Not Delegate** is a cost, in theory, attributable to the absence of verification and also suggests that the level of fallback payoffs to the owner will play a greater role in the *No Verification* than in the *Verification* treatment. In a more general sense, we are predicting that bargaining power becomes more important when actions are obscured.

#### *Administration of the Experiment*

The experiment involved 88 undergraduate students from business courses at a large university. We used a 2 X 2 full factorial design manipulating whether or not verification is present and whether the owner’s fallback payoff is high or low. Two experimental sessions were held in each of the four treatments. Figure 2 describes the experimental design, including the number of subject-pairs.

*[Insert Figure 2 about here]*

Each experimental session was conducted as follows. A group of subjects was recruited. They reported to a room where they were randomly assigned subject numbers and seating assignments. Instructions were distributed to the subjects. The instructions explained the task, how decisions were made and recorded, and the interrelated nature of decisions. Subjects were also informed of how the amount of money they earned depended on their own decisions and those of the other participants. After the subjects read the instructions, they were read aloud by the experimenter. Sample instructions

appear in Appendix B. After the instructions were read, but before the actual decision periods began, subjects completed a questionnaire to ensure that they understood how to compute outcomes and payoffs for themselves and other participants. All experimental tasks were computerized and subjects interacted anonymously.

In the initial part of the experiment, subjects played four practice periods. Before the first practice period, subjects were randomly assigned as either a Position 1 participant (owner) or a Position 2 participant (manager).<sup>4</sup> Subjects were informed that, when the actual periods began, they would remain as either a Position 1 or Position 2 participant. They were also informed that after the four practice periods they would be randomly re-matched with another participant, with the guarantee that it would not be the same participant that they were matched with during the practice periods. Finally, they were informed that balances from the practice periods would not count toward final cash payments.

Each period of the experiment proceeded exactly as described above. Subjects assigned to the **Low Fallback** treatments (*NV-LF* and *V-LF*) had fallback payoffs of 20 for both owner and manager. Subjects assigned to the **High Fallback** treatments (*NV-HF* and *V-HF*) had fallback payoffs of 40 and 20 for owner and manager, respectively. Owners in the verification treatments (*V-LF* and *V-HF*) were informed of the actual state realization at the end of each period, while owners in the *No Verification* treatments (*NV-LF* and *NV-HF*) were never informed of the true state. In all treatments, owners could infer managers' project choices from their own payoffs.

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<sup>4</sup> The terms Position 1 and Position 2 were used in the administration of the experiment in order to keep the instructions as context-free as possible. For expositional ease, throughout this paper subjects are frequently referred to as owners and managers.

### *Session Length*

Each session consisted of 20 periods with certainty and a 90 percent chance of continuation thereafter. This was explained to subjects using the heuristic of a bowl with 9 white chips and 1 red chip where the draw of a red chip would terminate the experiment. Prior to administering the sessions, a random draw was made consistent with the explanation of the stopping rule to be used for all sessions. The outcome of this draw was 9 additional periods for a total of 29 periods in a session.

### *Project Payoffs*

All project payoffs were in probability points, in an attempt to induce risk neutrality (Berg, Daley, Dickhaut and O'Brien 1986). In order to determine subjects' actual payoffs, a uniformly distributed number from 1 to 100 was drawn at the conclusion of each decision period. If this number was less than or equal to the subject's point payoff, the subject was paid \$2.00 for that period. If this number was greater than the subject's project payoff, the subject was paid nothing. In our analysis of the results, the probability points are referred to as subject earnings, again for ease of exposition.

## **3. Hypotheses Development**

Our primary variables of interest are the relative frequencies of delegation and manager obedience. We also analyze the relation between the history of play and the current period strategy of the owner, because we wish to better understand the manner in which a superior may use repeated play to discipline the actions of a subordinate.

### *Delegation*

We expect that the presence of verification will increase the owner's willingness to delegate. The rationale behind this conjecture is straightforward. In the *Verification*

treatments, delegation in *every* period is sustainable in equilibrium. However, in the *No Verification* treatments, delegation in *every* period is only equilibrium behavior in the **Low Fallback** treatment, but in this equilibrium the manager best responds by never being obedient. Therefore, this is an equilibrium we would expect the owners would try to avoid. In the *No verification* treatments, the manager must use the fallback payoff along the equilibrium path in order to induce obedience by the manager in some periods. Further, without verification, the owner must decide whether to punish based on his or her own payoffs, from which manager obedience cannot be perfectly inferred. Hence, we expect that verification will increase the frequency of delegation. There is, however, ambiguity in the exact nature of verification's efficacy in facilitating useful reputations. Even when verification is present, there remains the repeated stage game equilibrium, which for the **High Fallback** treatment implies no delegation. Further, verification tends to highlight defections, which might cause irreparable breakdowns in cooperative behavior, similar to that seen in Schwartz and Young's (2002) repeated-interaction, verification treatment.

