Intrinsic and Extrinsic Motivations in a Capital Budgeting Setting

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Abstract

Prior research has indicated that intrinsic motivations play an important rule in allocation of resources. Generally, these experimental settings are not of particular relevance to the study of management control, because they are not characterized by significant information asymmetries. This paper reports on a capital budgeting experiment wherein manager-subjects privately observe the cost of production after it occurs and have opportunity and incentives to misreport the cost to owner-subjects. With the introduction of information asymmetry into the resource allocation problem, the robust finding of reciprocation of trust and also the crowding out often found in prior experimentation do not obtain. Intrinsic motivations play a minor role in the baseline setting, and the introduction of both explicit and implicit incentives appear to enhance intrinsic motivations rather than crowd them out.

1. Introduction

This paper describes a laboratory experiment that utilizes a simple yet interesting setting to investigate the prominence of non-wealth maximizing behaviors in a capital budgeting context. The motivation for this study derives from two streams of literature. The first stream is devoted to the analysis of capital budgeting environments wherein there exists a local manager who possesses superior, private information regarding a project's costs (Antle and Eppen 1985; Antle and Fellingham 1997). In theory, the information asymmetry between owner and manager makes it *ex ante* optimal for the owner to commit to *ex post* sub-optimal levels of investment in order to curtail the manager's over-consumption of resources. The second stream is devoted to the study of intrinsically motivated behavior (Fehr and Falk 2001). Intrinsically motivated behavior derives from internal preferences such as a person's taste for fairness or honesty. This is in contrast to extrinsically motivated behavior, which derives from tangible (usually pecuniary) incentives. The importance of combining these two streams of literature is highlighted by the observations that: (1) theoretical analyses of capital budgeting settings have been performed almost exclusively without the consideration of intrinsic motivations, and (2) experimental studies of intrinsic motivations have not incorporated asymmetric information in a significant way. Through the use of a laboratory experiment, we investigate the nature of intrinsically motivated behaviors and the potential for an interaction between intrinsic and extrinsic motivations in an interesting capital budgeting setting.

We incorporate asymmetric information in an experimental design similar to the model of Fellingham and Young (1990). Initially, an owner (central management) decides whether to fund production. A manager is present, whose only role is to observe and report the cost of production. The manager has insufficient resources to fund the project on his or her own. The manager learns the cost subsequent to production (if ordered by the owner). The owner elicits a cost report from the manager and can commit to a transfer of funds conditional on the manager's report. These features when combined with the manager's

limited resources create a setting of interest wherein the owner optimally foregoes positive net present value production. Hence, social welfare is not maximized.

The loss of social welfare attributable to information asymmetry rests critically on the assumption of rational own-wealth-maximization by the owner and manager. Whether this assumption closely approximates individual behavior remains an empirical question. Social scientists have increasingly observed that individual behavior is affected by non-pecuniary intrinsic motivations. If intrinsic motivations are a non-negligible determinant of individuals' behavior in capital budgeting settings, traditional economic analyses might not produce valid predictions.

Of particular importance to this setting are two intrinsic motivations previously identified in the literature: (1) a tendency for experimental subjects to reciprocate trust (Berg et al., 1995; Fehr et al., 1997; Bolle, 1998)[1] and (2) a preference for honesty (Evans et al. 2001). The presence of either of these intrinsic motivations would militate against the validity of predictions derived from rational own-wealth-maximization assumptions.

There have been many experiments designed to investigate whether individuals reciprocate trust. These experiments share the following attributes. First, a trustor assigns resources without recourse to a trustee. Second, subsequent to the assignment of resources the trustee may, of his or her own volition, choose whether to return resources to the trustor. Third, the act of trusting, as in the *investment game* (Berg et al., 1995), or the act of reciprocating trust, as in the *gift-exchange game* (Fehr et al., 1997), increases total social wealth. The capital budgeting setting we investigate includes all these attributes.

The capital budgeting setting does differ though in two very important respects from the other experiments we are aware of that concern the reciprocation of trust. In previous research, the first-mover (trustor/owner) can infer perfectly the behavior of the second-mover (trustee/manager) by observing his or her own payoffs. In our setting, information asymmetry, which is at the center of the capital budgeting problem, prevents this from

[1] See Fehr and Gächter (2000b) for a review of the literature on reciprocity.

occurring. Although this lack of observability has no effect on the manager's *ability* to act on his or her intrinsic motivations, it might affect the manager's *willingness* to do so. The noisiness of this setting may act to distance managers from the commission of acts that would be undesirable from the viewpoint of the owners, thus decreasing the psychic cost of violating a social norm. Similar behavior was detected in Hoffman et al. (1996) and Schwartz and Wallin (2002). The second difference is that that the trustee in our setting must ultimately respond with a report, and hence must face the choice of whether or not to be dishonest. The inclusion of the reporting task in our experiment is motivated by the notion that reporting activities are fundamental to accounting.

The evidence to date on the willingness of individuals to submit dishonest reports is inconclusive. While Baiman and Lewis (1989), Evans et al. (2001) and Schwartz and Wallin (2002) all found that individuals appeared reluctant to submit dishonest reports, their findings differed in the following respect. Baiman and Lewis (1989) and Schwartz and Wallin (2002) both found that individuals were willing to overcome their reluctance to report dishonestly for a relatively small pecuniary gain. In contrast, Evans et al. (2001) found that individuals gave up significant earnings because they did not lie to the extent predicted by rational own-wealth-maximization.

The effects of reciprocation of trust and preferences for honesty, while beneficial to the owner, would be of second-order importance if they merely enhanced extrinsic motivations (Fehr and Gächter, 2000a). However, a growing body of literature suggests that interactions between intrinsic and extrinsic motivations occur to such an extent that caution must be taken when designing control mechanisms (Fehr and Falk, 2001). Social psychologists as far back as Lepper et al. (1973) and Deci (1975) have argued explicit monetary incentives "crowd out" an individual's intrinsic motivations to perform inherently rewarding tasks, such as volunteering. Crowding out refers to the phenomenon wherein imposing extrinsic rewards decreases individuals' existing intrinsic motivation to undertake an activity. For example, introducing a payment for blood donations had the effect of crowding

out volunteers' already-existing intrinsic motivations to perform their civic duty by freely donating blood (Upton, 1973).

Frey and Jegen (2000) provide a recent survey of the literature in which they contend that the evidence supporting motivational crowding is convincing. An experiment by Fehr and Gächter (2000a) extends the investigation to the potential crowding out of reciprocation of trust. The results of their experiment indicate that when workers were offered generous fixed wages, they contributed effort levels well above the level that was incentive compatible. However, when a direct enforcement mechanism was introduced in the form of a fine, workers only provided the incentive compatible level of effort. That is, when subjected to an explicit incentive mechanism, workers worked hard to avoid being fined, but for no other reason. In general, an interaction between explicit incentives and intrinsic motivations could cause enforcement mechanisms to be less effective in improving owner welfare than predicted.

