A Laboratory Investigation of Verification and Reputation Formation

In a Repeated Joint Investment Setting

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Abstract

This paper describes an experiment in which two division managers repeatedly invest funds provided by central management in either a division-specific or a common project. It is socially efficient for only the division manager with the less attractive division-specific opportunity to invest in the common project. However, investment in the common project constitutes a strictly dominated strategy, and so the unique Nash equilibrium contradicts the socially efficient rule. At the time investment decisions are made, managers are privately informed of their own division-specific return ratio and receive a forecast from the other manager regarding their division-specific return ratio. Therefore, socially efficient investment is only possible with truthful forecasts. However, managers have private incentives to manipulate their forecast, should they choose to conceal deviations from social efficiency. This creates an important role for *ex post* verification, the main manipulation in the experiment. The matching protocol is also manipulated, using both random and continuous pairing of subjects. The results of the experiment indicate: (1) with randomly pairing settings, verification significantly increases honest forecasting and efficiency and (2) with continuous pairings, verification improves coordination of investment strategies, but has only a muted effect on efficiency. Further, even without verification, continuous pairings increases efficiency. Together, these results are consistent with the notion that verification and repeated pairings act as substitutes in improving forecast quality and efficiency.

KEYWORDS: Experimental Economics, Verification, Reputation, Cooperation

1. INTRODUCTION

This paper describes an experiment that captures an important aspect of accrual accounting – its *verifiability*. To a large extent accounting numbers are based on *historical* transactions that can be verified through auditing. Ijiri (1975) argues that this makes the accounting numbers *hard*, that is, difficult to manipulate. He further notes that the hardness of historical accounting reports improves the quality of *forecasts* (p. 141). In similar fashion, Demski (1994, p. 253) emphasizes the importance of the integrity of the "accounting library."

The verifiability of accounting disclosures may increase the ability of an agent to build a *reputation*. For example, Kreps (1990b, p. 95) notes that, "reputations for behaving in a particular way work more efficiently the more deviations from that behavior are observable." In some situations, it can be in an agent's own best interest to develop a reputation for behaving efficiently and for enforcing efficiency.[1]

In our experiment, subjects play an investment game in which verification and repetition provide subjects with the opportunity to build reputations for playing efficiently. We manipulate the level of *verification* by providing subjects either perfect verification of forecasts, or no verification at all.[2] We also manipulate the *matching protocols*, using either random or continuous pairings. This latter aspect of our study may be of interest to accountants since, although firms may have an indefinite lifespan, they at times interact with specific economic agents (employees, managers, suppliers, etc.) on a one-time basis. Kreps (1990b) notes that a firm's reputation may be valuable even if interactions are of a single-period nature. The visibility of accounting reports may aid in building and maintaining a reputation, even when agents do not interact repeatedly with each other (Dickhaut and McCabe, 1997).

We choose to address these issues within an intra-firm resource allocation setting. In the experiment, two division managers invest in either a common project or a division-specific project. The common project exhibits a decreasing, but certain, return. In contrast, the division-specific project has an uncertain return, whose probability distribution is common-knowledge. Division managers privately learn their own division-specific project's return ratio. Subsequently, the division managers make a simultaneous "forecast" of their division-specific return.[3] After receiving each other's forecast, the managers simultaneously make their investment choices.

In a one-shot version of this investment game, there is a unique Nash equilibrium attained in strict dominant strategies: both managers invest privately, regardless of the rates of return on either division-specific project. Therefore, knowledge of the other division's return ratio has no *private* value. In contrast, the game is structured so that social efficiency is greatest if the manager with the less-attractive division-specific investment chooses the common project. Thus, knowledge of the other division's return ratio has *social* value.

Three characteristics of the game may impede social efficiency:

(1) Social efficiency requires that one player choose a strictly dominated strategy;

(2) Self-interested behavior may lead to bias in the forecasts, inhibiting the coordination necessary to achieve social efficiency;

(3) The socially efficient solution does not Pareto dominate the Nash equilibrium.

Thus, social efficiency requires a great deal of coordination and cooperation.

Let us consider a repeated version of the game. With an infinite horizon, there would remain a Nash equilibrium in which managers always choose the division-specific project. However, there may exist another Nash equilibrium in which socially efficient investment is observed along the equilibrium path. This equilibrium would exist if failure to follow the socially efficient rule could be detected *ex post* and sufficiently punished in the future. This equilibrium requires a great deal of coordination.

We chose a finite length for our game, because of its ease of implementation and data analysis. Of course, a backward induction argument would remove social efficiency as a potential Nash equilibrium. Despite the fact that the game in our experiment is of finite length, we posit that subjects will behave as if they were playing an infinite game because of: (1) its length (40 periods) and (2) the subjects' inexperience with endgame play (they only play one 40-period game). Our conjecture is based on prior research that shows that, in their first experience with a supergame, subjects fail to fully consider the strategic implication of the finite endpoint (Selten and Stoecker 1986).

In the experiment we manipulate two factors, verification and matching protocol, to examine their effects on the efficiency of investment. In all treatments, prior to play, subjects view a five-period investment history of their current partner. In the *continuous pairing* cells (C and D), this history pertains to previous play with the same partner, while in the *random pairing* cells (A and B), the history is continually updated to reflect a change in partners. In the *verification* cells (B and D), the investment history includes the disclosure of actual division-specific rates of return for the previous five periods, while in the *no-verification* cells (A and C), this information is omitted.

Implementation of the socially efficient rule requires that, prior to choosing investments, managers know each other's return ratios. In all experimental treatments herein, subjects *privately* know their own division-specific return ratio at the time of investment. Thus, if they wish to play efficiently, they must rely on the other manager’s *forecasted* return ratio. However, enforcement of cooperation requires that honest forecasting as well as cooperative investment be rewarded in the future, while other behaviors are punished.[4] If managers were unable to verify the cooperative behavior of others, reciprocation and punishment would not be possible. This ability to reciprocate and punish is considered vital for sustaining cooperative behavior in situations characterized by considerable conflict, Axelrod (1984). We therefore manipulate the ability of subjects’ to monitor the decisions of others via verification of forecasts.

The contribution of this paper is twofold. First, it provides evidence as to the ability of an *ex post* accounting report to discipline an *ex ante* forecast. Second, it examines the effect of the accounting report on the coordination and efficiency of manager investment decisions.[5]

The results of the experiment indicate that both verification and continuous pairings increase the truthfulness of forecasts. In addition, verification increases efficiency, especially in the random pairing cells. In the continuous pairing cells, while investment in the common project is actually slightly higher without verification, coordination declines without verification. That is, the individual investing in the common project is often *not* the one with the lower return ratio. When taken together, our findings indicate that verification and continuous pairings appear to be close substitutes in terms of eliciting truthful reports, and partial substitutes with regard to efficiency.

The rest of the paper is organized as follows. Section 2 discusses related literature. Section 3 introduces the model using the experimental parameters. Section 4 presents the experimental design. Section 5 states the hypotheses and Section 6 examines the results. Section 7 concludes the paper.

2. BACKGROUND

This paper focuses on the role of verification in developing reputations for playing a repeated game in a particular way. King and Wallin (1990) study the effects of verification on manager disclosures in a market setting. They find no evidence that verification increases the truthfulness of disclosures. However, because their experiment takes place in a market setting, managers have little incentive to develop a reputation as a truthful forecaster. In equilibrium, managers should not be able to increase the price of their security over its expected value, and so there is no individual benefit obtained from accurate forecasting. In contrast, our setting allows for the coordination of investment strategies using truthful forecasts, thereby increasing not only social efficiency, but individual benefit as well.

Our study has elements in common with resource allocation experiments such as Chow et al. (1994) and Waller and Bishop (1990). However, the models on which these papers are based assume no moral hazard problem, so there is no reason to base managers' payments on division performance. The simple (Groves) solution essentially makes managers' payments a function of aggregate firm performance. However, in a richer setting the presence of moral hazard on actions would make it inefficient to base the contract on aggregate performance. Further, negotiation and enforcement of contract may, in general, be costly. To create a setting in which accurate forecasts are necessary for efficient investing, we make the simplifying assumption that managers' payments are based solely on division profits, and managers can not write enforceable contracts with each other.