We expect the level of the fallback payoff to affect the owner's willingness to choose **Delegate**, for two reasons. First, in the *No Verification* treatment, the use of the fallback project along the equilibrium path is necessary in order to support any equilibrium that features *any* delegation and obedience. That is, without verification the owner cannot use the fallback project *solely* as an off-equilibrium threat to obtain obedience by the manager. In the **High Fallback** treatments, punishment (not delegating) is less costly than in the **Low Fallback** treatments. Hence, we expect owners to be more willing to punish managers by not delegating in the **High Fallback** treatments. Second, in

both the *Verification* and *No Verification* treatments, attempts to cooperate might fail, in which case the equilibrium that remains is repetition of the stage game equilibrium. If this breakdown in cooperation occurs, in the ***High Fallback*** treatments no delegation would ever occur, while in the ***Low Fallback*** treatments delegation would occur in every period.

Further, we expect the effects of the fallback payoffs to be muted in the *Verification* treatments. This is motivated by the observation that there exists a fully efficient, equal distribution equilibrium in the *Verification* treatments, which might be focal, while there exists no equilibrium with all these properties in the *No Verification* treatments. If play in the *Verification* treatments gravitates toward the equal distribution, fully efficient equilibrium, punishment will rarely be observed. Hence, we predict that in the *Verification* treatments the fallback payoff would have lesser effect on the frequency of delegation. This leads to our first set of hypotheses.

**H1A:** Verification will increase delegation (*DEL*):

$$DEL_{NV} < DEL_V.$$

**H1B:** Lower fallback payoffs will increase delegation:

$$DEL_{HF} < DEL_{LF}.$$

**H1C:** Verification will mute the effect of the fallback payoffs on delegation:

$$(DEL_{V-LF} - DEL_{V-HF}) < (DEL_{NV-LF} - DEL_{NV-HF}).$$

### *Managers' Obedience*

We expect the manager to be more obedient when there is verification. In the *Verification* treatments, the manager knows disobedience will be detected with certainty, which allows the owner to punish with precision. For this reason there exist equilibria that feature manager obedience in every round in the presence of verification. In the *No Verification* treatments, the manager cannot be obedient in every period along the

equilibrium path. The proof of this is simple: if the manager were always obedient the owner would best respond by always delegating. However, as explained previously, an owner who always chooses **Delegate** cannot induce obedience by the manager.

Therefore, manager obedience in every round cannot be part of an equilibrium in the *No Verification* treatments. This again is not an artifact of our design, but generalizes to any setting wherein manager disobedience does not admit a unique history profile. Using the same rationale as our expectations regarding owner delegation, we expect the manager's level of obedience to be affected by the level of the fallback payoff, but for that effect to be muted in the *Verification* treatments. This leads to our second set of hypotheses.

**H2A:** Verification will increase manager obedience (*OBD*):

$$OBD_{NV} < OBD_V.$$

**H2B:** Higher fallback payoffs will increase manager obedience:

$$OBD_{LF} < OBD_{HF}.$$

**H2C:** Verification will mute the effect of the fallback payoff on manager obedience:

$$OBD_{V-LF} - OBD_{V-HF} < OBD_{NV-LF} - OBD_{NV-HF}.$$

### *History contingent behavior*

We expect that, in order to induce obedience, the owner must sometimes choose **Not Delegate** after a defection, whether known (in *Verification* treatments) or suspected (in *No Verification* treatments). Therefore, in the *Verification* treatments we would expect a lower probability of the owner choosing **Delegate** following a period where (1) the manager chose Project *A* and (2) the owner learned that  $s_2$  obtained. However, defections cannot be detected in the *No Verification* treatments — the history of play provides only imperfect information regarding manager defection. When the owner obtains a payoff of 30, the owner in essence must decide whether it is sufficiently likely that the manager has been disobedient to deserve punishment. Therefore, we expect less delegation in a period

following an owner payoff of 30 than in a period following a payoff of 90. Further, we would expect the effect of prior play to be muted in the *No Verification* treatments relative to the *Verification* treatments. This leads to our third set of hypotheses.

**H3A:** In the *Verification* treatments, owners will be more likely to delegate in the period that follows known manager obedience (*OBD*) than in one that follows known disobedience (*DIS*):

$$DEL_t|OBD_{t-1} > DEL_t|DIS_{t-1}.$$

**H3B:** In the *No Verification* treatments, owners will be more likely to delegate in the period that follows their receipt of a payoff of 90 (where obedience must have occurred) than in one that follows a payoff of 30 (where obedience might not have occurred):

$$DEL_t|OWNPAY_{t-1} = 90 > DEL_t|OWNPAY_{t-1} = 30.$$

**H3C:** The differential propensity to delegate in the *Verification* treatments when the owner knows the manager must have been obedient versus *must have been* disobedient is greater than in the *No Verification* treatments when the manager must have been obedient versus *might have been* disobedient:

$$(DEL_t|OBD_{t-1} - DEL_t|DIS_{t-1}) > (DEL_t|OWNPAY_{t-1} = 90 - DEL_t|OWNPAY_{t-1} = 30).$$

#### 4. Results

We begin our analysis of the results by examining the delegation behavior of the owners. The willingness of owners to delegate might be indicative of the degree to which owners expect managers to act obediently. The relative frequency of delegation is presented in Table 1.

*[Insert Table 1 about here]*

Inspection of Table 1 reveals that the treatment means are in the direction expected for Hypotheses 1A and 1B. That is, both verification and lower fallback payoffs are associated with higher relative frequencies of delegation. As a statistical test of these associations, we conduct a fixed-effects ANOVA correcting for unequal sample sizes, using the relative frequency of delegation across all rounds *for each owner* as the dependent variable and verification and fallback payoff and their interaction as the

independent variables. The  $p$ -values for *Verification* and *Fallback* are .02 and .001, respectively, which support Hypotheses 1A and 1B.