Finally, it has often been theorized that the need for providing explicit incentives might be obviated if relationships continue into the future with a sufficiently high likelihood (Axelrod, 1984). The intuition behind this conjecture is that the implicit incentives present in repeated play can substitute for the explicit incentives derived from complete contracting. However, when considering the previous arguments that explicit incentives crowding out intrinsic motivations, the following question arises: In what way, if any, do the *implicit* incentives derived from repeated play interact with intrinsic motivations such as the reciprocation of trust? Experiments conducted by Gächter and Falk (2001) and Falk et al. (1999) indicate that repeated play can reinforce reciprocation of trust, in contrast to explicit incentives, which have been found to decrease reciprocation of trust.

Influenced by these recent findings, we investigate the capital budgeting problem under three different regimes. The first regime, our baseline treatment, is a simple single-period "spot market" transaction between an owner and a manager. The owner does not have at her disposal any enforcement mechanism to induce honest disclosure of the manager's

private information. In this case, only the manager's intrinsic motivations might preclude the standard prediction of a substantial loss in social welfare. The second regime also takes place in a single period – but introduces a strictly positive probability that the manager will be subjected to a fine if he or she submits a dishonest disclosure. In this case, the explicit incentives provided by the potential fine to the manager are predicted to mitigate the impact of the manager's informational advantage. However, if some degree of crowding out were to occur, this explicit incentive might not be as beneficial as the standard analysis would indicate. In the third regime, the owner and manager interact in a setting most resembling a labor relationship. In particular, the interaction continues to each succeeding period with a probability strictly greater than zero. Due to the potential for repeated interaction, the owner has the ability to provide the manager with implicit incentives, and therefore can potentially mitigate the loss due to the manager's informational advantage. In sum, the primary contributions of the paper are that, within a model of asymmetric information, we investigate whether intrinsic motivations are important and, if so, whether they interact with extrinsic motivations provided by either explicit or implicit incentives.

The results of our experiment indicate that both explicit and implicit incentives appear to enhance intrinsic motivations rather than crowd them out. In our baseline treatment, there is very little evidence that preferences for honesty and reciprocation of trust played a role at all. In the other two treatments, manager behavior was significantly more beneficial to the owner. Further, there is evidence of stronger intrinsically motivated behavior when explicit incentives are present. The importance of these results is that they were found in a setting containing the essential features of the capital budgeting problem: asymmetrically informed owner and local manager and the necessity of the owner relying on a manager's report. Our results suggest that, when these features are present, the very robust results of reciprocation of trust found in prior experimentation might not obtain. Further, the use of explicit or implicit incentives may be particularly efficacious in mitigating the efficiency loss due to information asymmetry.

The rest of this paper is organized as follows. Section two presents the experimental design and includes a brief description of the model with the parameters used in the experiment. Section three presents our behavioral predictions. Section four describes our results and section five summarizes the paper.

2. Experimental Design

The experiment was conducted at two large public universities. Participants were undergraduate business student volunteers. When participants arrived at the administration site, they were given written instructions. After allowing for a sufficient length of time for the participants to read the instructions, they were read aloud by one of the experimenters. Questions were answered and participants were shown examples on how to calculate earnings. Finally, participants completed a quiz on the calculation of earnings.

Our experimental setting is derived from the model first presented in Fellingham and Young (1990). This simple model consists of an owner and a manager. The owner is the residual claimant to a production process and decides whether or not to produce. The manager's sole purpose is to acquire cost information after production. The model is representative of the larger class of capital budgeting models whose primary feature is the information asymmetry between owners and their agents. Antle and Fellingham (1997) emphasize that these capital budgeting models are of particular relevance to accountants, because the frictions they study are of first-order importance with respect to the information gathering activities within the firm.

There are three treatments: (1) one-shot interactions with no opportunity to use explicit incentives (*ONESHOT*), (2) one-shot interactions with the opportunity to deduct points for detected untruthful reporting (*DEDUCT)* and (3) repeated interactions with no possibility of deducting points (*REPEAT*). The instructions that were read to the subjects for each treatment are included in the Appendix. Each treatment began with a training phase that lasted for eight *rounds*. Participants began as either owners or managers. In order to avoid role playing, the subjects were told they would be either a Type O or Type M player,

respectively. After four rounds, all participants switched roles. The cash-earning phase of the experiment consisted of eight *games*. Each game lasted for six rounds with certainty. After the sixth round, the game had a one sixth chance of ending at each subsequent round. Participants remained in the same role within a game, but switched roles after every game. The entire game structure was made known to the participants at the outset. Participants were remunerated with one U.S. Dollar for every 30 experimental points earned in the experiment. To equalize the expected payment across treatments, participants in the *ONESHOT* treatment also received a flat $10 payment.

2.1 *ONESHOT Treatment*

The *ONESHOT* treatment was used as a baseline for the other two treatments in the experiment. One round of the *ONESHOT* treatment proceeded as follows. The structure of the game was completely described to the owner and manager. Before the production decision was made, neither the owner nor the manager knew the cost. The owner moved first by deciding whether or not to produce. If the owner chose not to produce, the owner and the manager both received 0 and the round was over. If the owner chose to produce, the cost of producing would be either 60 or 105 with equal probability and revenues would be 100 with certainty. After production occurred, the manager privately learned the cost, but the owner never learned the true cost. If the cost was 60, the manager was allowed to report 60 or 105. If the cost was 105, the manager was required to report 105 in order to honor the model's limited liability constraint -- the manager was not allowed to report in a way that would cause him or her to lose money. The owner committed to transfer funds to the manager contingent on the manager's cost report. The contingent transfers were constrained to be $t_{60} \in \{60, 61, 62, \ldots, 105\}$ and $t_{105} = 105$. Assuming the owner chose to produce, the manager learned the owner's commitment, as well as the cost realization, prior to making his or her cost report. The manager kept any funds transferred in excess of the actual cost.

Participants were randomly re-paired after every round. The earnings of the owner and manager in each round are calculated in Figure 1.

[Insert Figure 1 about here.]

In the unique subgame perfect equilibrium the manager would always report the cost is 105 and the owner, anticipating the manager's reporting behavior, would never produce. Therefore, if the owner and manager acted rationally, the agency would lose expected surplus equal to 100 - [.5(60)+.5(105)] = 17.5 due to the information asymmetry. It is this loss in expected surplus that provides a potential role for reciprocation of trust and honesty. This role is developed in further detail in section three.

2.2 *DEDUCT Treatment*

Play proceeded exactly as in the *ONESHOT* treatment, except that there was a 50% probability that the manager would suffer a fine f if he or she provided a report of 105 when the cost was 60. (In the administration of the experiment the term *deduction* was used.) The value of f was chosen by the owner from among the set {0, 1, 2, . . . , 45}. The manager learned the *value* of f before submitting his or her report, but. did not learn whether the deduction would be applied until after he or she reported. This amount was not a transfer from the manager to the owner, but was simply a deadweight loss to the manager if applied. Having the owner set f and not know whether it had been levied on the manager captures a setting with the following properties: (1) the owner's intent to control the manager is reflected in his or her choice of f[2] and (2) it retains the crucial feature of the *ONESHOT* treatment -- the owner can never learn with certainty whether the manager misreported the cost.