Dickhaut et al. (1996) conduct an experiment wherein cooperation requires an inter-temporal link among strategies. Their game consists of two players, each of whom of has available three strategies. Although there are nine possible strategy combinations for the two players, there are only three possible payoff outcomes: (65, 5), (5, 65), and (5, 5). There exists a unique mixed strategy Nash Equilibrium, in which players receive (25, 25) per round in expectation. However, players may do better by cooperating so as to alternate between cells with payoffs of (65, 5) and (5, 65). This approach would yield an average payoff of (35, 35) per round. The experiment consisted of 28 pairs, of which only 3 appeared to cooperate.

In our experiment, social efficiency requires that *in every round* players exchange information about the payoffs (the division-specific return ratio). Thus, in our experiment efficient play requires that forecasts be used to coordinate strategies across periods. A simple alternating approach, which is socially efficient in Dickhaut et al. (1996) would not be efficient. Truthful forecasts are necessary for implementation of social efficiency in our experiment.

The forecasts in our game are a form of *cheap talk* (non-binding, pre-play communication). Crawford and Sobel (1982) analyze the truthfulness of cheap talk in a setting with asymmetric information. In their setting, cheap talk pertains to a player's type (division-specific rates of return may be considered a player type). They conclude the truthfulness of cheap talk varies inversely with the degree of conflict in the game. Dickhaut et al. (1995) conduct an experiment that supports the Crawford and Sobel predictions. Unlike Dickhaut et al. (1995), in our experiment the degree of conflict is too severe to allow for equilibria in the stage game that make non-trivial use of forecasts.

3. MODEL

In our setting each manager receives a loan from central headquarters of $100. This amount must be invested in either a common project or a division-specific project, and must be repaid to headquarters subsequent to the realization of the revenues from the investment. Managers keep only the return generated on the investments.

The revenues generated by the *common* project are common knowledge. It will produce revenues of $200 if selected by a single manager (a return ratio of 2) or $300 if selected by both managers (a return ratio of 1.5). We refer to this type of project as common, because the revenue is split equally between the managers, regardless of the managers' investment choices. Note the decreasing returns to scale -- the second $100 invested has a return ratio of 1.0.

In contrast, the revenues from the *division-specific* (hereafter, D-S) project are uncertain. Each manager's common knowledge beliefs are the return ratios on his or her own D-S project will be 1.1, 1.5 or 1.9, equally likely. We refer to this type of project as division-specific, because revenues accrue exclusively to the investing division.

Prior to investing, managers privately learn their own D-S return ratio. For simplicity, we assume the revenue realizations are statistically independent. Hence, when managers learn their own return ratio they do not change their beliefs about the other managers' D-S projects.

Figure 1a displays the payments to one manager, arbitrarily labeled "Manager A," contingent on nature and the strategies chosen by the other manager, "Manager B." Given the structure of our model, managers' payoffs depend on their own investment choice, their partner's investment choice and their own D-S return ratio. Inspection of Figure 1b further reveals that the game is solvable in dominant strategies. Further, the dominant strategy is for a manager to *always* choose D-S. A manager optimally chooses the D-S investment, independent of his or her own return ratio and independent of the other manager's strategy. Under the unique Nash equilibrium, each manager's expected payoff is $(^1/_3)\ 10 + (^1/_3)\ 50 + (^1/_3)\ 90 = 50$.

[Insert Figures 1a and 1b about here.]

Social welfare would increase if the managers moved from the Nash equilibrium. For example, consider the situation where Manager A and B learn their own return ratios are 1.1 and 1.5, respectively. If they play Nash, they receive $10 and $50, respectively -- a total of $60. If Manager A were to instead choose Common, while B remained at D-S, they would receive $0 and $150, respectively. The gain in social welfare would be

$150 – $60 = $90. This gain to having one and only one manager choose Common is present because the common project always earns a larger return on the first $100 invested than a D-S project, but a lower return ratio on the second $100. This relationship makes it efficient for the manager with the lowest return ratio to select the common project and the other to take the D-S project. Therefore, the socially efficient decision rule depends only on the realized *maximum* return ratio across the two divisions.[6] Figure 2 calculates individual payoffs and social welfare as a function of the state of nature. Since each division is *ex ante* identical and each return ratio is equally likely, the expected social welfare assuming efficient play is as follows: $(^1/_9)$ 110 + $(^1/_3)$ 150 + $(^5/_9)$ 190 = 167 $^7/_9$ (an average of 83 $^8/_9$ per person).

[Insert Figure 2 about here.]

Referring back to Figure 1b, it is easy to see the difficulty associated with the social welfare maximizing solution. First, to maximize social welfare, someone must know which manager has the larger return ratio. This is not known at the start of the game, and each manager's return ratio becomes private knowledge. Second, even if this information were somehow communicated honestly, the managers would continue to have incentives to invest in their own D-S project. The temptation to deviate from the socially efficient rule is strong. One manager must make a significant sacrifice so the other can benefit. But consider the expanded strategy space that would result if the managers repeated the game. Now one manager may become willing to make this sacrifice if he or she believes the other will reciprocate in the future. This motivates the experiment described in the next section.

Another aspect of the social welfare maximizing solution is the potential for ties in the return ratio. The resolution of ties is not an issue in achieving social efficiency but, nevertheless, ties must be resolved. For this reason, we define what we henceforth term the *cooperative solution*. The cooperative solution has three parts. First, the managers communicate their return ratios honestly. Second, as in the socially efficient solution, whichever manager has the higher division return ratio invests in the D–S project, and the other invests in the common project. Third, ties are dealt with as follows: (1) when both return ratios are 1.1, managers invest in the common project; (2) when both managers have return ratios of 1.5 or 1.9, both invest in the D-S project. The cooperative solution as defined above resolves the indeterminacy of the socially efficient solution, and has the intuitively appealing characteristic that when managers face the same set of circumstances, they are treated symmetrically.[7] This solution sacrifices only a small amount, as the expected payoff per manager, per round is 80.[8] The cooperative solution is implementable in each treatment of the experiment, and was provided to each of the subjects.

4. EXPERIMENTAL DESIGN

Students were recruited from undergraduate business courses at The Ohio State University and Binghamton University.[9] Students were told the approximate length of the experiment. All experimental tasks were computerized. Students were assigned to one of four cells (see Figure 1) based on the night chosen to participate in the experiment. Cell assignments were randomly made to experimental sessions. Six experimental sessions were held: two each for Cells A and B and one each for Cells C and D. Sessions of ten and twelve subjects were conducted for Cell B while sessions of six and eight subjects were conducted for Cell A. Each of the sessions for Cells C and D had 20 participants. The experimental design is shown in Figure 3.

[Insert Figure 3 about here.]

When the subjects arrived, they were given a set of written instructions and allowed to spend as much time as needed to read them. After the subjects read the instructions, they were reviewed orally by the experimenter. In addition, subjects were shown examples and subsequently given a quiz on how to calculate their points. Sample instructions for Cell A appear in the Appendix.

In the initial part of the experiment, the subjects played five training rounds. During the training rounds, point balances were not counted towards cash payments. Subsequently, the subjects played the game for forty periods, during which subjects earned cash.

The parameters used in the experiment are identical to those found in Section 3 (presented to subjects as "points," rather than dollars). At the beginning of the round, subjects received a 100-point loan from corporate headquarters, which would be returned to corporate headquarters at the end of each round. In addition, subjects were informed of the return ratio they would obtain from their own D-S project. This ratio was randomly determined by the computer, according to the probability distribution described in the Model section above.

The division managers kept only the return generated on these points. Points could not be allocated between the D-S and common projects -- all the points had to be invested in either the D-S or the common project.[10] Total revenue from the common projects was split equally between the two subjects, regardless of who invested in it. However, all of the revenue from the D-S project retained by the investing subject.

4.1 Cells C and D: Continuous Pairing

Subjects were matched once at the beginning of the training rounds and then re-matched at the beginning of the cash earning rounds. The pairing remained unchanged for the remainder of the experiment.

After learning their D-S return ratio, subjects were required to send a "forecast" to the other manager regarding their return ratio. Subjects were free to choose any forecast from among the following: 1.1, 1.5 or 1.9. The forecasts were sent simultaneously. Therefore, subjects could not condition their forecast on that of the other subject.