What is perhaps of greater interest is the significance of the interaction between fallback payoff and verification, which has a  $p$ -value of .05. This result supports Hypothesis 1C. However, the interaction is not exactly of the nature that we expected. The fallback payoff generally has a significant effect regardless of the verification treatment, but verification has a significant effect only in the ***High Fallback*** treatment. This finding indicates that the fallback payoff is the more important of the factors. This is contrary to what is suggested by our theory development, wherein verification allowed for an equal distribution, full delegation equilibrium at both fallback levels while the fallback manipulation was expected to be more important in equilibrium selection in the *No Verification* treatments.

*[Insert Table 2 about here]*

Table 2 presents the relative frequency of obedience, calculated as the number of times a manager chose Project *B* in  $s_2$  divided by the number of times the owner delegated and  $s_2$  occurred. We test the significance of the difference in means using an ANOVA appropriate for unequal sample sizes, where the unit of observation is an owner-manager pair. That is, an average rate of obedience is calculated across all trials for each pair. Given the difference in the means in Table 2, the statistical insignificance of the results in the ANOVA might be surprising. The statistical insignificance occurs because owners in *V-HF* cut off disobedient managers from delegation very early (disobedience therein being perfectly inferable), and in *NV-HF* owners gave managers very little opportunity to establish a reputation given the noisiness found in that treatment (three

managers were given no opportunity at all to be obedient in *NV-HF*). Therefore, pairs with disobedient managers have mean observations based on as few as one period, and are in a sense over-weighted when each pair is reduced to a single observation.<sup>5</sup> Hence, while the overall mean level of obedience in the **High Fallback** treatments is more than double that in the **Low Fallback** treatments, the *average manager's* level of obedience does not differ significantly.

*[Insert Figure 3 about here]*

In Figure 3 we display graphs of the relative frequencies of delegation and obedience through the course of the experiment. The data is aggregated for each five-period segment of the experiment, except for the last segment, which is four periods. The most discernable trend is the decreasing relative frequency of delegation found in *V-HF* over the last half of the experiment. The pattern fits well with the observation that managers in *V-HF* were simply not that obedient, given what might have been expected from the theory development. We observe that owners, who in *V-HF* are faced with a perfect revelation of the manager's behavior, began to retaliate. What we do not observe is an increase in obedience from the managers in *V-HF*. In fact, the relative frequency of obedience appears to trend downward. One note of caution is necessary. The relative frequency of obedience in the high fallback treatments is derived from a small number of observations and therefore abrupt changes between two five-period segments may not be that meaningful. Inspection of the graphs also reveals that any learning appears to have occurred in the first five periods, as play is relatively stable from then on.

*[Insert Table 3 about here]*

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<sup>5</sup> The non-parametric Wilcoxon rank-sum test provides one-way *p*-values of .20 and .08 for the effects of the fallback and verification respectively.

In Table 3 we present data on the owners' level of welfare and their welfare as a percentage of the total surplus generated. It is not surprising that, given the data already presented, owners fared better in the *High Fallback* treatments. However, in contrast with what might be expected, this result was *not* attained by a shift from an efficient but unequal distribution of wealth in the *Low Fallback* treatments to an efficient and equal distribution of wealth in the *High Fallback* treatments. Inspection of the data reveals that owner wealth increases in the *High Fallback* treatments were accompanied by severely reduced social efficiency. This is especially true in the *NV-HF* treatment, where delegation levels were not very high, despite the willingness of (at least some) managers to be obedient. In the absence of verification, owners showed little patience with managers in *NV-HF*. With the advantage of the high fallback payoff, owners accepted few outcomes of 30 before resorting to use of the fallback project, or in an extreme display of mistrust, never delegated at all.

In Tables 4 and 5 we present the empirical distribution of the relative frequency of delegation, given the outcome of the previous period.<sup>6</sup> This gives an indication of the owners' use of history contingent strategies to discipline manager behavior at the aggregate level. The data in Tables 4 and 5 clearly indicate that owners in the *Low Fallback* treatments made little effort to discipline managers.<sup>7</sup> For example, in *V-LF* cases where managers were obedient (disobedient) in the prior period, the owners delegated authority with a relative frequency of 100% (85.3%). In *NV-LF*, there is even

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<sup>6</sup> In order to simplify the analysis, we only consider strategies to be contingent on the immediately preceding round.

<sup>7</sup> We include the standard measure of statistical significance arising from a Chi-squared test of differences in relative frequencies. The significance of these tests is likely overstated since individual owners contribute more than one observation. Therefore, the  $p$ -

less evidence of owners' attempts to discipline manager behavior in *NV-LF*. This is strong evidence of owners' resignation to accept lower earnings in the ***Low Fallback*** treatments, with a slight effect from the verification manipulation. It is therefore of little surprise that the owners received minimal obedience from the managers in these treatments. In contrast, the behavior of owners in the ***High Fallback*** treatment is consistent with an attempt to obtain obedient behavior from managers. For example, in *V-HF* cases where managers were obedient (disobedient) in the prior period, owners delegated authority with a relative frequency of 97.7% (41.9%). The results are similar, although less pronounced, in the *NV-HF* treatment.