The subgame perfect equilibrium for play in this treatment is straightforward. The rational own-wealth-maximizing manager will prefer to report truthfully when the true cost is 60 if only if *incentive compatibility* (*IC*) holds.

$$t_{60} - 60 \geq 105 - .5f - 60 \qquad (IC)$$

[2] It is important that the owner and not the experimenter choose the value of f, as its choice indicates how the owner wishes to treat the manager. The experimental economics literature has shown that intentions matter (Falk et al. 2000).

The left-hand side is the manager's benefit for reporting truthfully, and the right-hand side is the expected benefit from reporting untruthfully. The difference between the left- and right-hand sides is the manager's net benefit of reporting truthfully -- it is the increase in expected slack obtained by telling the truth when the cost is 60. We denote this quantity by $\Delta SLACK$.

$$\Delta SLACK = t_{60} - [105 - .5\ f]$$

Given a wealth-maximizing manager, the owner should set f to the maximum of 45, in order to minimize t_{60}. With f set at 45 and t_{60} set at 82.5, the owner receives 6.25 in expectation per round and the manager receives 11.25 in expectation per round if production is chosen.[3] Because 6.25 exceeds zero -the amount the owner would receive by not producing - the owner would prefer to produce and set f to 45.[4]

2.3 *REPEAT Treatment*

Play in the *REPEAT* treatment proceeded exactly as that in the *ONESHOT* treatment, except that within a game subjects were paired with the same individual. After each game, participants were re-paired. This within-game repetition provides for the possibility that individual behavior could be implicitly contingent on the play observed in prior rounds. In fact, there now exist equilibria in which the owner would choose to produce in some periods.[5] However, the introduction of repeated interaction still leaves intact a repetition of the single-shot subgame perfect equilibrium in which the owner never produces.

3. Behavioral Predictions

The main purpose of this paper is to investigate the importance of intrinsic motivations, primarily *reciprocation of trust* and *honesty*, in a capital budgeting setting. The

[3] Although this penalty scheme is exogenously imposed, the penalty was not chosen arbitrarily. This penalty scheme, with f set at the maximum of 45, provides the *manager* with the same expected slack per period as the endogenously derived, optimal two-period contract found in Fellingham and Young (1990). We chose these parameters for the penalty because it would appear representative of the magnitude of enforcement the owner may apply in settings resembling ours. The penalty scheme is more favorable to the *owner* than the two-period contract in Fellingham and Young, but because we are primarily interested in the effects of different incentives on manager behavior, this is not of great concern.

[4] Given that t_{60} is restricted to integer values in the experiment, the expected per round owner and manager earnings are 6 and 11.5, respectively.

robustness of trust reciprocation previously observed in more abstract laboratory settings would seem to indicate its occurrence in our experiment is a foregone conclusion. However, as previously discussed, the capital budgeting setting we explore includes an important feature not found in prior research on intrinsic motivations: owners (first movers) often have limited ability to observe managerial (second mover) behavior. It is this feature that requires a manager to submit a report, bringing into question the managers' preference for honest reporting.

Below we describe the behavior expected under *rational own-wealth-maximization* (**MAX**) and also under the intrinsic motivations of *reciprocation of trust* (**RECIP**) and *honesty* (**HON**). In addition, we discuss potential interactions of intrinsic motivations with explicit and implicit incentives. Recall that the subject-managers had freedom to choose their reports only when the cost was 60. Therefore, the hypotheses concerning managerial reporting apply only to those observations where the cost was 60.

3.1 Manager reporting without explicit or implicit incentives

MAX implies that in the *ONESHOT* treatment, where there are neither explicit nor implicit incentives, $t_{60} = 105$ is required in order to elicit a truthful report. With $t_{60} = t_{105} = 105$, the owner receives a profit of - 5 and therefore should not produce. However, it is possible that the owner would attempt to produce anyway, perhaps because he or she does not believe the manager's behavior will follow **MAX**. For example, the owner might attempt to produce if he or she thought the manager would be willing to reciprocate a trust. If the owner believed the manager would be willing to reciprocate, it could be rational for the owner to *place a trust* in the manager by producing and setting t_{60} less than 105.

In the *ONESHOT* treatment, we define reciprocation of trust to occur whenever t_{60} is less than 105 and the manager reports the cost truthfully. We expect the likelihood of reciprocation of trust to be increasing in t_{60}. The reasoning behind this conjecture is best illustrated with a brief comparison of our setting to the *Investment Game* of Berg et al.

[5] A proof is available from the authors.

(1995). In the *Investment Game* a first-mover decides how much of a ten dollar initial endowment to give to a second-mover. Any transferred funds triple, with certainty. A second-mover then decided how much of the tripled transfer to send back to the first mover. Berg et al. (1995) found that the second mover returned (on average) some *but not all* of the tripled transfer. That is, the second mover has a threshold for how much he or she is willing to return. Our baseline treatment may be similarly described as a first-mover deciding whether or not to send five units to a second-mover. If sent, the five units become either zero or forty units. If forty units obtain the second-mover must decide whether to send back one hundred minus t_{60} or zero to the first mover. Therefore, the higher t_{60} the more likely the amount requested to be returned is within the threshold acceptable to the second-mover, assuming of course the second-movers are motivated to reciprocate trust at all.

One notable difference between our setting and Berg et al. (1995) is that the owners' choice of t_{60} fixes the level of potential reciprocation by the manager. In Berg et al. the second-mover was free to return any feasible level of resources. We include t_{60} in our design for two reasons. First, it would appear unlikely that a manager would be asked to submit a report and be left to his discretion regarding the level of resources to remit back to the owner. This would entail the manager essentially admitting to the misappropriation of funds. Second, a fully defined contract is both implied and necessary in the implementation of the explicit incentives treatment. It is implied, because it is the lack of discretion which has been hypothesized to crowd-out intrinsic motivations (Frey 1997). It is necessary, because without a binding level of funds to be remitted to the owner in the case of a report of 60, the manager could simply report truthfully that the cost was 60 in order to avoid the fine and then not remit any resources back to the owner.

Another reason why managers might be reluctant to report the cost untruthfully is that they have an inherent taste for honesty (**HON**). Consistent with the findings of Baiman and Lewis (1989) and Schwartz and Wallin (2002), if subjects exhibited a preference for honest we would expect the likelihood of an honest report to be increasing in the benefits

from honest reporting. The prediction under **HON** is thus identical to the prediction under **RECIP** -- the likelihood of an honest report should be increasing in the value of t_{60}. Hence, given our design, it would not be possible to distinguish between the manager's desire to reciprocate trust and the manager's preference for honesty. The hypotheses that correspond to the three behavioral assumptions are as follows.