After receipt of the forecasts, each subject decided whether to invest in the common project or the D-S project, without knowledge of the decision of the other manager. After both subjects entered their investment decisions into the computer, the subjects' point balances were updated.[11] The computer screen contained information regarding the prior five rounds of play (i.e., subject's own D-S return ratio, forecasts sent and received and project choices). In addition, subjects in Cell D received verification of their partner's *actual* D-S return ratio at the end of each round; this information was included in the five round history.

Following forty rounds of play, the subjects were remunerated with one dollar for every 175 points earned, in five-cent increments. All subjects were paid in cash, privately, at the end of the experiment. The subjects' expected earnings if they played Nash or the cooperative solution would be (50)(40)/175 = $11.43 and (80)(40)/175 = $18.29, respectively

4.2. Cells A and B: Random Pairing

Subjects in Cells A and B were randomly re-matched with a partner at the conclusion of each round, for both the training and cash-earning segments of the experiment. Play in the random pairing cells (A and B) closely follows that in the continuous pairing cells (C and D), with the differences highlighted below.

In each round in Cells A and B, prior to sending the forecast, each subject received a five-period history concerning his or her current partner. In order to maintain consistency between treatments, the history contained the same type of information as that reported in the continuous pairing cells (C and D).[12] The history consisted of (up to) five observations on: (1) the current partner's forecasts, (2) the forecasts the current partner received, and (3) the project choices of the current partner. In addition, subjects in Cell B received (up to) five observations of their current partner’s *actual* D-S return ratio. Figure 4 describes the sequence of events.

[Insert Figure 4 about here.]

4.3 Other Aspects of the Game

In each of the four cells, subjects were provided with the cooperative solution described in the Model section. This was done for the following reason: if subjects wished to cooperate, but were either unaware or disagreed on how to cooperate, then cooperation is not likely to occur. Kreps (1990b, p. 105) recognizes this issue when he notes, "When we say that compliance must be observable, we naturally suppose that we understand what ‘compliance’ means." Smith (1996) extends the discussion to experimental settings when he argues that all aspects of a game must be common knowledge to the subjects for the results to be meaningful. This cues the subjects, but this same cue is provided in all four cells.[13]

5. HYPOTHESES

A plausible consequence of verification is subjects can develop a reputation for sending truthful forecasts, as part of a more comprehensive reputation for socially efficient play.[14] Presumably, subjects are motivated by the expectation of reciprocal treatment from their partners. This is seen as desirable, given the disparity in expected remuneration between the Nash equilibrium and the cooperative solution. Hypothesis 1, therefore, relates to the most direct implication of the introduction of verification.

Hypothesis 1: Subjects in the verification cells (Cells B and D) will disclose their return ratio truthfully more often than subjects in the no-verification cells (Cells A and C).

Socially efficient play requires accurate forecasting, so that investment choices can be made conditional on the return ratios on the D-S projects. If subjects believed the forecasts received were informative, and they were attempting to follow the cooperative solution, we would expect the following.

Hypothesis 2: Subjects' incidence of investment in the common project is positively correlated with their partners' forecast of their D-S return ratio.

Noisy forecasts impede implementation of social efficiency, while biased forecasts may be the result of an attempt to exploit one's partner. We therefore postulate that the reliance placed on the received forecasts is strongly conditional on the perceived veracity of these reports. This conjecture, together with Hypothesis 1, implies the following.

Hypothesis 2A: Subjects in the verification cells (Cells B and D) are more likely to condition their investment choices on the forecast than those that do not receive verification (Cells A and C).

Socially efficient play has two distinct components, truthful forecasting and contingent investment strategies. More precisely, under the cooperative solution the likelihood of common project investment is non-decreasing in the forecast received and non-increasing in one's own D-S return ratio. If we make the reasonable assumption that subjects' willingness to invest in the common project varies inversely with their own obtained D-S return ratio, Hypotheses 1 and 2A together imply Hypothesis 3.

Hypothesis 3: Subjects will attain a higher level of efficiency in the verification cells (Cells B and D) than in the no-verification cells (Cells A and C).

In our game, in Cell B there is verification but random matching. Hence, defectors may be detected and punished, but not by the person originally affected by the defection. Instead, players may enforce the cooperative solution by retaliating against players that have shown a tendency not to cooperate in the past. Experiments conducted by Schwartz et al. (2000) and Kahneman et al. (1986) provide evidence that subjects will punish transgressors, even if they themselves were not the injured party. However, even with verification, enforcement may be difficult in Cell B. Subjects may perceive that, without direct experience with their partner, the punishment mechanism is not sufficiently strong as to enforce efficient play. Therefore, they themselves have insufficient incentives to choose the cooperative solution.

In addition, in Cell B only five periods of financial history are shown on the screen. The player may have had only one or two opportunities to defect in those five days, and players may not be certain how often any other player is defecting. While we do not necessarily feel this effect is strong, its presence could mitigate the effectiveness of the verification provided Cell B.

If it is not common knowledge how the group intends to punish defections, cooperation may quickly break down or not even be attempted. VanHuyck et al. (1997) has an interesting discussion of this type of coordination problem. The coordination problems would be exacerbated in Cell B because subjects are randomly matched.

Keeping in mind these countervailing forces, Hypothesis 4 is stated below in its alternative form.

Hypothesis 4: In the presence of verification, subjects will attain a higher level of efficiency in the continuous matching protocol (Cell D) than in the random matching protocol (Cell B).

Our expectations regarding the effect of matching protocols in the non-verification cells is even less definitive than for the non-verification cells. Subjects in Cell C may attempt to discern whether defections are likely by using the known probability distribution of the D-S return ratios. Further, a computer tournament reported in Bendor et al. (1991), suggests that lenient strategies may work well in a noisy environment. Therefore, one might expect efficiency to be greater in Cell C than in Cell A. However, in another paper, Bendor (1993) reports that the results obtained in the computer tournament were highly contingent on the ecology present. Equilibria wherein subjects choose "Cooperate," require significant, if not extraordinary, computing resources, and are generally not unique. Further, in our game no such equilibria exist, due its known endpoint. Keeping in mind these countervailing forces, Hypothesis 5 is stated below in its alternative form.

Hypothesis 5: Without verification, subjects will invest more efficiently in the continuous matching protocol (Cell C) than in the random matching protocol (Cell A).

6. RESULTS

6.1 Summary Statistics

Summary statistics for the experiment are found in Table 1. Average points earned per subject, per round were approximately 62, 68, 70 and 71 in Cells A, B, C and D, respectively. The narrow range is not all that surprising, because expected points earned from Nash equilibrium play are 50, as compared to 80 from the cooperative solution. Because similar experiments typically show strong learning effects, in most of the ensuing analysis we present separate results for each half of game, that is, for Rounds 1-20 and 21-40.

[Insert Table 1 about here.]

6.2 Specific Hypotheses

To explore Hypothesis 1, we perform a two-way analysis of variance with subject truthfulness as the dependent variable and verification and matching protocol as the independent variables. The results of the ANOVA's support Hypothesis 1. Verification significantly increases the truthfulness of subject forecasts; p-values are .07 and .01 for Halves 1 and 2, respectively. Table 2 also reveals a statistically significant cross-effect; p-values are .09 and .02 for Halves 1 and 2, respectively. Since there is significant interaction effect, the remainder of our analysis is generally performed with consideration of the matching protocol.

[Insert Table 2 about here.]

Related to the significant cross-effect, the levels of truthfulness in Cells B, C and D are not very different from each other. The presence of both continuous pairing and verification does not appear to increase the level truthfulness over that resulting if only one of these elements is present. However, the difference in truthfulness between Cell A versus Cells B, C, and D for all periods has a two-way significance of 0.06. Thus, verification seems to be important for inducing truthful forecasts, but only when individuals do not deal repeatedly with each other. Verification and continuous pairing seem to function as substitutes for each other.

While truthful forecasting might be expected in the verification cells, B and D, the high level of truthful forecasting in Cell C, where there is no verification, is surprising. Our findings may be similar to that of Bendor et al. (1991), who report that in an iterated noisy repeated Prisoner's Dilemma lenient strategies work well. Our Cell C shares important attributes with their setting. Continued conformance to the cooperative solution, which implies truthful reporting, may be considered lenient.