*[Insert Tables 4 and 5 about here]*

The results therefore provide support for H3A, the use of history contingent strategies in the *Verification* treatments, and for H3B, the use of history contingent strategies in the *No Verification* treatments. However, the support for these hypotheses is found almost entirely in the ***High Fallback*** treatments. The raw data also seem consistent with H3C, that history contingence is stronger in the *Verification* treatment for both the ***Low*** and ***High Fallback*** treatments. However, an F-test for the difference in history dependence between the *Verification* and *No Verification* treatments is not significant at conventional levels.

Two other observations are in order. The first is the relatively low frequency of delegation subsequent to a period of no delegation in the both the ***High Fallback*** treatments. This is indicative of owners who have either given up trying to steer the manager towards obedience or who have simply never attempted to extract obedience

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values provided would appear more useful as a measure of the relative strength of the association rather than an absolute measure of statistical significance.

from the manager. The second observation is the 41.9% incidence of delegation in *V-HF* subsequent to a period of disobedience by the manager. It is not clear why owners would fail to punish disobedient behavior in this treatment, because disobedience is perfectly revealed and punishment is not costly to the owner. This reluctance to punish undesired behavior in *V-HF* might be, in part, responsible for the unexpectedly low obedience of 50.6%, relative to expectations from the theory.

In our final analysis, we examine the interaction of owner and manager at the individual pair level. There are two issues we hope to address with this analysis. The first issue is to what extent owners and managers found an acceptable cooperative arrangement in which owners delegated in some periods and managers acted obediently in some periods. The second issue is to what extent owners acquiesced to the managers' disobedience.

In order to proceed, we must first determine what constitutes evidence of a cooperative arrangement and owner acquiescence. We arbitrarily classify as a *cooperative arrangement* those cases where an owner delegated in at least 50% of the periods and a manager acted obediently in at least 50% of his opportunities to do so. We classify as *owner acquiescence* those cases where a manager never acted obediently but the owner delegated in at least 90% of the periods. We present the results of our analysis in Table 6.

*[Insert Table 6 about here]*

Inspection of Table 6 reveals the following. The frequency of cooperative arrangements is surprisingly low, given that our theory development indicated the possibility of cooperative arrangements in each of the treatments. The combination of

*Verification* and **High Fallback** produced the highest relative frequency of cooperative arrangements at 40% - none of the relative frequencies in the other treatments were greater than 20%. Only in the **Low Fallback** treatments do we observe owner acquiescence, with a particularly high level found in the *NV-LF* treatment.<sup>8</sup> Finally, in the four cooperative arrangements in the **Low Fallback** treatments reported in Table 6, owners delegated in 115 of 116 administered periods. Therefore, the obedience that was obtained in the **Low Fallback** treatments might be considered inherent, and not due to the efforts of owners.

## 5. Discussion of Results and Conclusion

This experiment was designed to further our understanding of *ex post* verification in a bilateral, repeated interaction setting that shared important attributes of existing institutions. The most salient of these attributes are: (1) managers held private information, (2) owners were unable to contract on all contingencies and (3) owners and managers interacted repeatedly. We expected that the introduction of *ex post* verification, in the form of noiseless signals regarding the state of nature, would allow owners to discipline managers and extract greater returns from delegation. We further expected that the relative bargaining power of owner and manager would affect behavior, but to a lesser degree when verification is present.

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<sup>8</sup> One potential criticism of our analysis is the chosen criteria for determining cooperative arrangements might be biased in favor of the verification treatments, because higher levels of delegation and obedience were possible in these treatments in equilibrium. If for the verification treatments we increased the cutoff for classification as a cooperative arrangement from 50% delegation and obedience to 75%, the number of cooperative arrangements in *V-LF* and *V-HF* would be 1 and 2, respectively. In addition, our interpretations are also sensitive to a small downward revision in the level of obedience alone. If we define a cooperative arrangement as at least 50% delegation and 40% obedience, the number of cooperative arrangements in *NV-LF*, *NV-HF*, *V-LF* and *V-HF* are 2, 3, 4 and 5, respectively.

The results of our experiment lend mixed support to our predictions. When the owners had low bargaining power (low fallback), the results resembled those that would be expected in a repetition of the stage-game equilibrium. That is, repeated interaction did not enable the owner to garner greater benefits from the manager's superior information, contrary to what is suggested by our theory development. Even when owners noiselessly observed the managers' undesirable actions, they were virtually unable (nor made little effort) to induce manager obedience. In the treatments when the owners had greater bargaining power (high fallback payoff for the owner), they received greater obedience from the managers. However, without verification, owners used delegation in a very limited way, which in turn significantly inhibited overall efficiency. Only when the owners had superior bargaining power did the predicted effects of verification partially emerge, allowing the owners to receive greater obedience with less punishment.

The data from the experiment can be interpreted in the following manner. The level of the fallback payoff had a profound effect on the subjects' perceptions of fairness, while the type of feedback had an effect on the subjects' ability to enforce their view of fairness. In the *Low Fallback* treatments, managers expected to receive a larger distribution of wealth and the owners acquiesced. In those instances where cooperation was observed, it was largely through no effort of the owners. In contrast, in the *High Fallback* treatments owners expected a larger share of the surplus. Managers to some extent accepted this, but not without a fight. The obedience in the *High Fallback* treatments was achieved through significantly lower social efficiency. Without verification, owners in the *High Fallback* treatment (*NV-HF*) did not trust the managers

sufficiently to delegate a significant number of periods, although they received levels of cooperation similar to those received by owners in *V-HF*.

