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Abstract

Productivity Under Group Incentives: An Experimental Study

Although the United States still leads the world in worker productivity, there is some concern that the rate of productivity growth we have experienced over the past two decades may not be sufficient to maintain our leading position as we enter the next century. One possible avenue through which productivity can be increased is through the design and implementation of carefully crafted incentive compensation systems, which will induce workers to work both harder and smarter.

In this paper we take a first, experimental examination of this possibility. We report on a set of 13 experiments run using 588 paid human volunteers, the purpose of which was to investigate the problem of group moral hazard and the performance characteristics of a variety of group and individual incentive schemes. The specific schemes we investigate range from simple Revenue Sharing (egalitarian partnerships) to more sophisticated, target-based systems such as Profit Sharing or so-called Productivity Gainsharing. These are prototypical real-world incarnations of what economists call "forcing contracts." We also examined the properties of team-based tournaments, denoted here as "Competitive Teams" in which intra-firm competition, say between profit centers, is created so that relative performance becomes the basis of incentive awards. The performance of all these group incentive systems is then compared to that of individual incentive systems characterized by probabilistic monitoring and efficiency wages. Our conclusions are straight forward and can be summarized by five simple observations.

Observation 1: Shirking Happens

When experimental subjects are placed under an incentive plan which provides strong incentives to shirk, their effort levels do approach the shirking equilibrium as they near the end of the experiment.

Observation 2 History Matters

The performance of an experimental group using any particular group incentive formula depends on the effort norm established by this group in its previous experience with other incentive schemes.

Observation 3: A Little Competition goes a Long Long Way

Tournament-based group incentive mechanisms that create competition between sub-groups in the organization for a fixed set of prizes (i.e. which create internal tournaments) determine higher mean outputs than all target based mechanisms examined and smaller variances in those outputs than many of them.

Observation 4: Vulnerability Matters

The output of groups functioning under incentive plans which implement Pareto-optimal effort levels as Nash equilibria will be greater the less "vulnerable" (i.e. risky) are those equilibria to deviations or mistakes by the group agents.

Observation 5: Monitoring Works But is Costly

When monitoring is possible but not perfect, high levels of effort can be elicited from workers. However, unless the probability of detection is great (and therefore costly to maintain), such monitoring schemes are likely to fail.

Section 1: Introduction

Although the United States still leads the world in worker productivity, there is some concern that the rate of productivity growth we have experienced over the past two decades may not be sufficient to maintain our leading position as we enter the next century. In the effort to strengthen American competitiveness, a great deal of attention has been placed on developing technology and encouraging investment in both the physical and human capital used in production. But there is another, more behavioral avenue, to achieve productivity improvement: through the design and implementation of carefully crafted incentive compensation systems, American workers may be induced to work both harder and smarter, that is, to use even existing technologies in new and better ways that enhance their productivity. This approach has been given less attention, especially among economists, but, at least in the short-run and perhaps beyond, it could well prove to be the most effective instrument.

Certainly American business appears to have grasped this potential. Over the last decade or more, there has been a dramatic move in American industry away from individual-based incentives towards group incentives or so-called "variable pay." Indeed more and more American workers have a portion of their compensation contingent on some measure of firm or organizational performance. The sources and consequences of this development have received insufficient attention in the economics literature especially on the empirical side.

The paucity of empirical research in this area is quite surprising given the substantial theoretical advances that have been made in the analysis of labor contracting. The work in contract theory has shed considerable light on the nature of optimal contracts under alternative assumptions about the level and distribution of information amongst contracting parties and differences in their respective attitudes towards risk. But until very recently, there has been no empirical study of the pertinent theoretical results in relation to group incentives. What has emerged in the last several years are a number of rigorous econometric studies of the links between profit sharing (and/or employee stock ownership) and labor productivity. (For an excellent and thorough review of this econometric literature see Weitzman and Kruse, 1990). But to our knowledge there has been no direct investigation of the **relative** performance of alternative group incentive systems, something which is of considerable academic and practical interest given the huge variety of group incentive systems actually employed by firms.

In this paper we take a first, experimental, step in rectifying this omission. We report on a set of 13 experiments run using 588 paid human volunteers, the purpose of which was to investigate the problem of group moral hazard and the performance characteristics of a variety of group and individual incentive schemes. Unlike previous studies, we focus directly on the reward formulae themselves, trying to illuminate the behavioral and operational mechanics of the incentive structures they define. The specific schemes we investigate range from simple Revenue Sharing (egalitarian partnerships) to more sophisticated, target-based systems such as Profit Sharing or so-called Productivity Gainsharing. These are prototypical real-world incarnations of what economists call "forcing contracts." We also examined the properties of team-based tournaments, denoted here as "Competitive Teams" in which intra-firm competition, say between profit centers, is created so that relative performance becomes the basis of incentive awards. The performance of all these group incentive systems is then compared to that of individual incentive systems characterized by probabilistic monitoring and efficiency wages.