When retaliation does occur in Cell C, it may take the form of withheld contribution, as opposed to untruthful forecasting. Failure to invest in the common project, when it would have been called for by the socially efficient decision rule, sends a clear message that the sender's intention is to retaliate, rather than to exploit. This holds because when there is no verification, overstated forecasts can not be immediately detected, and may be ultimately interpreted as an attempt at exploitation, rather than retaliation.

Hypothesis 2 concerns whether subjects systematically condition their investment decisions on the forecasts received from their partners. Table 3 discloses the average amount invested in the common project, conditioned on the forecast sent by their partner. Most striking is the case where subjects' partners forecast a return ratio of 1.9. Here, the level of common investment is much higher in Cells B, C and D than in Cell A. This is consistent with the subjects acting in a socially efficient way.

[Insert Table 3 about here.]

To provide additional information regarding the conditioning of investment on forecasts, we fit the following logit model

$$Pr(INVEST_{it} = 1) = G[\ \beta_0 + \beta_1 * F_{it}\ ],$$

where:

$INVEST_{it}$	=	1 if player i invested in common project in round t, 0 otherwise;
F_{it}	=	Return ratio on D-S project forecasted by partner of player i, in round t. Coded as 0, 1 or 2, corresponding to forecasts of 1.1, 1.5, or 1.9, respectively;
G[x]	=	$[\ 1+Exp(-x)\ ]^{-1}$ (the logistic cumulative density function).

The logit results are separately summarized in Table 4 for Halves 1 and 2.

Hypothesis 2 would be supported by a positive estimate of β_1. In the first half of the experiment, the estimate of β_1 was positive and statistically significant at the .05 level in all but the random pairing/no-verification cell (Cell A). In the second half of the experiment, the estimate of β_1 was positive only in the continuous pairing/verification cell (Cell D). This difference in the estimates of β_1 between the two halves of the experiment suggests that subjects may have lost confidence in the forecasts received from their partner. Except for Cell A, this decline in conditioning occurred even though the level of truthfulness remained high. One might speculate that the cause of this decline in the continuous pairing/no-verification cell may have been the subjects' inability to recognize random sequences of events. Wagenaar (1972) reports that subjects expect random sequences to show self-correcting tendencies that would not actually be present in a random sequence. Subjects may have felt that the other division manager was "too lucky" and therefore stopped responding to sequences of three or four high return ratio forecasts in a row. However, this observation is also consistent with a trigger strategy (Kreps, 1990a). In a trigger strategy, one subject punishes another when the probability that his or her partner defected becomes sufficiently high.

[Insert Table 4 about here.]

In summary, the results of the logistic regression are consistent with Hypothesis 2, especially in the earlier periods. Further, the sustained use of the forecast in investment choices in Cell D is consistent with Hypothesis 2A: subjects in the verification cells were more likely to condition their investment on the forecasts sent by their partner.

Hypothesis 3 relates to attained levels of social efficiency. Two measures of efficiency are computed. The first, Standard Efficiency, is the ratio of points earned by the pair to the total possible points that could be earned, as defined in equation (1):

$$\text{Standard Efficiency} = \text{PE} / \text{MPE}, \qquad (1)$$

where PE denotes total points earned by the pair and MPE denotes total possible points that could have been earned by the pair, given the realized return ratios. The advantage of this measure, as opposed to non-normalized points, is that it controls for the random effect of nature. However, given the nature of the game, even inefficient forms of play would lead to a Standard Efficiency measure much greater than zero. To better separate the observations, we also compute Minmax Efficiency, defined in equation (2):

$$\text{Minmax Efficiency} = (\text{PE} - \text{NPE}) / (\text{CPE} - \text{NPE}), \qquad (2)$$

where NPE denotes total points earned by the pair if the Nash equilibrium had been played and CPE denotes total points that could have been earned by the pair if they had played the cooperative solution. In contrast to Standard Efficiency, Minmax Efficiency focuses on the gain above that obtained by Nash equilibrium play. We exclude observations wherein both subjects have a return ratios of either 1.5 or 1.9, since the points earned from Nash equilibrium and cooperative play would in those cases be equal.

Some remarks on the unit of observation used to test Hypothesis 3 are in order. In the continuous pairing cells (C and D), an observation is defined as a pair's efficiency, averaged over forty rounds of play. Therefore, each pair of subjects is included as a single observation. In the random pairing cells (A and B), an observation is defined as the efficiency for a subject, averaged across all of his or her partners for forty rounds. While we have included standard statistical measures of significance, we recognize the need for their cautious interpretation in Cells A and B.

Inspecting Table 5, in both halves of the experiment, verification appears to have a moderate effect on Standard Efficiency. This effect increases slightly in the latter portion of the experiments. Clearly, the effect on efficiency is more pronounced in the random pairing cells than in the continuous pairing cells.

[Insert Tables 5 and 6 about here.]

To better understand the Table 5 results, we present the data in Table 6. Increased earnings (above the Nash equilibrium level), are a function of both subject willingness to use the common project, and the relative frequency of its use in conformance with the cooperative solution (hereafter, effectiveness). Table 6 shows that in the random pairing cells, subjects in Cell B (ex-post verification), use the common project both more frequently and more effectively than in Cell A (no ex-post verification), hence the relatively large difference (as compared to the continuous pairing cells) in efficiency. In the continuous pairing cells, the more frequent use of the common project in Cell C (no-verification) is offset by the more effective use of the common project in Cell D (verification). Using an approximate randomization technique (10,000 trials), the difference between Cells C and D in the frequency of "correct" usage of the common project has a two-way p-value of 0.12. The difference in the random pairing cells is not significant at conventional levels.

[Insert Table 7 about here.]

Table 7 reports the results of an ANOVA using Minmax Efficiency as the dependent variable for each half of the experiment. The presence of verification appears to significantly increase Minmax Efficiency, especially in the random pairing cells. Comparing Tables 5 and 7, Minmax better captures gains in efficiency due to more effective use of the common project.

We investigate Hypothesis 4 by performing a t-test on the difference in mean Minmax Efficiency between Cells B and D. This test produces insignificant results for both halves of the experiment. This result is consistent with Schwartz et al. (2000), who find that disclosures of a player's past choices may substitute for direct experience.

We investigate Hypothesis 5 by performing a t-test on the difference in mean Minmax Efficiency between Cells A and C. The test does produce significant results for both halves of the experiment; the p-values corresponding to Halves 1 and 2 are .001 and .050, respectively. As conjectured, the subjects' knowledge of the underlying probability distribution of the return ratio may have permitted the emergence of efficiency in Cell C. In Cell A, despite the fact that subjects see a reporting history of their current partner, the absence of both verification and continuous pairings appears to substantially inhibit efficiency.

6.3 Post Hoc Analysis

In order to gain a better understanding of subject play through time, Tables 8 and 9 provide a round-by-round summary of play for the continuous pairing cells. (Such an analysis is not possible for the random pairing cells.) Each cell consists of ten pairs. In these tables, a summary notation is used to characterize the play of a pair in a round.

[Insert Tables 8 and 9 about here.]

Table 8 depicts the play in the no-verification cell, Cell C. It appears that the play of only a few pairs may be easily characterized. Almost without exception, Pair 3 attempted to act efficiently throughout the game. The play of Pair 1 can best be described as non-cooperative; however, they did occasionally contribute to the common investment. Pair 9 appears to have made limited attempts at efficient investment until Round 20, after which such attempts were completely abandoned. The play of the remaining pairs appears to follow no discernible pattern.

The frequency of common project investment wherein the players did not conform to the cooperative strategy is quite striking. In many instances, the italicized *N's* in Table 8 represent occasions in which the subject with the lower return ratio invested in the common project, while one or both of the subjects sent false messages. Despite the presence of untruthful forecasts, investment decisions were *as if* they were conditioned on the true return ratios as prescribed by the cooperative solution. Therefore, the abundance of common project investment greatly increased the average earnings for subjects in Cell C.

It is surprising that, given the willingness of subjects to invest in the common project in Cell C, they quite frequently disregarded the cooperative solution. There are several possible explanations not easily distinguishable with the data as collected. Subjects may not have understood the game or were just bored, subjects may not have understood the mechanics of the cooperative solution, or subjects may not have believed the cooperative solution given to them was optimal (Andreoni 1995).