ONESHOT-MAX: In the ONESHOT treatment managers will report truthfully if and only if $t_{60} = 105$.

ONESHOT-RECIP and ONESHOT-HON: In the ONESHOT treatment managers will report truthfully even if $t_{60} < 105$ and the incidence of truthful reporting will be positively associated with t_{60}.

3.2 Manager reporting with explicit incentives

MAX implies that in the *DEDUCT* treatment (*IC*) must hold to induce manager truthfulness: that is, $\Delta SLACK \geq 0$. **RECIP** and **HON** both imply that even if (*IC*) does not hold, some managers will still report truthfully and the likelihood of an honest report would be increasing in $\Delta SLACK$, as in the *ONESHOT* case.

DEDUCT-MAX1 Hypothesis: In the DEDUCT treatment, managers will report truthfully if and only if (IC) holds.

DEDUCT-RECIP and DEDUCT-HON Hypotheses: In the DEDUCT treatment, managers will report truthfully even when (IC) does not hold. Further, the likelihood of an honest report is increasing in $\Delta SLACK$.

If (*IC*) holds, **MAX** implies managers will report truthfully when the cost is 60. However, Fehr and Gächter (2000a) found that subject-employees sometimes acted as if they resented explicit incentives. That is, subject-employees displayed behaviors not desired by the owner, even though such behavior was incentive compatible. This occurred in about 20% of their observations. They conjecture that the use of a monetary incentive scheme sends a message of distrust, which prompts the subject to resent the scheme. We denote this type of behavior by *resentment of explicit incentives*, or **RESENT**. Under **RESENT**, the use of a fine in order to induce truthful disclosure might influence the manager to report untruthfully even though (*IC*) holds. If the inequality were nearly tight, the manager would suffer only a small loss by reporting untruthfully, whereas the owner would suffer a much greater loss. Owners

might try to overcome potential resentful behavior by building greater slack into (*IC*), that is, to increase *ΔSLACK*. Fehr and Gächter (2000a) observed that building more slack into the incentive compatibility constraint did overcome managers' resentment. The potential for resentful behavior by the manager generates the following alternative hypotheses.

DEDUCT-MAX2: In the DEDUCT treatment, the incidence of truthful reporting when (IC) holds will be independent of the magnitude of ΔSLACK.

DEDUCT-RESENT: In the DEDUCT treatment, the incidence of truthful reporting when (IC) holds will be positively associated with the magnitude of ΔSLACK.

3.3 Manager reporting with implicit incentives

In the *REPEAT* treatment **RECIP**, **HON** and **MAX** all could be consistent with some level of manager truth telling. If managers were motivated to reciprocate trust or had a preference for honesty, they might report truthfully even if $t_{60} < 105$. Also, we would expect them to respond to larger transfer offers with more truthful reporting, as in the *ONESHOT* treatment. Finally, even if managers were solely motivated by rational own-wealth-maximization, because of the repeated nature of interactions in *REPEAT*, larger transfer offers would better support equilibria that feature truth telling. Therefore, the following hypothesis can be derived from all three of these behavioral assumptions.

REPEAT-MAX, REPEAT-RECIP and REPEAT-HON Hypotheses: In the REPEAT treatment, the incidence of truthful reports by managers will be positively associated with the magnitude of t_{60}.

3.4. Interaction Between Explicit Incentives and Intrinsic Motivations

Thus far, we have considered the effects of each treatment in isolation. We now turn our attention to the interaction between explicit incentives and intrinsic motivations. If no interaction occurs, manager behavior in the *DEDUCT* treatment when (*IC*) does not hold would be identical to manager behavior in the *ONESHOT* treatment. In particular, the managers' propensity to report truthfully would be identical in the *DEDUCT* treatment and the *ONESHOT* treatment for a given cost of honesty (*ΔSLACK*). In contrast, if crowding out occurs, managers in the *DEDUCT* treatment would be less responsive to offers that were not incentive compatible than managers in the *ONESHOT* treatment for a given level of

ΔSLACK. The evidence from experimental labor markets on this issue is mixed -- Fehr and Gächter (2000a) found crowding out but Anderhub et al. (2002) found that extrinsic and intrinsic motivations can coexist. We present two alternative hypotheses.

DEDUCT/ONESHOT-NOINT: Given that (IC) does not hold, the incidence of managers reporting truthfully will be identical in the ONESHOT and DEDUCT treatments, holding constant the magnitude of ΔSLACK.

DEDUCT/ONESHOT-NEG: Given that (IC) does not hold, the incidence of managers reporting truthfully will be less in the DEDUCT treatment than in the ONESHOT treatment, holding constant the magnitude of ΔSLACK.

3.5 Interaction Between Implicit Incentives and Intrinsic Motivations

The issue of potential interaction of implicit incentives and intrinsic motivations is of particular importance, because most interactions within the firm are of a repeated nature. It is possible that if owners provide implicit incentives for truthful reporting in the *REPEAT* treatment, they will interact with the intrinsic motivations that have been so commonly observed. As of yet, this phenomenon has not been sufficiently addressed by theorists. The empirical evidence from Gächter and Falk (2001) and Falk et al. (1999) indicates that the effects of repeated play are actually complementary to the effects of intrinsic motivations rather than destructive. Unfortunately, it would be difficult to draw useful inferences about our study from those settings. For example, Gächter and Falk (2001) employ a known finite endpoint. Therefore, we do not put forth any alternative to the hypothesis that there is no interaction between implicit incentives from repeated play and intrinsic motivations.

REPEAT/ONESHOT-NOINT: The incidence of managers reporting truthfully will be equal in the REPEAT and ONESHOT treatments for a given value of t_{60} (which proxies for the pecuniary benefit of dishonesty).

Figure 2 summarizes the relationship between our experimental design and hypotheses.

[Insert Figure 2 about here.]

The above hypotheses all relate to manager behavior, although the behavior and welfare of owners should be of interest to researchers concerned with the capital budgeting problem. However, making inferences from our results to a more general setting would be

difficult, because owner behavior and welfare are likely to depend as much on parameterization as on the treatment manipulations.[6] Therefore, we do not present formal hypotheses concerning owner behavior and welfare.

4. Results

An overview of the results is found in Table 1. The data is segmented into halves, where Half 1 represents Games 1 through 4 and Half 2 represents Games 4 through 8. Perhaps the most striking aspect of the data shown in Table 1 is the minimal evidence of intrinsic motivations in the *ONESHOT* treatment. For example, the relative frequency of truthful reporting is only 17.4% in Half 2. This is in sharp contrast to the numerous prior experiments in which reciprocation of trust was found to be much more pervasive. Also of note is the higher relative frequency of truthful reporting in the *DEDUCT* versus *REPEAT* treatment. On the whole, the data suggest that extrinsic motivations may be more constructive in capital budgeting characterized by asymmetric information than in other experimental settings.