In an attempt to place this experiment in context, we will confine our attention to a few of the most relevant prior studies. In Van Huyck, Battalio and Walters (2001), an experiment was conducted on the reputation effects in the Peasant-Dictator game. Their setting is similar to our *V-HF* treatment. Specifically, in their experiment feedback was noiseless and the “peasants” were made better off by not investing, analogous to delegation, rather than investing with a selfish “dictator.” They found the level of investment varied with respect to the potential gains to the investor. Our results are more consistent with their low rate of return treatment, reflecting the fact that there are only modest potential gains from delegating to an obedient manager versus not delegating (60 versus 40). Other factors in our experiment that might have inhibited cooperation relative to theirs were the lack of flexibility in distribution options for the manager, who can only choose between obedience and disobedience, and the stochastic nature of the owner’s payoff under cooperation.

Of note is an experiment conducted by Butler, Schwartz and Young (2001), which pertains to a capital investment setting. The “reputation setting” in that experiment is very similar to the *NV-HF* treatment in this study. In both instances, owners can accept a default payoff that is greater than that received from manager disobedience, but less than that received from manager cooperation. Further, feedback to the owners was noisy. However, our results differ from that study in one important aspect. While the level of manager obedience is almost identical in both experiments, the level of owner investment, analogous to delegation, is much greater in Butler et al. (2001). This might,

again, reflect the effect of potential gains to delegation. In Butler et al. (2001) the default payoff to the owner was zero, so that investment and cooperation from the manager, which yielded a positive payoff in expectation, was greatly preferred to the default payoff.

Also of particular relevance to the current study is an experiment that focused on the effects of verification by Schwartz and Young (2002). They found in their bilateral dilemma game that, contrary to expectations, verification produced only modest gains to efficiency. In fact, without verification subjects cooperated more than expected, perhaps because they feared that their partners would be quick to judge them uncooperative. This might help explain the results in our experiment. In the *Low Fallback* treatments, there appeared to be a tacit acceptance on the part of owners of the selfish behavior of managers. Therefore, in these treatments verification was for the most part irrelevant. In the *High Fallback* treatments, managers cooperated only slightly less often when verification was absent than when it was present, even though our theory indicates that perfect compliance was possible in equilibrium with verification but not without. That is, contrary to predictions derived from our model, verification did not raise the level of obedience significantly.

The implications of this research for the study of managerial control issues are twofold. Our results confirm and strengthen the finding in Schwartz and Young (2002) that in situations where noiseless feedback matters, its effect is to change the perceptions of the receiver of the feedback and not the behavior of the person whose actions are being verified. Stated differently, noisy feedback is often sufficient to discipline manager behavior, but without perfect verification owners are less willing to believe managers are

behaving desirably. Therefore, it may be that managers have as much incentives as ownership in making their own actions observable. The second implication of our results is that extensive monitoring systems may be ineffective if those performing the monitoring are in a disadvantageous bargaining position relative to the persons being monitored. Simply put, if the payoff obtained by the owner's resulting from a manager's undesirable behavior is greater than the owner's next best alternative, it may be difficult to discipline the manager, even if the manager's behavior is fully revealed. The numerous second chances given to star athletes after various rules infractions would seem consistent with this point of view.

After considering the results of this experiment in the context of prior experimentation, we are still left with many open questions. Why is it that the results in Iterated Prisoner's Dilemma games are usually very close to optimal efficiency, yet optimal levels of efficiency are rarely observed in other settings? Under what circumstances is noiseless verification most important? What is the role of relative bargaining power in repeated games? Future research can be aimed at addressing these questions as well as the continued development of a theory that can help explain the results obtained in repeated dilemma games by incorporating intrinsic motivations such as trust, fairness, and altruism.

## **Instructions for *Verification/High Fallback Treatment***

### **Introduction**

Welcome and thank you for participating in this experiment. Your pay will depend on the decisions you make during the experiment. If you follow the instructions and make appropriate decisions, you can earn an appreciable amount of money. At the end of today's session, you will be paid in private and in cash. It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you.

Before the first decision period begins participants will be assigned as Position 1 participants or Position 2 participants. Half of the participants will be assigned as Position 1 participants and half of the participants will be assigned as Position 2 participants. Each Position 1 participant will be randomly matched with a Position 2 participant and will remain matched for the duration of the experiment.

### **Position 1 Participants' Task**

Each decision period Position 1 participants choose between KEEP and PASS. If KEEP is chosen then the Position 1 participant receives 40 POINTS and the Position 2 participant receives 20 POINTS and the next period begins. If PASS is chosen then the Position 2 participant will select from two projects labeled project 1 and project 2.

### **Position 2 Participants' Task**

Position 2 participants only have the opportunity to make a decision if the Position 1 participant chooses PASS. If the Position 1 participant chooses PASS then the Position 2 participant's task is to select a project from two projects labeled project 1 and project 2. Projects yield payoffs for both Position 1 and Position 2 participants. These payoffs will

be described below. The payoffs depend on a random state. The Position 2 participant always knows the random state BEFORE selecting a project. The Position 1 participant LEARNS the true random state only after the Position 2 participant selects a project.