The above explanations do not seem to adequately describe the phenomenon. The actual play of the game took about thirty minutes, and subjects did not appear frustrated with the length or requirements of the game. In addition, before the start of play any subjects who wished to leave were allowed to do so with the receipt of their extra credit points. Subjects were quizzed on the nature of the game prior to the beginning of play. Play was not allowed until all subjects completed the quiz correctly. There were no complaints received subsequent to play related to the difficulty of the game. If subjects were unable to understand the mechanics of the cooperative solution, this phenomenon would appear in all cells; yet, this phenomenon appears to be unique to the no-verification cells. As discussed below, about half the subjects in the verification cell had no difficulties in understanding the solution.

The most likely explanation for the pervasive investment in the common project in the no-verification cell is that subjects are making a conscious attempt to increase efficiency. While their approach is often not consistent with the cooperative solution, this inconsistency may not be all that surprising. Subjects had greater freedom to experiment without jeopardizing their reputation in the no-verification cell as opposed to the verification cell. For example, subjects appear to be quite forgiving and, at times, even appeasing toward their partner. There are many instances wherein subjects underreported their return ratio and then made the investment in the common project. Subjects may have felt their partner was getting frustrated at their continued "luck," and opted to sacrifice remuneration for a round rather than risk losing future cooperation. At the other extreme, there were frequent attempts at exploitation. Pair 7 is an excellent example, with players 1 and 2 receiving the highest and lowest remuneration in the experiment, respectively.

The pattern of play in the verification cell is more easily characterized. An examination of Table 9 reveals that Pairs 2, 3, 4, and 5 attempted to play efficiently throughout the game. Pairs 6, 7, and 9 engaged in only sporadic cooperative play, while Pairs 1 and 10 show an inconsistency of play similar to that widely found in the no-verification cell. Pair 8 displayed a quite unusual pattern of behavior in that they began the game by clearly not cooperating, about half way through the experiment they began a period of sustained cooperation.

While the presence of verification may enhance the effectiveness of common investment, it may decrease its frequency. In the case of errors or experimentation by the subjects, verification will fully reveal a deviation from social efficiency. In such circumstances, efficiency may be difficult to restart. Note that in the verification cell, all pairs began the game with at least one of the players investing in the common project. By Round 3, Pairs 1, 6, 7, 8 and 10 experienced a deviation. With the noticeable exception of

Pair 9, who experienced a defection in Round 4, these are the pairs that invested less frequently in the common project. Only Pair 8 appears to have been able to return to efficiency after a deviation. At least 50% of the pairs appeared to understand the mechanics of the cooperative solution (2, 3, 4, 5 and 8), and so a lack of understanding appears unlikely to fully explain the play in the no-verification cell.

In both the no-verification and the verification cell, there appears to be little unraveling of efficient play at the end of the game. In the no-verification cell, 4 of 10 pairs either played efficiently or at least invested in the common project in the last period. In the verification cell, this frequency was 7 of 10. The most likely explanation is that subjects learn end of game play through experience, and in our experiment they experienced only one end game (Selten and Stoecker 1986).

7. CONCLUSION

An experiment was formulated in which there were social gains to truthful forecasting, because it facilitates coordination to efficient investment decisions. However, efficient investment decisions are in conflict with private incentives. Verification of subjects' forecasts were introduced into the experiment, allowing subjects to build a reputation for sending truthful forecasts and investing cooperatively. Verification of forecasts is viewed as an important role played by accounting data (Ijiri 1975). We examine the role of verification using both continuous and random matching protocols.

The results of our experiment indicate subjects in the verification and/or continuous pairing cells used the common project more efficiently than did subjects in the no-verification/random pairing cell. Also, subjects in the verification or continuous pairing cells also appear to send more truthful forecasts than did subjects in the no-verification/random pairing cell. Subjects may trust the other manager's forecasts due to the presence of verification. Verification reveals untruthful forecasts and hence allows individuals to discipline those that violate from efficient investment behavior. Interestingly, the presence of either verification or continuous pairing increased the frequency of truthful forecasts, but the effect was not additive. In essence, verification and continuous pairing seemed to act as substitutes with regard to elicitation of truthful forecasts and social efficiency. However, without verification, forecasts were less heavily relied upon in making investment decisions.

There are several possible extensions of this work. In our experiment, there are significant benefits relative to the Nash equilibrium strategy from adopting strategies that do not require forecasting, but do make use of verification. For example, the subjects might all invest in the common if and only if their own return ratio is 1.1.[15] Verification would allow enforcement of this or other types of forecast-independent cooperation. Thus, one might consider a similar experiment that eliminates the forecast. This would enable the experimenter to distinguish whether verification is helpful because it encourages honest forecasting, or whether it simply allows enforcement of a type of cooperation that requires no forecasts. A second extension might be to explore the effects of allowing subjects to negotiate face-to-face.

FIGURES

Figure 1a

Payoff Consequences to Manager A

Manager B's Strategy

Manager A Strategy	**Common**	**Division-Specific**
Payoffs to A if A has Return Ratio = 1.1		
Common Investment	.5(300) - 100 = 50	.5(200) - 100 = 0
D-S Investment	.5(200) + 110 - 100 = 110	110 - 100 = 10
Payoffs to A if A has Return Ratio = 1.5		
Common Investment	.5(300) - 100 = 50	.5(200) - 100 = 0
D-S Investment	.5(200) + 150 - 100 = 150	150 - 100 = 50
Payoffs to A if A has Return Ratio = 1.9		
Common Investment	.5(300) - 100 = 50	.5(200) - 100 = 0
D-S Investment	.5(200) + 190 - 100= 190	190 - 100 = 90

Figure 1b

Upper left entry in each cell is Manager A's payoff.
Lower right entry in each cell is Manager B's payoff.

	Mgr B Return Ratio	**1.1**		**1.5**		**1.9**	
	Mgr B Strategy	**Common**	**D-S**	**Common**	**D-S**	**Common**	**D-S**
Mgr A Return Ratio	**Mgr A Strategy**						
1.1	**Common**	50 50	0 110	50 50	0 150	50 50	0 190
	D-S	110 0	10 10	110 0	10 50	110 0	10 90
1.5	**Common**	50 50	0 110	50 50	0 150	50 50	0 190
	D-S	150 0	50 10	150 0	50 50	150 0	50 90
1.9	**Common**	50 50	0 110	50 50	0 150	50 50	0 190
	D-S	190 0	90 10	190 0	90 50	190 0	90 90

Figure 2

Socially Efficient Outcomes

Maximum Return Ratio	Profit to Manager with Minimum Rate	Profit to Manager with Maximum Rate	Social Welfare
1.1	.5(200) – 100 = 0	.5(200) + 110 – 100 = 110	110
1.5	.5(200) – 100 = 0	.5(200) + 150 – 100 = 150	150
1.9	.5(200) – 100 = 0	.5(200) + 190 – 100 = 190	190

Figure 3

Experimental Design

CELL A No verification Random pairing n = 14	**CELL B** Verification Random pairing n = 22
CELL C No verification Continuous pairing n = 20	**CELL D** Verification Continuous pairing n = 20

Figure 4

Sequence Of Events

Sequence of Events	**Cell A** Random No verif.	**Cell B** Random Verification	**Cell C** Continuous No verif.	**Cell D** Continuous Verification
Subjects randomly paired	X	X		
Subjects observe current partner's: (1) forecasts sent, (2) forecasts received and (3) investment choice for prior 5 rounds	X	X	X	X
Subjects observe current partner's actual return ratio for prior 5 rounds		X		X
Subjects privately observe their return ratio	X	X	X	X
Subjects make forecast	X	X	X	X
Subjects observe forecasts of other manager	X	X	X	X
Subjects choose project	X	X	X	X
Subjects observe other manager's project choice	X	X	X	X
Point balances updated	X	X	X	X
Subjects observe other manager's return ratio		X		X