[Insert Table 1 about here.]

4.1 Analysis of ONESHOT Treatment

The first set of hypotheses concern the importance of intrinsic motivations in the *ONESHOT* treatment, where there are neither explicit nor implicit incentives. Table 2 presents the relative frequency of truthful reporting, conditioned on t_{60}. We expected that if intrinsic motivations were important in *ONESHOT*, truthful reporting would be increasing in the benefit of honesty, or equivalently, increasing in t_{60}.

[Insert Table 2 about here.]

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[6] For example, should owners produce in *ONESHOT*? The answer would depend on whether owners expected the level of managers' reciprocation of trust sufficient to support profitable production. This, in turn, would to a large degree depend on the parameters chosen. We certainly would expect some production given the significant observations of trusting behavior found in Fehr et al. (1997), Berg et al. (1995), Bolle (1998) and Guth et al. (1998). Owners would at least be expected to "test the waters". Similar ambiguity exists concerning related to owner behavior in the *DEDUCT* and *REPEAT* treatments.

The data in Table 2 indicate that managers showed a low incidence of intrinsically motivated behavior and were not particularly responsive to higher offers for a report of 60. This evidence is consistent with hypothesis *ONESHOT-MAX* and contrary to hypotheses *ONESHOT-RECIP* and *ONESHOT-HON*. This result is surprising, because honest reporting could have been motivated both by a preference for honesty and by a desire to reciprocate trust. Although the evidence in the literature on preferences for honesty is inconclusive, there has been substantial evidence that individuals reciprocate trust, as previously discussed. It appears that some aspect of our capital budgeting setting, possibly the private information of the manager, has inhibited the reciprocation of trust that has so often been observed in settings of complete information. In fact, given the parameters of our experiment, the choices of production made by owners and the responses of managers led to negative earnings on average for owners. These findings leave much room for potential increases in total welfare from using explicit or implicit incentives.

4.2 Analysis of DEDUCT Treatment

The next set of hypotheses concern the managers' behavior in the *DEDUCT* treatment, where owners were able to employ explicit incentives in the form of a deduction in order to make truth telling incentive compatible. In Table 3 we present the managers' reporting behavior in the *DEDUCT* Treatment, conditioned on whether or not (*IC*) was satisfied.

[Insert Table 3 about here.]

The data from Table 3 indicate that the managers did respond to the explicit incentives provided by the owner in the manner expected under **MAX**. For example, managers reported the cost truthfully 79.6% (28.3%) of the time when (*IC*) was satisfied (not satisfied) in the second half of the experiment. This result supports hypothesis *DEDUCT-MAX1*, but we cannot ignore the possibility that subjects were also affected by intrinsic motivations in the *DEDUCT* setting.

In order to investigate whether managers in the *DEDUCT* treatment responded to intrinsic motivations as well as explicit incentives, we present the results from Logistic Regression 1, which includes all observations from the *DEDUCT* treatment wherein: (1) production occurred, (2) the cost was 60 and (3) (*IC*) *was not* satisfied. We include MANSPECIFIC as a control variable, because the regression uses multiple observations from the same subject-manager (Slonim and Roth 1998). With this regression, we examine whether the managers' willingness to report truthfully was increasing in $\Delta SLACK$ in those situations where truth telling was not incentive compatible. The purpose is to address whether managers' intrinsic motivations co-exist with the explicit incentive scheme.

Logistic Regression 1: $TRUE_{it} = f(\beta_0 + \beta_1 \Delta SLACK_{it} + \beta_2 MANSPECIFIC_{it} + \varepsilon_{it})$
where: $f(x) = e^x/(1+e^x)$
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$\Delta SLACK_{it} = t_{60} - (105 - .5\,f)$ for manager i in round t
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

[Insert Table 4 about here.]

The results from Logistic Regression 1 indicate that in the presence of explicit incentives managers still responded to an increase in $\Delta SLACK$, by increasing the relative frequency of truthful reporting. This result has a very different qualitative nature than that obtained by Fehr and Gächter (2000a), who found that when contracts were not incentive compatible, an increase in fixed wages did little to increase effort in their explicit incentives treatment.

We also wished to investigate managers' reporting behavior when (*IC*) *was* satisfied. We hypothesized that managers might resent the owners' attempts to discipline the managers with the use of the penalty. We hypothesized that the owner may appease the manager's resentment by increasing $\Delta SLACK$. We present a test of this hypothesis with Logistic Regression 2, shown below. With this regression we examine the association between manager reporting and $\Delta SLACK$ for those instances where (*IC*) held.

Logistic Regression 2 includes all observations from the *DEDUCT* treatment wherein: (1) production occurred, (2) the cost was 60 and (3) (*IC*) *was* satisfied. The results of Logistic Regression 2, summarized in Table 5, indicate that the likelihood of a truthful report when truth telling was incentive compatible was increasing in the benefits of honesty. This result is consistent with an interpretation of manager's resentment being eased by increasing the remuneration for a truthful report. Another possible interpretation is that an increase in *ΔSLACK* increased the saliency of incentive compatible truth telling, and overcame the noise generated from the bounded rationality of the subjects. Given the design of our experiment, it is not possible to distinguish between these two explanations. However, because the results from Logistic Regression 1 imply that in *DEDUCT* intrinsic motivations exist in the presence of explicit incentives, the saliency explanation may be more plausible than the easing of resentment.

Logistic Regression 2: $TRUE_{it} = f(\beta_0 + \beta_1\ \Delta SLACK_{it} + \beta_2\ MANSPECIFIC_{it} + \varepsilon_{it})$
where: $f(x) = e^x/(1+e^x)$
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$\Delta SLACK_{it} = t_{60} - (105 - .5f)$ for manager i in round t
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

[Insert Table 5 about here.]

4.3 Analysis of REPEAT Treatment

We hypothesize that in the *REPEAT* treatment either intrinsic motivations or implicit incentives arising from repeated play would support the conjecture that higher levels of t_{60} would increase the relative frequency of truthful reporting by the managers. Table 6, which presents the relative frequency of truthful reporting conditioned on the level of t_{60}, clearly reveals that higher levels of t_{60} led to a higher incidence of truthful reporting by managers. A comparison of the responsiveness of managers to t_{60} in the *REPEAT* and *ONESHOT* treatments will be analyzed later in this section.

[Insert Table 6 about here.]

4.4 Analysis of Interaction Between Extrinsic and Intrinsic Motivations

In conducting this experiment, we were interested in both the prominence of intrinsic motivations and the potential for interaction with extrinsic motivations in a capital budgeting setting. So far we have seen that in the *ONESHOT* treatment (our baseline setting) intrinsic motivations appear to play only a minor role in role in determining manager behavior, whereas in the *DEDUCT* treatment both explicit incentives and intrinsic motivations affected manager behavior. Not only did we not find motivational crowding out as might have been expected from prior literature, but we appear to have found a "crowding in" effect, wherein explicit incentives *enhance* intrinsic motivations. To more formally investigate this issue we present Logistic Regression 3, which includes all observations from the *ONESHOT* treatment wherein: (1) production occurred and (2) the cost was 60 and all observations from the *DEDUCT* treatment where (1) production occurred, (2) the cost was 60 and (3) (*IC*) did not hold.