Our conclusions are straight forward and can be summarized by five simple observations.

Observation 1: Shirking Happens

When experimental subjects are placed under an incentive plan which provides strong incentives

to shirk, their effort levels do approach the shirking equilibrium as they near the end of the experiment.

This conclusion simply confirms a stylized observation, found by many in the public goods literature (see, Issac, McCue and Plott (1985), and Ledyard (1994, Forthcoming)) in which free riding is observed as an experiment is repeated.

Observation 2 History Matters

The performance of an experimental group using any particular group incentive formula depends on the effort norm established by this group in its previous experience with other incentive schemes.

Observation 2 simply means that how a group incentive formula will work when imposed on a specific group will depend on how that group was performing before the imposition. Formulae with good incentive properties do well when imposed on groups with positive (i.e. non-shirking) effort norms. Even good schemes can be adversely affected by bad histories.

Observation 3: A Little Competition goes a Long Long Way

Tournament-based group incentive mechanisms that create competition between sub-groups in the organization for a fixed set of prizes (i.e. which create internal tournaments) determine higher mean outputs than all target based mechanisms examined and smaller variances in those outputs than many of them.

This observation implies that fixed targets (whether set exogenously as in Profit Sharing or endogenously as in Gainsharing) are less good at motivating workers in their jobs than are relative performance mechanisms where the performance of the group is judged by a comparison to another, comparably situated, performance group.

Observation 4: Vulnerability Matters

The output of groups functioning under incentive plans which implement Pareto-optimal effort levels as Nash equilibria will be greater the less "vulnerable" are those equilibria to deviations or mistakes by the group agents.

This observation basically demonstrates the possibility that all Pareto-Optimal Nash equilibria are not created equal. Some are more risky (or more vulnerable) than others in the sense that in some mechanisms, if an agent adheres to the Pareto-optimal equilibrium while others deviate, that agent's loss in utility may be quite substantial while, under other mechanisms, the loss in utility suffered may be quite mild. As a result, what happens out of equilibrium may greatly affect the probability that a mechanism's Pareto-optimal equilibrium will be selected.

Observation 5: Monitoring Works But is Costly

When monitoring is possible but not perfect, high levels of effort can be elicited from workers. However, unless the probability of detection is great (and therefore costly to maintain), such monitoring schemes are likely to fail.

In this paper we will proceed as follows: In Section 2 we will motivate our work by presenting a quick overview of the industrial use of group incentive or variable pay schemes in the United States and the empirical research estimating their effectiveness. In Section 3 we will make our discussion more precise by presenting the theory behind the exact group incentive mechanisms we are considering. Section 4 reviews our experimental design while Section 5 presents our results by substantiating the five observations listed above. Finally, Section 6 offers some conclusions.

Section 2: Industrial Practice and Previous Empirical Research

The records of the Treasury indicate that there were 2113 qualified deferred and combination-cash profit sharing plans operating in the United States in 1945. By 1974 that figure had risen to

186,499; ten years later the number stood at 374,557. Except for a brief period of decline during the mid-seventies and more recently in 1984-85, the pace of growth evidenced in the intervening period has been steadily increasing. It is estimated that of the more than 450,000 profit-sharing plans in the U.S. (as of 1989), a little over one quarter maintain immediate cash provisions. A far smaller percentage of profit-sharing firms rely exclusively on current cash payments. While larger firms predominate among the profit sharers (indeed many of the country's largest firms are conspicuous among the profit sharers), an increasing number of small and medium sized firms have adopted profit sharing. If public utilities and hospitals, where profit sharing appears most infrequently are excluded, it is estimated that 25 to 30 percent of all firms have some form of profit sharing in operation.