### **The Random States and Project Payoffs**

The payoffs to both participants from a given project depend on a random state. The random state is either state 1 or state 2 with each equally likely. The random state is determined by a computer and is identical to flipping a coin with a 1 on one side and a 2 on one side. The random state is determined each period.

The table below gives the POINTS payoffs from projects 1 and 2 for each possible random state. The first number in each cell is the POINTS payoff to the Position 1 participant and the second number in each cell is the POINTS payoff to the Position 2 participant.

	State 1	State 2
Project 1	30,90	30,90
Project 2	0,0	90,30

The POINTS payoff is not a dollar amount. The POINTS payoff is the probability of receiving \$2.00.

#### **Example One**

If the Position 1 participant chooses PASS and the actual random state is 1 and the Position 2 participant chooses project 1 then the Position 1 participant's POINTS payoff is 30. The Position 2 participant's POINTS payoff is 90.

## **Example Two**

If the Position 1 participant chooses PASS and the actual random state is 2 and the Position 2 participant chooses project 2 then the Position 1 participant's POINTS payoff is 90. The Position 2 participant's POINTS payoff is 30.

## **Sequence of Events and Duration**

Each period the Position 1 participant decides whether to PASS or KEEP.

If he/she chooses KEEP then the Position 1 participant receives 40 POINTS and the Position 2 participant receives 20 POINTS. If he/she chooses PASS then the Position 2 participant selects a project. The Position 1 participant LEARNS the true random state only after the Position 2 participant selects a project. POINTS payoffs are received and then converted to cash. The decision periods will last for 20 periods with certainty. After that there is a 90% chance of continuation each period thereafter.

## **Summary**

Before the first decision period begins participants will be assigned as Position 1 participants or Position 2 participants. Half of the participants will be assigned as Position 1 participants and half of the participants will be assigned as Position 2 participants. Each Position 1 participant will be randomly matched with a Position 2 participant and will remain matched for the duration of the experiment. Position 1 participants can choose to either KEEP or PASS. Position 2 participants choose a project if the Position 1 participants chooses PASS. Payoffs are in points, which are converted to dollars after each decision period.

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## Tables

**Table 1: Relative Frequency of Delegation by Owners**

	<i>Low Fallback</i>	<i>High Fallback</i>
<i>No Verification</i>	.856 <i>n</i> = 319	.279 <i>n</i> = 319
<i>Verification</i>	.899 <i>n</i> = 348	.610 <i>n</i> = 290

<b>ANOVA Results</b>	
Main Effects:	<i>p</i> -value
<i>Verification</i>	.020
<i>Fallback</i>	.002
Interaction	.050
Model	.001

*n* = total number of decisions (**Delegate** and **Not Delegate**) made by owners

**Table 2: Relative Frequency of Obedience by Managers**  
Proportion of cases manager chose Project *B* , given delegation and *s*<sub>2</sub>

	<i>Low Fallback</i>	<i>High Fallback</i>
<i>No Verification</i>	.143 <i>n</i> = 133	.413 <i>n</i> = 46
<i>Verification</i>	.229 <i>n</i> = 157	.506 <i>n</i> = 89

<b>ANOVA Results</b>	
Main Effects:	<i>p</i> -value
<i>Verification</i>	.21
<i>Fallback</i>	.27
Interaction	.48
Model	.35

*n* = number of cases where owner chose **Delegate** and *s*<sub>2</sub> occurred.

**Table 3: Average Per Period Earnings of Owners and Managers**

		<i>Low Fallback</i>	<i>High Fallback</i>
<i>No Verification</i>	Owner	32.0	40.6
	Total welfare	108.1	76.03
	Owner's share of total surplus	29.6%	53.4%
<i>Verification</i>	Owner	35.1	43.2
	Total welfare	111.4	96.6
	Owner's share of total surplus	31.5%	44.7%

**Table 4: History Contingent Frequency of Delegation for *No Verification* Treatments**

History ( <i>t</i> -1 only)	(1) { <b>Not Delegate</b> }	(2) { <b>Delegate</b> , Owner Payoff = 30} → possible disobedience	(3) { <b>Delegate</b> , Owner Payoff = 90} → known obedience
<b>Low Fallback</b>	.630 <i>n</i> = 46	.893 <i>n</i> = 242	.895 <i>n</i> = 19
<b>High Fallback</b>	.095 <i>n</i> = 221	.606 <i>n</i> = 66	1 <i>n</i> = 19
Total: No Verification	.187 <i>n</i> = 267	.831 <i>n</i> = 308	.947 <i>n</i> = 38

Chi-Squared test comparing (2) and (3)	<i>p</i> -value
<i>NV-LF</i>	.976
<i>NV-HF</i>	.009
<i>Combined</i>	.063