Logistic Regression 3:

$TRUE_{it} = f(\beta_0 + \beta_1\ \Delta SLACK_{it} + \beta_2\ ONESHOT_{it}\ \Delta SLACK_{it} + \beta_3\ MANSPECIFIC_{it} + \varepsilon_{it})$,

where: $f(x) = e^x/(1+e^x)$

$TRUE_{it}$ = 1, if manager i reported truthfully in round t; 0 otherwise.

$\Delta SLACK_{it} = t_{60} - (105 - .5\ f)$ for manager i in round t.

$ONESHOT_{it}$ = 1 if manager i in round t is in the *ONESHOT* treatment and 0 otherwise.

$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

[Insert Table 7 about here.]

If crowding out had occurred, we would expect the coefficient on the interaction term in Table 7 (β_2) to be positive, indicating increasing sensitivity to the benefits of honesty in the *ONESHOT* treatment relative to the *DEDUCT* treatment. Instead, we found β_2 to be negative and significant, indicating the introduction of explicit incentives enhanced, rather than inhibited, intrinsic motivations.

Finally, we wished to investigate whether there would be an interaction between implicit incentives and intrinsic motivations. In order to investigate this question, we use Logistic Regression 4, which includes all observations from the *ONESHOT* and *REPEAT*

treatments where production occurred and the cost was 60. As a proxy for the pecuniary benefits of honesty we use t_{60}, because we can think of these treatments as restricted versions of *DEDUCT* wherein $f = 0$. The results are found below in Table 8. As with explicit incentives, implicit incentives appear to enhance, rather than crowd out, intrinsic motivations.

Logistic Regression 4:
$TRUE_{it} = f(\beta_0 + \beta_1\, t_{60it} + \beta_2\, ONESHOT_{it} t_{60it} + \beta_3\, MANSPECIFIC_{it} + \varepsilon_{it})$,
where: $TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$t_{60it} = t_{60}$ offered to manager i in round t.
$ONESHOT_{it} = 1$ if manager i in round t is in the *ONESHOT* treatment and 0 otherwise.
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager *i* over all rounds excluding round *t*.

[Insert Table 8 about here.]

5. Discussion and Conclusions

We find no evidence of crowding out, which runs counter to much of the prevailing literature. This finding does however, appear to be consistent with the arguments presented in Kunz and Pfaff (2002). They re-interpret several existing meta-analyses and conclude that "the antecedents for such effects [that undermine intrinsic motivation] . . . seldomly prevail in business." In business, they claim, individuals should find it reasonable that they are rewarded for performance. Further, the tasks performed by employees are often not intrinsically rewarding. Therefore, they argue that motivational crowding should be of minimal concern in institutional design.

In our experiment, the task was not rewarding *per se*. However, intrinsic rewards potentially could be derived from the subjects' adherence to the norms of honesty and reciprocation of trust. Whether the subjects expected an explicit incentive scheme as found in our *DEDUCT* treatment is unknown, and we will not conjecture on this point. What we can say is that the crowding of reciprocation of trust is exactly what was found by Fehr and Gächter (2000a) in their study. What differentiates our study from Fehr and Gächter (2000b), as well as other studies on reciprocation of trust, is that ours is characterized by asymmetric

information, which we consider to be an integral part of settings where management control is an issue. The information asymmetry allows the manager-subject to obtain slack, undetected by their owner-partner. Hence, when manager-subjects believe they are acting with great anonymity they might be more likely to violate social norms, which may be the explanation for why extrinsic rewards were so important in this setting.

The primary implication for capital budgeting and other accounting settings where management control is important is that careful attention must be paid to the precise nature of the environment under consideration. We found minimal evidence of intrinsically motivated behavior. In contrast, Evans et al. (2001), found the influence of preferences for honesty to be very important in a capital budgeting setting similar to our own. There were several differences between our experiment and Evans et al. (2001) -- for example, their owners were hypothetical and managers had greater discretion over how much they left to owners. These differences may provide the basis for future exploration into the conditions under which intrinsic motivations become important and when researchers and decision makers should consider the possible impact of intrinsically motivated behavior and motivational crowding.

Appendix: Instructions

Instructions for ONESHOT Treatment:

YOU MAY NOT SPEAK TO ANY OTHER PARTICIPANT
ONCE THE EXPERIMENT HAS STARTED.

General. Thank you for participating in this experiment. Throughout the experiment, you will accumulate points that will be converted to cash at the end of the experiment. You will receive $1.00 for every 30 points you earn, in addition to a $6.00 flat fee. The decisions you make will determine your point total (and thus, the cash payment you receive).

Your Task. You will be assigned the role of either a Type O player or a Type M player. During the experiment you will have the opportunity to play both roles. For each round of the experiment players will be paired. Each pair will consist of a Type O and a Type M player.

Type O Players. A Type O player has available to them an investment opportunity each round of the experiment. The investment opportunity, if taken, will return 100 points (this is accomplished by entering 100 into the computer). The cost of the investment will be either 60 points or 105 points, each equally likely. At the beginning of the round, the Type O player decides whether or not to take the investment opportunity. In order to execute the investment a Type M player is necessary. If the investment opportunity is taken, the Type O player must decide how many points to pay the Type M player. The payment in points will be dependent on a report given by the Type M player to the Type O player on how much the investment costs. If a report of 105 is received the payment to the Type M player is 105. If a report of 60 is received, the payment may range from 60 to 105 (the Type O player enters the amount into the computer when prompted). The Type O player decides the amount of the payment before a report is received.

The Type O player never learns the true cost of the investment.

If the investment opportunity is not taken, Type O player receives 0 points for that round.

Type M Players. If the Type O player in the pair decides to take the investment opportunity, the Type M player will be notified of the offered payment for a report of 60, (remember the payment for a report of 105 is always 105) and the actual cost. The Type O player never learns the actual cost. The Type M player is now prompted for a report. If the cost is actually 105 then a report of 105 must be given. If the cost is 60 then a report of 60 or 105 may be given. The Type M player will receive points equal to 105 (if a report of 105 is given) or the payment for a report of 60 (if a report of 60 is given, less the actual cost. After the report is given the computer will calculate each players' net points for the round, and will update the total points earned for the experiment.

Net point summary.
If investment opportunity is taken.
Type O player: Net points = 100 - payment to Type M player.
Type M player: Net points = payment from Type O player - actual cost.

Both Type M and Type O players receive 0 points if investment opportunity is not taken.