Productivity gainsharing and employee share ownership have also become quite common among American firms. The New York Stock Exchange (1982) study estimated that 7 percent of all firms with 500 or more employees had some form of productivity gainsharing program as of 1982. This percentage has increased dramatically in the past five years as gainsharing has proved to be among the most popular of share concepts. Indeed, a more recent survey found that 13% of responding firms had some kind of gainsharing plan (O'Dell and McAdams (1987)). Company management appears drawn to the more narrowly focused performance measures typical of gainsharing; these plans target productivity variables more directly controlled by the concerned employees and encourage worker involvement in the organization of work. Kendrick (1987) reports that there were approximately 5000 employee stock bonus and ESOP plans in place as of 1984. This number too has grown substantially through the eighties aided in part by the role of ESOPs in management takeover defenses and company buyouts. Current estimates are that between 7,000 to 8,000 U.S. companies now maintain employee stock ownership plans (Blasi (1988)). More significantly, Blasi and Kruse (1991) project that by the year 2000 more than one quarter of publicly traded companies on the New York, American, and Over the Counter exchanges will be more than 15% owned by their employees. And the companies involved represent a sizable share of the American economy with respect to sales, market value, and employment. Clearly employee ownership is an important broad-based trend.

With respect to empirical research, there have been numerous field studies detailing company experiences with group incentives. (See, for example, Graham-Moore and Ross, 1983; Fein 1982; National Commission on Productivity and Work Quality, 1975). There have also been some simple correlational studies examining the relationship between specific forms of group incentives and various measures of firm performance and/or labor productivity (Howard and Dietz, 1969; Metzger and Colletti, 1971; Metzger, 1975). In addition, a number of industrial surveys have been conducted that compare and evaluate the effectiveness of broad classes of group incentive plans. (General Accounting Office, 1981; New York Stock Exchange, 1982). Overwhelmingly, the assessments of group incentives offered in these studies are positive, though the variance of productivity effects is considerable.

Some recent studies of group incentives have utilized more sophisticated statistical techniques applied to time series and cross sectional data. For example, Wagner et., al (1988) combined an interrupted time series design with intervention analysis to track the long-term effects of a gainsharing program at a midwestern foundry. They found that the introduction of the plan resulted in large increases in labor productivity, with post-intervention productivity growing as an asymptotic power function. Fitzroy and Kraft (1986; 1987) employed simultaneous WLS-Tobit estimation methods to examine the effects of profit sharing and employee share ownership on factor productivity and firm profitability in a sample of 65 medium-sized firms in the West German metal working industry. They showed strong positive effects in both areas "with none of the 'feedback' from profits to profit sharing which is sometimes hypothesized" (1986, p. 113). Other studies have produced similar results (see, for example Cable and Wilson (1989) and Kruse (1992)).

Of course, there are exceptions to these findings (Weiss, 1987; Pearce et. al., 1985; Conte and Svejnar, 1989) and much of the empirical literature cited is suspect due to methodological weaknesses, most notably, the frequent failure to control for other potential explanatory variables and for feedback relationships. Nonetheless, it is clear that the preponderance of evidence supports the claim that group incentives can and often do contribute to significant increases in labor productivity and firm performance. [1]

To our knowledge, except for the work of Schotter and Weigelt (1992) on long and short term corporate bonuses and Bull, Schotter, and Weigelt (1987) and Schotter and Weigelt (1992) on symmetric and asymmetric tournaments, there has not been any experimental work in economics on the group incentive problem as posed herein. Schotter and Weigelt (1992) demonstrate that the long term corporate bonus plans which they implement experimentally in the lab can not be expected to make the behavior of managers consistent with the preferences of stockholders. The reason is that many real world industrial long term bonus plans do not alter the tradeoffs that managers face when they view the long and short term performance of the firm. In Bull, Schotter, and Weigelt (1987) and Schotter and Weigelt (1992) the authors attempt an investigation of the incentive properties of tournament incentive programs and find that empirically they perform about as expected.

Section 3: A Simple Model of the Group Incentive Problem

To economize on space we will present a set of models which underlie our experiments, using the exact experimental parameterization and functional forms. This will allow us to avoid

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[1]Detailed reviews and evaluations of the empirical evidence can be found in Nalbantian (1987) and Blinder (1990).

presenting the theory twice, once in its general and once in its specific experimental form. Consider a firm composed of six workers indexed i = 1,2,....,6. Each member of the firm can choose an effort level e_i from the closed interval [0,100]. Effort is costly with the cost defined by $C(e_i) = e_i^2/100$. The effort levels of the firm's workers produce output using a simple stochastic linear technology specified as,

$$Y = \sum_{1}^{6} e_i + \epsilon$$

where Y is firm output

e_i is the effort of the i^{th} worker and

ϵ is a random variable defined uniformly over the integers in closed interval [-40,+40].