**Table 5: History Contingent Frequency of Delegation for *Verification* Treatments**

History ( <i>t</i> -1 only)	(1) { <b>Not Delegate</b> }	(2) { <b>Delegate</b> , Owner observes <i>s</i> <sub>1</sub> ) → No Incentive for Manager to be Disobedient	(3) { <b>Delegate</b> , Owner Observes <i>s</i> <sub>2</sub> , Manager chose Project <i>A</i> } → Disobedience	(4) { <b>Delegate</b> , Owner Observes <i>s</i> <sub>2</sub> , Manager chose Project <i>B</i> } → Obedience
<b>Low Fallback</b>	.824 <i>n</i> = 34	.927 <i>n</i> = 151	.853 <i>n</i> = 116	1.000 <i>n</i> = 34
<b>High Fallback</b>	.330 <i>n</i> = 106	.839 <i>n</i> = 87	.419 <i>n</i> = 43	.977 <i>n</i> = 44
Total: Verification	.450 <i>n</i> = 140	.895 <i>n</i> = 138	.673 <i>n</i> = 159	.987 <i>n</i> = 78

Chi-Squared test comparing (3) and (4)	<i>p</i> -value
<i>V-LF</i>	between .018
<i>V-HF</i>	less than .001
<i>Combined</i>	less than .001

*NV-LF* = *No Verification* and *Low Fallback*

*NV-HF* = *No Verification* and *High Fallback*

*V-LF* = *Verification* and *Low Fallback*

*V-HF* = *Verification* and *High Fallback*

Obedience (Disobedience) = Manager chose Project *B* in *s*<sub>1</sub> and Project *A* (*B*) in *s*<sub>2</sub>.

**Table 6: Cooperative Arrangements (at least 50% Delegation and at least 50% Obedience) Versus Owner Acquiescence (at least 90% Delegation and 0% Obedience)**

Treatment	Total Number of Pairs	Number of Cooperative Pairs	Number of Acquiescent Owners
<i>NV-LF</i>	11	2	6
<i>NV-HF</i>	11	1	0
<i>V-LF</i>	12	2	3
<i>V-HF</i>	10	4	0