Length of the Experiment. The experiment will consist of a training phase and a cash-earning phase. The training phase lasts 8 rounds. During the first 4 rounds you will

consistently play either the Type O or Type M role. For training rounds 5 through 8 you will play the role you were not assigned in rounds 1 through 4. Points in the training rounds will not count towards cash payments. The cash-earning rounds will consist of 8 games. Each game will consist of 6 rounds with certainty. After the sixth round a die will be rolled. If the outcome is a 1, the game will end, otherwise the game will continue for at least one more round after which the die will be rolled again. This will continue until a 1 is rolled. You will switch roles after every game.

Sequence. 1. Type O player decides whether to invest in opportunity

Steps 1 through 5 assume investment opportunity has been taken, is the opportunity is not taken, then both players receive 0 points for the round.

2. Type O player decides how much to pay the Type M player for a report of 60.
3. Type M player observes the true cost and the payment for a report of 60.
4. Type M player enters a cost report.
5. Type O player receives the cost report, both players points are updated.

Pairings. The computer will randomly re-pair players after every round. It is possible to play with the same person in two consecutive rounds, the probability of this event is dependent on the number of people participating in the experiment.

Payment. At the end of the experiment, please remain seated until someone has recorded your earnings. You will be called individually to receive your payment, in private. The amount you receive, will not be disclosed by the experimenter to any other participant.

Questions. If, at any time during the experiment, you have a question, please raise your hand and someone will assist you.

REMEMBER, ONCE YOU ARE SEATED,
YOU MAY NOT COMMUNICATE WITH ANY OTHER PARTICIPANT.

Instructions for REPEAT Treatment:

*The instructions for **REPEAT** are identical to those for **ONESHOT**, with the exception of the "Pairings" paragraph:*

Pairings. The computer will randomly re-pair players after every game. During a game you will be paired with the same partner. You will also be paired with the same partner for rounds 1 through 4 and 5 through 8 in the training phase.

Instructions for DEDUCT Treatment:

Instructions for DEDUCT are the same as for ONESHOT up until "Type O Players":

Type O Players. A Type O player has available to them an investment opportunity each round of the experiment. The investment opportunity, if taken, will return 100 points. The cost of the investment will be either 60 points or 105 points, each equally likely. The Type

O player is not informed of the cost. The Type M player in the pair is aware of the cost. As a Type O player you will have to make up to three decisions each round. The first decision is whether or not to take the investment opportunity. If the investment is not taken, then both the Type O and Type M player in the pair will receive 0 points for the round. If the investment opportunity is taken, the Type O player must decide how many points to pay the Type M player. The payment in points will be dependent on a report given by the Type M player to the Type O player on how much the investment costs. If a report of 105 is received the payment to the Type M player must be 105, the computer does this automatically. If a report of 60 is received, the payment may be any whole number from 60 to 105 (the Type O player enters this amount into the computer when prompted). If the investment is taken, the Type O player must decide on a potential point deduction for the Type M player. The point deduction has a 50% chance of applying if the Type M player reports the cost as 105 when it is in fact 60. The point deduction will not apply in any other circumstances. The points will be deducted from the Type M player's total, but will not be given to the Type O player, nor will the Type O player be aware whether the deduction was applied. The deduction may be any whole number between 0 and 45. If the investment is taken the Type O player will receive points equal to 100 – the payment to the Type M player.

Type M Players. If the Type O player in the pair decides to take the investment opportunity, the Type M player will be notified of the offered payment for a report of 60, (remember the payment for a report of 105 is always 105), the actual cost, and the potential deduction. The Type M player is now prompted for a report. If the cost is actually 105 then a report of 105 must be given. If the cost is 60, the Type M player must decide whether to report 60 or 105. If the cost is 105, the Type M player receives 0 for the period. If the cost is 60 and the Type M player reports 60, the Type M player will receive the payment for a report of 60 (decided earlier by the Type O player) minus 60. If the cost is 60 and the Type M player reports 105, the Type M player receives the payment for a report of 105 (always set to 105), minus 60, and also minus the deduction if it is applied. Remember there is a 50% chance the deduction will be applied if the Type M player reports the cost as 105 when it is 60. After the report is given, the computer will calculate each player's net points for the round and will update the total points earned for the experiment.

Net point summary.
If investment opportunity is taken:

Type O player: Net points = 100 - payment to Type M player.
Type M player: Net points = payment from Type O player - actual cost – 50% chance of deduction if cost of 60 is reported as 105.

If investment opportunity is not taken:

Both Type M and Type O players receive 0 points.

Length of the Experiment. The experiment will consist of a training phase and a cash-earning phase. The training phase lasts 8 rounds. During the first 4 rounds you will consistently play either the Type O or Type M role. For training rounds 5 through 8 you will play the role you were not assigned in rounds 1 through 4. Points in the training rounds will not count towards cash payments. The cash-earning rounds will consist of 8 games. Each game will consist of 6 rounds with certainty. After the sixth round a die will be rolled (performed by the computer). If the outcome is a 1, the game will end, otherwise the game will continue for at least one more round after which the die will be rolled again. This will continue until a 1 is rolled. You will switch roles after every game.

Sequence. 1. Type O player decides whether to invest in opportunity

Steps 1 through 5 assume investment opportunity has been taken; if the opportunity is not taken, then both players receive 0 points for the round.

2. Type O player decides how much to pay the Type M player for a report of 60, and the amount of the potential deduction.
3. Type M player observes the true cost, the payment for a report of 60 and the potential deduction.
4. Type M player enters a cost report.
5. Type O player receives the cost report, both players' points are updated pursuant to the procedures described above.

Pairings. The computer will randomly re-pair players after every round. It is possible to play with the same person in two consecutive rounds, the probability of this event is dependent on the number of people participating in the experiment.

The instructions that followed "Pairings" were the same as for ***ONESHOT.***

Figures

	Owner	Manager
No production	0	0
Production: Manager reports 60 Manager reports 105	 100 - t_{60} 100 - 105 = -5	 t_{60} - 60 105 - actual cost

Figure 1: Owner and manager earnings per round

Figures (continued)

Treatment	*REPEAT*	*ONESHOT (Baseline)*	*DEDUCT*
Description	Repeated interactions. No deduction available	One-shot interactions. No deduction unavailable.	One-shot interactions. Deduction available.
Motivations	Implicit incentives possible. Explicit incentives not possible. Intrinsic motivation possible.	Implicit incentives not possible. Explicit incentives not possible. Intrinsic motivation possible.	Implicit incentives not possible. Explicit incentives possil Intrinsic motivation poss
Results	*MAX* Incidence of truth increasing in t_{60}. *RECIP-HON* Incidence of truth increasing in t_{60}.	*MAX* Truth iff t_{60}=105. *RECIP-HON* Truth when t_{60}<105 and incidence of truth increasing in t_{60}.	*MAX1* Truth only if (*IC*) holds. *MAX2* Any deviation from Tru when (*IC*) holds will be independent of *ΔSLACK*. *RECIP-HON* Truth even when (*IC*) do not hold and incidence o truth increasing in *ΔSLA* *RESENT* Incidence of truth increa in *ΔSLACK*.
		NO INTERACTION For a given level of *ΔSLACK* < 0, the incidence of truth is identical in *ONESHOT* and *DEDUCT*. *NEG* For a given level of *ΔSLACK* < 0, the incidence of truth is in *DEDUCT* than in *ONESHOT*.	
	NO INTERACTION For a given value of t_{60}, the incidence of truth is identical in ONESHOT and REPEAT.		