TABLES

Table 1

DESCRIPTIVE STATISTICS

Rounds 1 - 40

Treatment	Number of subjects	Average points per subject, per round	Standard Deviation of average points	Minimum average points	Maximum average points
Cell A Random pairing No verification	14	61.86	10.24	40.50	76.75
Cell B Random pairing Verification	22	67.83	13.83	40.25	87.25
Cell C Continuous pairing No verification	20	69.68	18.54	38.25	106.25
Cell D Continuous pairing Verification	20	70.91	12.37	49.50	95.50

Table 2

AVERAGE LEVEL OF TRUTHFULNESS OF FORECASTS, BY SUBJECT

Means: Rounds 1 to 20

	No-verification	Verification
Random Pairing	**.600**	**.802**
Continuous Pairing	**.815**	**.820**

ANOVA: Rounds 1 to 20

Source	DF	F Value	P Value
Verification	1	3.34	.07
Pairings	1	4.21	.04
Verification*Pairings	1	3.03	.09
Error	72		
Total	75		

Means: Rounds 21 to 40

	No-verification	Verification
Random Pairing	**.532**	**.823**
Continuous Pairing	**.795**	**.825**

ANOVA: Rounds 21 to 40

Source	DF	F Value	P Value
Verification	1	8.72	.00
Pairings	1	5.96	.02
Verification*Pairings	1	5.76	.02
Error	72		
Total	75		

Note: the unit of observation is a subject's relative frequency of truthful forecasts.

Table 3

AVERAGE INVESTMENT IN COMMON PROJECT CONDITIONED ON PARTNER'S FORECAST

Rounds 1 to 20

		Average points invested in common project			
Partner Forecast	**Expected points if played cooperative solution**	**Cell A** Random pairing, No verification	**Cell B** Random pairing, Verification	**Cell C** Continuous pairing, No verification	**Cell D** Continuous pairing, Verification
1.1	33.33	28.07	33.09	31.25	24.65
1.5	33.33	29.17	28.68	30.33	33.33
1.9	66.67	24.47	47.62	48.80	42.18

Rounds 21 to 40

		Average points invested in common project			
Partner Forecast	**Expected points if played cooperative solution**	**Cell A** Random pairing, No verification	**Cell B** Random pairing, Verification	**Cell C** Continuous pairing, No verification	**Cell D** Continuous pairing, Verification
1.1	33.33	36.21	33.54	39.53	21.70
1.5	33.33	25.68	32.81	29.75	22.31
1.9	66.67	27.78	41.56	32.00	41.33

Table 4

EFFECT OF FORECAST ON PROBABILITY OF INVESTMENT IN COMMON PROJECT

Model: $\Pr(\mathrm{INVEST}_{it} = 1) = G[\beta_0 + \beta_1 * F_{it}]$

INVEST_{it} = 1 if player i invested in common project in round t, 0 otherwise;
F_{it} = Return ratio on D-S project forecasted by partner of player i, in round t. Coded as 0, 1 or 2, corresponding to forecasts of 1.1, 1.5, or 1.9, respectively;
G[x] = $[1+\mathrm{Exp}(-x)]^{-1}$ (the logistic cumulative density function).

Logistic Regression Estimate of Slope Coefficients

Round	**Statistics**	**Cell A** Random pairing, No verification	**Cell B** Random pairing, Verification	**Cell C** Continuous pairing, No verification	**Cell D** Continuous pairing, Verification
1 to 20	β_1	-.0877	.3319*	.4062*	.3999*
	Wald χ^2	.3120	7.4718	9.8924	9.8094
	p-value	.5765	.0063	.0017	.0017
21 to 40	β_1	-0.2103	.1733	-.1624	.5007*
	Wald χ^2	1.8669	2.1455	1.6336	13.1569
	p-value	.1718	.1430	.2012	.0003

* Significant at the .05 level.

Table 5
AVERAGE EFFICIENCY PER PAIR, PER PERIOD
(EFFICIENCY = PE / MPE)

Means: Rounds 1 to 20

	No-verification	Verification
Random Pairing	**.747**	**.821**
Continuous Pairing	**.857**	**.850**

ANOVA: Rounds 1 to 20

Source	DF	F Value	P Value
Verification	1	3.55	.07
Pairings	1	15.22	.00
Verification*Pairings	1	5.22	.03
Error	52		
Total	55		

Means: Rounds 21 to 40

	No-verification	Verification
Random Pairing	**.739**	**.812**
Continuous Pairing	**.803**	**.825**

ANOVA: Rounds 21 to 40

Source	DF	F Value	P Value
Verification	1	4.22	.05
Pairings	1	2.73	.11
Verification*Pairings	1	1.20	.28
Error	52		
Total	55		

Table 6

PERCENT OF INVESTMENT IN THE COMMON PROJECT CONFORMING TO COOPERATIVE SOLUTION

Panel A:

Percentage of the investments in the common project that were made in accordance with the cooperative solution provided to the subjects assuming subjects knew the *actual* return ratio of their partner.

Treatment	**Cell A** Random pairing, No verification	**Cell B** Random pairing, Verification	**Cell C** Continuous pairing, No verification	**Cell D** Continuous pairing, Verification
# of investments in the common project	162	323	288	251
% of cooperative investment	.611	.723	.781	.896

Panel B:

Percentage of the investments in the common project that were made in accordance with the cooperative solution provided to the subjects given the *forecasted* return ratio of their partner

Treatment	**Cell A** Random pairing, No verification	**Cell B** Random pairing, Verification	**Cell C** Continuous pairing, No verification	**Cell D** Continuous pairing, Verification
# of investments in the common project	162	323	288	251
% of cooperative investment	.673	.808	.809	.916

Table 7
AVERAGE MINMAX EFFICIENCY PER PAIR, PER PERIOD
(EFFICIENCY = (PE - NPE) / (CPE - NPE))

Means Rounds 1 to 20

	No-verification	Verification
Random Pairing	**.422**	**.586**
Continuous Pairing	**.681**	**.656**

ANOVA Rounds 1 to 20

Source	DF	F Value	P Value
Verification	1	1.70	.20
Pairings	1	9.50	.00
Verification*Pairings	1	3.11	.08
Error	52		
Total	55		

Means Rounds 21 to 40

	No-verification	Verification
Random Pairing	**.374**	**.600**
Continuous Pairing	**.535**	**.652**

ANOVA Rounds 21 to 40

Source	DF	F Value	P Value
Verification	1	7.68	.01
Pairings	1	2.97	.09
Verification* Pairings	1	0.77	.38
Error	52		
Total	55		

Table 8
PLAY BY PERIOD CONTINUOUS PAIRING/ NO-VERIFICATION (Cell C)

Pair Round	1	2	3	4	5	6	7	8	9	10
1	N	*1*	*1*	B	*2*	*1*	*1*	N	*N*	*N*
2	N	*2*	*N*	N	N	*2*	*N*	*N*	N	*N*
3	*N*	*1*	*1*	*N*	*2*	*N*	*2*	*N*	*N*	*1*
4	N	*B*	*2*	*1*	*N*	*2*	*2*	*N*	*2*	*2*
5	*N*	*N*	*1*	*N*	N	*N*	B	B	N	*N*
6	N	*N*	*2*	*2*	*N*	N	*N*	*1*	*N*	N
7	N	B	*2*	*N*	*2*	N	*N*	*N*	B	B
8	*N*	*2*	*2*	*2*	N	*N*	*N*	*1*	*1*	B
9	*N*	*1*	B	*1*	N	*2*	*N*	B	B	N
10	N	*B*	*1*	N	*N*	*N*	*2*	*N*	*N*	N
11	N	B	B	*2*	N	*N*	*N*	*N*	N	*2*
12	N	*1*	B	*N*	*N*	*B*	B	*N*	*N*	B
13	N	*N*	*2*	*2*	*1*	*2*	*N*	*1*	N	*2*
14	N	B	*2*	N	B	N	*2*	*N*	*N*	*N*
15	N	*1*	*2*	B	N	B	*1*	B	*N*	N
16	N	*N*	*B*	N	*1*	*N*	*N*	*N*	B	*1*
17	*N*	*1*	*1*	*1*	*1*	*1*	N	N	*N*	N
18	*N*	*N*	*B*	N	B	*2*	*B*	N	*1*	*1*
19	N	B	B	*2*	*N*	*N*	N	*1*	*N*	*1*
20	N	*N*	*1*	*2*	*N*	N	*N*	*N*	*1*	*N*
21	N	*N*	*1*	B	N	N	*B*	*1*	*N*	*2*
22	N	*N*	*1*	N	*N*	*N*	*1*	*N*	N	*2*
23	N	N	*2*	*N*	*N*	*N*	*B*	*N*	N	N
24	N	*2*	*2*	*N*	N	*B*	N	*1*	N	N
25	*N*	*N*	B	*N*	N	N	*N*	N	B	*N*
26	N	*N*	*2*	*N*	*N*	*2*	*2*	*1*	B	*N*
27	N	*1*	*B*	N	N	B	*N*	*1*	N	*B*
28	N	N	*1*	*N*	N	*B*	B	N	N	*1*
29	N	*1*	*B*	N	*N*	B	*N*	N	N	*2*
30	*N*	*N*	*2*	*N*	N	*1*	*N*	*1*	N	*B*
31	N	N	*B*	*N*	B	N	*N*	*2*	N	B
32	N	N	B	*N*	N	B	*2*	B	N	N
33	N	*N*	*1*	*1*	*2*	*B*	*2*	*N*	N	N
34	N	*1*	*1*	N	*N*	*N*	N	*2*	N	*N*
35	N	*N*	*1*	*N*	N	*1*	*N*	N	N	N
36	N	*N*	*1*	*N*	*B*	N	*N*	N	N	N
37	N	*1*	B	N	N	*B*	*N*	*2*	N	B
38	N	*N*	B	*N*	*2*	B	*N*	*N*	N	*2*
39	*N*	*1*	*B*	N	*2*	*2*	B	N	N	*2*
40	N	N	*1*	*N*	N	*N*	N	*N*	N	N