Assume next that this firm sells its output on a competitive market for a price of 1.5. As a result the firm's revenue is

$$R = 1.5Y = 1.5(\sum_{1}^{6} e_i + \epsilon) .$$

Given this specification, the Pareto optimal effort level for each worker can be defined by solving the following simple maximization problem:

$$\max \ \pi = 1.5\left(\sum_{1}^{6} e_i + \epsilon \right) - \sum_{1}^{6} e_i^2/100$$

Given R is linear in each e_i and each $C(e_i)$ is strictly convex, the first order conditions define a unique profit maximizing effort level as:

$$\partial\pi_i/\partial e_i = 1.5 - 2e_i/100 = 0, \qquad i = 1,2,\ldots,6, \quad \text{or} \quad e_i = 75 \ .$$

The problem for principal-agent theory is how to design an incentive scheme or mechanism which will implement these Pareto-optimal effort levels as Nash equilibria.

In the remainder of this section we will review a set of different incentive mechanisms which can either be derived from principal-agent theory or actually observed in industrial practice. These are applied to the firm outlined above. Some of these mechanisms implement Pareto-optimal effort levels as Nash equilibria while others do not. In general, we lump all incentive schemes into four categories; partnership schemes, target-based schemes, tournament-based schemes, and individualistic monitoring schemes. We will review these types of mechanisms each in turn since they are the mechanisms we eventually test experimentally.

Section 3.2: Partnership Schemes: Revenue Sharing

Egalitarian partnership schemes are an example of the archetypical free-rider shirking incentive schemes comparable to the voluntary contribution mechanisms of public goods theory. As such, they suffer from the same disincentive effects. We illustrate such schemes using a Revenue Sharing mechanism in which the payoff to the i^{th} worker is defined as follows:

$$\pi_i = 1.5\ (\sum_{1}^{6} e_i + \epsilon)/6 - e_i^2/100\ .$$

As we can see, in this scheme all revenue generated by the firm is shared equally and a worker's final payoff is simply his revenue share minus his cost of effort.

To determine the Nash equilibrium effort levels defined by the game associated with this mechanism, we take the partial derivative of each worker's payoff function with respect to his or her own effort level e_i and set it equal to zero. This yields

$$\partial\pi_i/\partial e_i = 1.5/6 - 2e_i/100 = 0 \text{ or } e_i = 12.5.$$

Note that since the marginal benefit from exerting a unit's worth of effort is 1.5/6 and is independent of the effort level, choosing 12.5 is a dominant strategy under Egalitarian Revenue Sharing. Hence this scheme defines a typical free-riding prisoners' dilemma situation in which there is a dominant strategy yielding Pareto-inferior outcomes for all.

3.3: Target Based Schemes

3.3.1: Forcing Contracts

Holmstrom (1982) has made a number of suggestions about finding solutions to the shirking dilemma presented by Revenue Sharing. Among his suggestions is the forcing contract mechanism. Such a mechanism is the generic form representative of a class of target-based mechanisms in which revenue or other outcome targets are set for the firm or a performance group within the firm. If the target revenue is achieved, the workers share in all of the revenue generated while if the firm's revenue falls short of the target, each worker is paid a relatively low penalty wage.

More formally, the payoff to workers under this kind of forcing contract is defined as follows:

$$\pi_i = \begin{cases} 1.5\,(\sum_{1}^{6} e_i + \epsilon)/6 - e_i^2/100 & \text{if } 1.5(\sum e_i + \epsilon) \geq R^* \\ B & \text{otherwise} . \end{cases}$$

If we define $Y^* = R^*/1.5$ then, with a uniform distribution for ϵ distributed over the integers in the interval [-40,+40], the probability of worker i's firm actually meeting the target when he or she chooses the effort level e_i and his co-workers collectively choose $\sum_{j \neq i} e_j$ is:

$$P(e_i, \sum_{j \neq i} e_j) = \begin{cases} 0 & \text{if } \sum_{i=1}^{6} e_i < Y^* \text{ and } Y^* - \sum_{i=1}^{6} e_i > 40 \\ \frac{1}{2} + \frac{(\sum_{i=1}^{6} e_i - Y^*)}{81} & \text{if } \sum_{i=1}^{6} e_i > Y^* \text{ and } \sum_{i=1}^{6} e_i - Y^* \leq 40 \\ \frac{1}{2} - \frac{(Y^* - \sum_{i=1}^{6} e_i)}{81} & \text{if } \sum_{i=1}^{6} e_i < Y^* \text{ and } Y^* - \sum_{i=1}^{6} e_i \leq 40 \\ 1 & \text{if } \sum_{i=1}^{6} e_i - Y^* > 40 . \end{cases}$$