Figure 2: Summary of hypotheses concerning intrinsic motivation, implicit incentives and explicit incentives

Tables

	ONESHOT Treatment		*DEDUCT* Treatment		*REPEAT* Treatment	
	Half 1	Half 2	Half 1	Half 2	Half 1	Half 2
Relative frequency of production	45.3%	20.5%	80.2%	71.0%	70.6%	58.2%
Number of contracts	811	658	911	840	625	680
Mean of t_{60} where production occurred	75.63	77.33	82.56	84.79	75.88	78.48
Relative frequency of reports of 60 where production occurred and cost was 60	18.1%	17.4%	57.7%	69.3%	39.0%	42.2%
Number of incidences where production occurred and cost was 60	182	69	357	300	210	206

Table 1
Overview of results

Tables (continued)

t_{60}	60-64	65-69	70-74	75-79	80-84	85-89	90-94	95-99	100-104	105
Relative frequency of honest disclosures	12.5%	15.8%	6.7%	17.9%	16.7%	35.3%	20.9%	18.2%	0.0%	50.0%
n	64	19	15	39	30	17	43	11	5	8

Table 2
Relative frequency of truthful reports in *ONESHOT* treatment conditioned on t_{60}

n = number of incidences where owners chose production and cost was 60 (number opportunities to report untruthfully)

Tables (continued)

	Half 1: Games 1 to 4	Half 2: Games 4 to 8
(*IC*) holds (Truth is incentive compatible.)	75.1% n = 209	79.6% n = 240
(*IC*) does not hold (Truth is not incentive compatible.)	33.1% n = 148	28.3% n = 60

Table 3
Relative frequency of truthful reports in the DEDUCT treatment conditioned on whether (*IC*) holds

n = number of incidences where owner chose production and cost is 60 (number opportunities to report untruthfully)

Tables (continued)

Logistic Regression 1: $TRUE_{it} = f(\beta_0 + \beta_1 \Delta SLACK_{it} + \beta_2 MANSPECIFIC_{it} + \varepsilon_{it})$, where:
$f(x) = e^x/(1+e^x)$
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$\Delta SLACK_{it} = t_{60} - (105 - .5f)$ for manager i in round t
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

	Coefficient (p-value)
Intercept	-.476 (.1383)
ΔSLACK	.158 (.0001)
MANSPECIFIC	2.502 (.0001)

Table 4
The relationship between *ΔSLACK* and honest reporting in *DEDUCT*
Includes only instances where (*IC*) was not satisfied and cost was 60

Logistic Regression 1 includes all observations from the *DEDUCT* treatment wherein: (1) production occurred, (2) the cost was 60 and (3) (*IC*) *was not* satisfied.

Tables (continued)

Logistic Regression 2: $TRUE_{it} = f(\beta_0 + \beta_1 \Delta SLACK_{it} + \beta_2 MANSPECIFIC_{it} + \varepsilon_{it})$, where:
$f(x) = e^x/(1+e^x)$
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$\Delta SLACK_{it} = t_{60} - (105 - .5f)$ for manager i in round t
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

	Coefficient (p-value)
Intercept	-4.125 (.0001)
ΔSLACK	.365 (.0001)
MANSPECIFIC	5.726 (.0001)

Table 5
The relationship between *ΔSLACK* and truthful reporting in *DEDUCT*
(includes only instances where (*IC*) was satisfied and cost was 60)

Logistic Regression 2 includes all observations from the *DEDUCT* treatment wherein: (1) production occurred, (2) the cost was 60 and (3) (*IC*) *was* satisfied.

Tables (continued)

t_{60}	60-64	65-69	70-74	75-79	80-84	85-89	90-94	95-99	100-104	105
Relative frequency of honest disclosures	18.7%	12.9%	31.0%	34.2%	47.7%	54.8%	62.2%	75.0%	0.0%	80.0%
n	64	31	29	76	65	93	37	12	4	5

Table 6
Relative frequency of truthful reports in *REPEAT* treatment conditioned on t_{60}

n = number of incidences where owner chose production and cost was 60 (number of opportunities to report untruthfully)

Tables (continued)

Logistic Regression 3:
$TRUE_{it} = f(\beta_0 + \beta_1 \Delta SLACK_{it} + \beta_2\ ONESHOT_{it}\ \Delta SLACK_{it} + \beta_3\ MANSPECIFIC_{it} + \varepsilon_{it})$, where:
$f(x) = e^x/(1+e^x)$
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$\Delta SLACK_{it} = t_{60} - (105 - .5\ f)$ for manager i in round t.
$ONESHOT_{it} = 1$ if manager i in round t is in the *ONESHOT* treatment and 0 otherwise.
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager i over all rounds excluding round t.

	Coefficient (p-value)
Intercept	-1.043 (.8497)
ΔSLACK	.142 (.0001)
ONESHOT ΔSLACK	-.097 (.0003)
MANSPECIFIC	3.519 (.0001)

Table 7
Relationship between *ΔSLACK* and truthful reporting in *DEDUCT* and *ONESHOT* treatments

Logistic Regression 3 includes all observations from the *ONESHOT* treatment where production occurred and the cost was 60 and all observations from the *DEDUCT* treatment where production occurred, the cost was 60 and (*IC*) was not satisfied.

Tables (continued)

Logistic Regression 4:
$TRUE_{it} = f(\beta_0 + \beta_1 * t_{60it} + \beta_2 * ONESHOT_{it} * t_{60it} + \beta_3 * MANSPECIFIC_{it} + \varepsilon_{it})$, where:
$TRUE_{it} = 1$, if manager i reported truthfully in round t; 0 otherwise.
$t_{60it} = t_{60}$ offered to manager i in round t.
$ONESHOT_{it} = 1$ if manager i in round t is in the *ONESHOT* treatment and 0 otherwise.
$MANSPECIFIC_{it}$ = mean relative frequency of truthfulness for manager *i* over all rounds excluding round *t*.

	Coefficient (p-value)
Intercept	-5.931 (.0001)
ΔSLACK	.052 (.0001)
ONESHOT ΔSLACK	-.007 (.012)
MANSPECIFIC	3.496 (.0001)

Table 8
Relationship between *ΔSLACK* and truthful reporting in *REPEAT* and *ONESHOT* treatments

Logistic Regression 4 includes all observations from the *ONESHOT* and *REPEAT* treatments where production occurred and the cost was 60.

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