1 or *2* = The pair used the cooperative strategy given to them. The number indicates which of the two players in the pair made the investment in the common project

B or *B* = The pair used the cooperative strategy, both players had the identical return ratio. (Italics indicate an investment in the common project was made.)

N or *N* = The pair did not use the cooperative strategy. Either one or both played lied <u>or</u> the proper investment in the common project was not made. (Italics indicate an investment in the common project was made.)

Table 9
PLAY BY PERIOD CONTINUOUS PAIRING/ VERIFICATION (Cell D)

Pair Round	1	2	3	4	5	6	7	8	9	10
1	B	*1*	B	B	*1*	*2*	*N*	B	B	*N*
2	*1*	*2*	*1*	B	B	*2*	N	*1*	*1*	*N*
3	N	*2*	*1*	*1*	B	N	N	N	*1*	*N*
4	B	*2*	B	*2*	*1*	N	N	N	*N*	N
5	*2*	*2*	*1*	*1*	*2*	*N*	N	N	*2*	*N*
6	*1*	B	*B*	*1*	*B*	N	B	*N*	B	*N*
7	*N*	*2*	*2*	*1*	B	N	*N*	B	*N*	N
8	N	B	B	*2*	*2*	*1*	B	N	*N*	*N*
9	*B*	*2*	*1*	*2*	*1*	N	*N*	N	N	N
10	N	*N*	*1*	*2*	*2*	N	N	N	N	*N*
11	N	*2*	*1*	*2*	*1*	*N*	N	N	N	*N*
12	*B*	*2*	*1*	*2*	*2*	N	*1*	*1*	N	*N*
13	*N*	*2*	*B*	*1*	*2*	*N*	B	B	N	N
14	B	*1*	B	*1*	*1*	N	N	N	N	*N*
15	*N*	B	*2*	*1*	*1*	N	N	N	N	*N*
16	*2*	*2*	B	*1*	*2*	N	B	N	N	N
17	*2*	*1*	B	*B*	*N*	B	N	*2*	N	*N*
18	*B*	*B*	B	*2*	B	*N*	N	*N*	*N*	N
19	B	*2*	*2*	B	N	*N*	N	*1*	*2*	*N*
20	*1*	*2*	B	*1*	*1*	N	N	*2*	*N*	N
21	*N*	*1*	*2*	B	*2*	N	*1*	*1*	*N*	N
22	N	*1*	*1*	B	B	N	N	*1*	N	*N*
23	N	*1*	B	B	*2*	*N*	N	*1*	N	N
24	N	*2*	*1*	*1*	*1*	N	N	*1*	N	B
25	N	B	B	B	*1*	*B*	N	*2*	N	*N*
26	N	*2*	*2*	*2*	*2*	B	N	*2*	N	N
27	B	*2*	B	B	*B*	*1*	N	*1*	N	*N*
28	B	*1*	*1*	*1*	*1*	*N*	N	*2*	N	*N*
29	*1*	B	*B*	*2*	*2*	N	B	*2*	N	N
30	*2*	*1*	B	*2*	*1*	*N*	*N*	*2*	*N*	*N*
31	B	*1*	B	*2*	*1*	*N*	*2*	B	N	N
32	*N*	*1*	*2*	*2*	*B*	*N*	N	N	N	N
33	N	*2*	B	*2*	*1*	*N*	N	N	N	N
34	N	*1*	*1*	*1*	*2*	N	N	*1*	N	*N*
35	N	*2*	*2*	*2*	*N*	N	N	N	*N*	N
36	B	*1*	*1*	*1*	*2*	*N*	N	N	*N*	N
37	N	*1*	*1*	*B*	B	N	N	*2*	N	*N*
38	*N*	B	*1*	*1*	*1*	*N*	*N*	N	B	*N*
39	N	B	*B*	B	*N*	N	B	B	N	N
40	N	B	B	*2*	B	*N*	*N*	*1*	N	N

1 or *2* = The pair used the cooperative strategy given to them. The number indicates which of the two players in the pair made the investment in the common project

B or *B* = The pair used the cooperative strategy, both players had the identical return ratio. (Italics indicates an investment in the common project was made.)

N or *N* = The pair did not use the cooperative strategy. Either one or both played lied <u>or</u> the proper investment in the common project was not made. (Italics indicate an investment in the common project was made.)

APPENDIX

INSTRUCTIONS (Cell A)

YOU MAY NOT TALK TO ANY OTHER SUBJECT ONCE THE EXPERIMENT HAS STARTED!

Thank you for participating in this experiment. Throughout this experiment, you will accumulate points. At the end of the experiment, you will receive one cent for every 1.75 points you accumulate (or equivalently, $1.00 for every 175 points). The decisions you make, coupled with the result of random draws, will determine your point total (and thus the cash payment you receive). Your cash payment will be made at the end of the experiment.

Your Task. You will be assigned a role equivalent to that of a division manager in a firm with two divisions. Each period or round (called **day**), you will be paired with another participant playing the other division manager. Each of you face the same set of choices. Each period, you have 100 points you must invest in either a **common project** of the two divisions, or an alternative investment opportunity (**AIO**) that is available only to your division. At the end of the period you must return the 100 points, thereby keeping only the return on your investment.

Common Project. One of three events can occur with the common project: neither manager invests in it, both invest, or only one invests. The first 100 points invested in the common project is doubled (i.e., multiplied by 2.0). The second hundred points are not doubled (i.e., multiplied by 1.0). The resulting balance in the common project is split evenly between both managers, even if only one manager invests in the common project. The Table below summarizes what may happen:

		Points Received From Common Project		
Your Investment in the Common Project	Other Manager's Investment in the Common Project	Total	Your Share	Other Manager's Share
0	0	0	0	0
100	0	200	100	100
0	100	200	100	100
100	100	300	150	150

Alternative Investment Opportunity (AIO). Each period you will receive information about your AIO. Your AIO will be either 1.1, 1.5 or 1.9, each equally likely. The other manager will also have an AIO of 1.1, 1.5 or 1.9, each equally likely. These draws are done independently, therefore, you may have different AIOs or the same AIO. There is no information you can learn about the other manager's AIO by knowing your own.

If you invest 100 points in the common project, you automatically have invested 0 points in your AIO. Similarly, if you invest 0 points in the common project, you automatically have invested 100 points in your AIO. The amount returned from an AIO investment is the value of the AIO (1.1, 1.5 or 1.9) times the amount invested (0 or 100 points). Suppose your **AIO is 1.1**. You may do one of two things: invest 100 points in the common project (and thus 0 in the AIO), or 0 points in the common project (and thus 100 in the AIO). The possible returns are outlined below where your net return is calculated as follows:

Points Received From Common Project + (AIO Investment)(AIO) - 100 Points.