*NV-LF = No Verification and Low Fallback*

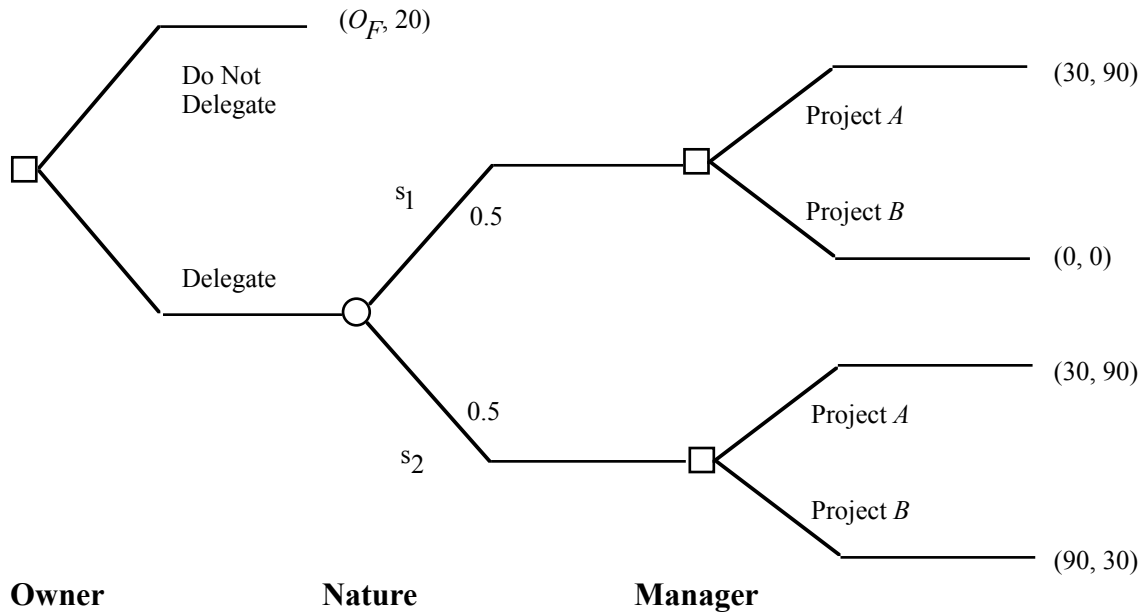
*V-LF = Verification and Low Fallback*

*NV-HF = No Verification and High Fallback*

*V-HF = Verification and High Fallback*

**Figures**

**Figure 1: Extensive Form Representation of the Stage Game (Owner's payoff, Manager's payoff)**



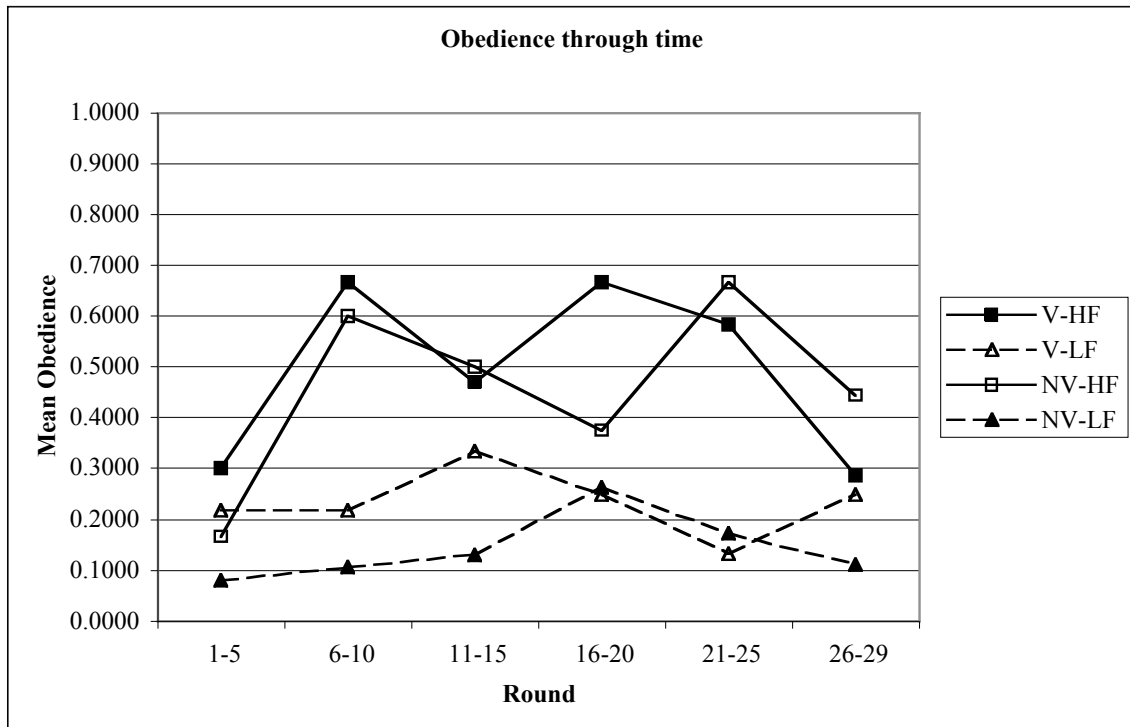
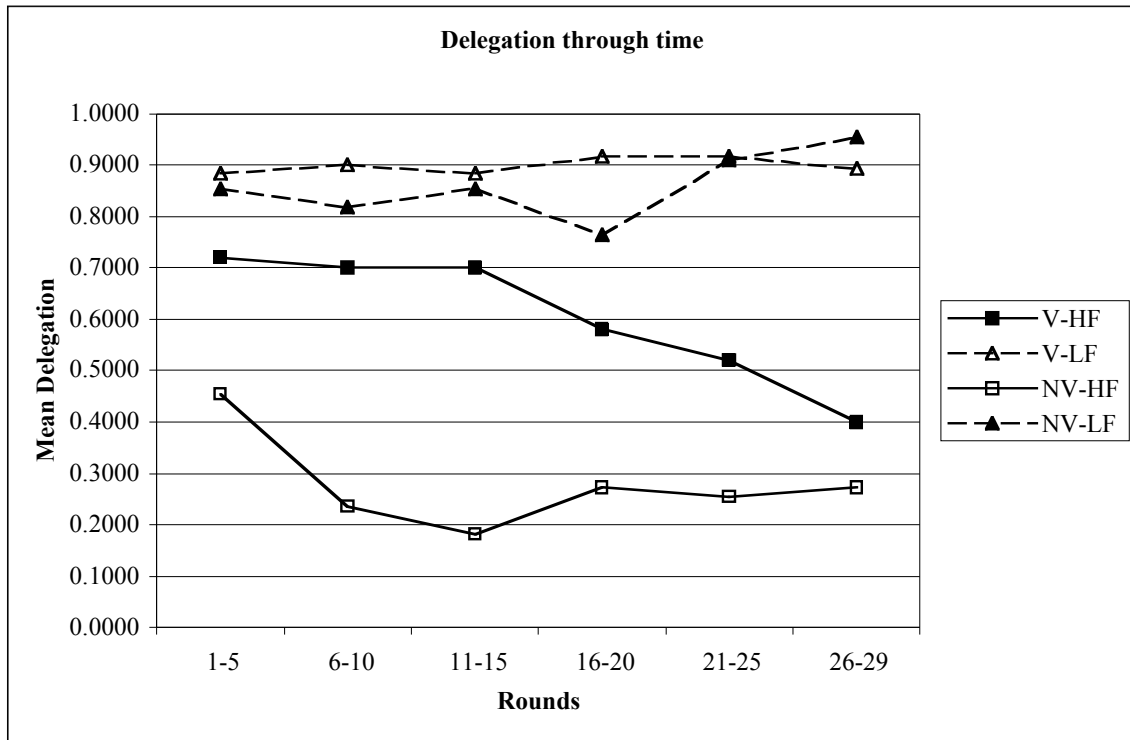
$O_F$  = Owner's Fallback Payoff

**Figure 2: Experimental Design Treatment Notation and Number of Pairs**

	<i>Low Fallback</i>	<i>High Fallback</i>
<i>No Verification</i>	<i>NV-LF</i> $n = 11$	<i>NV-HF</i> $n = 11$
<i>Verification</i>	<i>V-LF</i> $n = 12$	<i>V-HF</i> $n = 10$

$n$  = number of owner-manager pairs  
*NV-LF* = No Verification and Low Fallback  
*V-LF* = Verification and Low Fallback  
*NV-HF* = No Verification and High Fallback  
*V-HF* = Verification and High Fallback

**Figure 3: Delegation and Obedience Behavior over Time**



*NV-LF = No Verification and Low Fallback*  
*V-LF = Verification and Low Fallback*  
*NV-HF = No Verification and High Fallback*  
*V-HF = Verification and High Fallback*