Your net return is to the left of the parentheses in **bold** print.

Your Investment in Common Project	Therefore, Your AIO Investment is	Other Manager's Investment in Common Project	Points Received From Common Project	Net Return	
0	100	0	0	**10**	(0 + 110 - 100)
0	100	100	100	**110**	(100 + 110 - 100)
100	0	0	100	**0**	(100 + 0 - 100)
100	0	100	150	**50**	(150 + 0 - 100)

Suppose your **AIO is 1.5**. The possible returns are outlined below:

Your Investment in Common Project	Therefore, Your AIO Investment is	Other Manager's Investment in Common Project	Points Received From Common Project	Net Return	
0	100	0	0	**50**	(0 + 150 - 100)
0	100	100	100	**150**	(100 + 150 - 100)
100	0	0	100	**0**	(100 + 0 - 100)
100	0	100	150	**50**	(150 + 0 - 100)

Suppose your **AIO is 1.9**. The possible returns are outlined below:

Your Investment in Common Project	Therefore, Your AIO Investment is	Other Manager's Investment in Common Project	Points Received From Common Project	Net Return	
0	100	0	0	**90**	(0 + 190 - 100)
0	100	100	100	**190**	(100 + 190 - 100)
100	0	0	100	**0**	(100 + 0 - 100)
100	0	100	150	**50**	(150 + 0 - 100)

Your Net Points in a Period. At the end of the period, you must return the 100 points given to you initially. The returns listed above are net of this 100 points. As you can see, your net return for any one day can range from 0 to 190 points. You begin the experiment with 0 points. Each day your net return is added to this amount. At the end of the experiment you will receive one cent for every 1.75 points, rounded up to the nearest nickel. The experiment will consist of 5 training rounds and 40 cash earning rounds. After the training rounds your point total will be reset to 0. Your cash payment depends on the points earned in the cash earning rounds (i.e., you do not earn cash for the training rounds).

Investment Messages. At the beginning of each period, you will learn your AIO for that period (a new one will be randomly selected each day). You do not observe the other manager's AIO. After observing your AIO, you will have an opportunity to send a **message** to the other manager. This message may be 1.1, 1.5 or 1.9. All messages will be entered without the decimal (i.e., 11, 15 or 19). Regardless of your actual AIO, you may send any message you wish. The possible usefulness of these messages is discussed in **Cooperating with the Other Manager**.

Investment. After sending your message you will observe the other manager's message. At this time, you will choose the amount you wish to invest in the common project (either 0 or 100 points). The other manager will choose his/her investment as well. After you both have chosen, you will observe your net for the period: Note that you will not know the other manager's message before you've sent yours. The other manager will not know your message before he/she has sent his/her message. Similarly, you will not know the other manager's investment before you've selected your investment. The other manager will not know your investment before he/she has selected his/her investment.

The Other Division Manager. A number of individuals are participating in this experiment at the same time. You will be randomly paired with another division manager each period, in both the training rounds and the cash-earning rounds. The computer will make random assignments to the best of its ability.

Review of Sequence. The following events occur each period:

1. You observe your AIO (1.1, 1.5 or 1.9).
2. You select a message (11, 15 or 19).
3. You observe the other manager's message.
4. You select your investment.
5. You observe the other manager's investment and your net.

The last five days of your experience will be maintained on the upper portion of the screen.

Cooperating with the Other Manager. The following is intended to serve as a guide on how to use the common project and the pre-investment messages. **However, you are**

free to use any strategy you wish. Some participants have found that cooperating with the other division manager leads to higher returns for both participants. If you would like to cooperate, the most efficient method is as follows. First, always **truthfully** report the value of your AIO to the other division manager. Next, compare your actual AIO to the AIO reported by the other division manager. If you have the lower AIO then invest in the common project (enter 100 under 'Your Investment'). If you have the higher AIO then invest in the AIO (enter 0 under 'Your Investment'). If you both have the same AIO then invest in the common project if you both have an AIO of 1.1, and invest in the AIO if you have an AIO of 1.5 or 1.9. Sometimes you will earn 0 points in a round using this strategy, however, in the long run, you and the other division manager will earn higher returns when you use the common investment **efficiently**. Remember, the common project returns a higher yield then any AIO provided only one division manager invests in it. The cooperative strategy is summarized below:

Comparison of AIOs	Action
Your AIO < Other Manager's *Reported* AIO	Invest in Common Project
Your AIO > Other Manager's *Reported* AIO	Invest in AIO
Your AIO = Other Manager's *Reported* AIO	Invest in Common Project if AIO = 1.1 Otherwise, Invest in AIO

Payment. When the experiment is finished, please remain seated until someone has recorded your earnings. You will be called individually to receive your payment. The amount you receive will not be communicated by the experimenter to any other participant. Again, thank you for participating in this experiment.
REMEMBER, ONCE YOU ARE SEATED, YOU MAY NOT COMMUNICATE WITH ANY OTHER PARTICIPANT.

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ENDNOTES

[1] We use the word reputation in the colloquial sense (Mayhew 1998, p. 3; Van Huyck et al. 1997, p. 2). We use the word efficient to mean maximization of social welfare.
[2] The choice of perfect or no verification is extreme, but simple.
[3] The important aspect of the game is that a manager has superior information about his or her won division. While it may be unrealistic to assume the forecaster knows this for certain, it greatly simplifies the game.
[4] Although a stylized setting is employed in this experiment, there are documented cases where accounting data has been used to verify behavior. A good example is the electric turbine industry in the 1960's, which was dominated by two competitors, General Electric and Westinghouse. The firms were locked in fierce price wars due to the complexity of the pricing formula of turbines. Public accounting firms were brought in to verify that neither firm was offering secret price cuts to their customers. After the prices were audited and made public, collusive pricing was possible. See Porter (1983) for an extended discussion.
[5] These two effects do not completely separate. For example, if managers do not take into account *ex ante* forecasts when making investment decisions, there is little need for disciplining such forecasts.
[6] In the event of a tie, social welfare is independent of which division selects the common project.
[7] This solution also has the advantage of not requiring subjects to take turns across periods. Taking turns would not sacrifice efficiency, but coordination to it would be difficult.
[8] There is also a "symmetric" randomized solution that improves further on the proposed solution. It requires that when both subjects have return ratios of 1.1. 1.5 and 1.9, each subject invests in the common project with a probability of 0.9, 0.5 and 0.1, respectively. Each subject's expected payoff per round is 81.5, which slightly exceeds that from the cooperative solution 80, but is less than that from the socially efficient solution, 83 $^{8}/_{9}$. This solution would be difficult to enforce.
[9] Cells B, C and D were conducted at The Ohio State University, Cell A was conducted at Binghamton University. A change in computing facilities at Ohio State necessitated the change in venue. Students at Binghamton University were recruited from an identical introductory accounting class, offered the same amount of extra credit for participating (1% of their grade), and were seated in a room of very similar layout to that used at Ohio State. Data on subject demographics was not collected.
[10] Not permitting the subjects to invest in both projects simplifies the decision structure for the subjects and simplifies the game. Further, each manager funding, say, 50% of the common project each period would Pareto dominate the single-period Nash solution, eliminating one of the important features of the game we wish to study.
[11] Given the parameter values used in the experiment, subjects could not lose points in any round. It has been theorized that individuals may have different utility functions when they perceive that they are in a deficit situation, e.g., *prospect theory* (Kahneman

and Tversky 1979). The game was structured to avoid this possibility.

[12] One difference between the random and continuous pairing cell is that subjects in the continuous cell may recall and try to exploit information about their partner that relates to behavior that occurred more than five periods prior. Limitations on the size of the computer screen size made it difficult to include information on the screen that goes back more than five periods. We do not believe this difference would have a significant affect on the subjects' behavior.

[13] Of course, this design leaves unexplored the effect of any possible interactions between the treatments and whether the solution was provided.

[14] Subjects may be inherently reluctant to report dishonestly (Baiman and Lewis 1989).

[15] This strategy would provide each subject with expected points of 74 $^4/_9$.