Associated with this probability is i's marginal probability of meeting the target defined as,

$$\frac{\partial P(e_i, \sum_{j \neq i} e_j)}{\partial e_i} = \begin{cases} \frac{1}{81} & \text{if } -40 \leq \sum_{i=1}^{6} e_i - Y^* \leq 40 \\ 0 & \text{otherwise} \end{cases}$$

To find the Nash equilibrium for this mechanism note that for a fixed e_i and $\sum_{j \neq i} e_j$, the expected value of the firm's output, conditional on meeting the target is

$$E(Y | Y > Y^*) = \frac{(e_i + \sum_{j \neq i} e_j + Y^* + 40)}{2},$$

where the constant 40 represents half of the support of the random variable ϵ.

Consequently each worker faces a payoff function of

$$\pi_i(e_i, \sum_{j \neq i} e_j) = B + P(e_i, \sum_{j \neq i} e_j) \left[\frac{\frac{1.5}{6}(e_i + \sum_{j \neq i} e_j + Y^* + 40)}{2} - B \right] - \frac{e_i^2}{100}.$$

For a Nash equilibrium the following first order condition must hold for each i:

$$\frac{\partial \pi_i}{\partial e_i} = -P'(\cdot)B + P'(\cdot) \left[\frac{1.5}{6} \frac{(e_i + \sum_{j \neq i} e_j + 40 + Y^*)}{2} \right] + P(\cdot)(0.125) - \frac{2e_i}{100} = 0, \quad i = 1,2,....6.$$

This condition sets up a relationship, given all of the other parameters, between Y^* and B such that $Y^* = 450$ ($e_i^* = 75$) implies $B = 1.125$. In other words, if one wants to force Pareto Optimal effort levels upon the workers of a firm, one must set a target of $Y^* = 450$ and a penalty wage no higher than 1.125.

For a number of reasons, we will later also want to set an equilibrium output target of $Y^* = 240$ implying an individual target of $e_i^* = 40$. For such a target a penalty wage of at most $B = 5.325$ is required.

3.3.2: Profit Sharing

Profit sharing schemes are nothing more than forcing contract schemes. For our purposes here, however, we will define a profit sharing scheme as a particular forcing contract with $Y^* = 75$ ($e_i^* = 12.5$) and $B = 0$. Note that the target set under profit sharing is the same output that resulted at the equilibrium of our Revenue Sharing scheme. With these parameters, following the analysis in Section 3.3 above, we find the Nash equilibrium is 19.1.

3.3.3: Gainsharing

A Gainsharing scheme is a target-based profit sharing scheme in which the target is generated endogenously by the previous output of workers. Hence, Gainsharing is a forcing contract with a target based on historical or "base period" performance. In our experiments we always perform Gainsharing after an initial 25 round Revenue Sharing experiment taking the average output of the last ten rounds of the Revenue Sharing experiment as the target for the Gainsharing experiment. (Subjects were not told that their performance in the first Revenue Sharing experiment would in any way influence their payoffs in the second experiment they would perform. In fact, they knew no details at all of the second Gainsharing experiment they were to perform until the Revenue Sharing experiment that preceded it was completed.) Note that since we do not know, a priori, the output of our subjects in their Revenue Sharing experiment, we could make no predictions about the output for these Gainsharing experiments or whether they even had equilibria.

3.4: Tournament-Based Schemes: Competitive Teams

In contrast to target based schemes are tournaments which make the payoffs of agents or groups of agents contingent upon relative rather than absolute performance. In our experiments we test a tournament-like mechanism which we call Competitive Teams since it works by dividing the firm into two (or more) teams and having these teams compete for prizes. The team producing the most output gets the big prize while the loser gets the small prize. As a result, our Competitive Teams scheme relies on competition to motivate the workers functioning under it.

To be more precise, let the firm be divided into two teams T_1 and T_2 and let $R_1 = 1.5(Y_1)$ and $R_2 = 1.5(Y_2)$ be the revenues and outputs generated by these teams. Under a Competitive Team mechanism the payoff for any worker i on Team 1 is defined as :

$$\pi_i(Y_1, Y_2, e_i) = \begin{cases} \dfrac{R_1 + TR}{6} - \dfrac{e_i^2}{100} & \text{if } Y_1 > Y_2 \\ \dfrac{R_1 - TR}{6} - \dfrac{e_i^2}{100} & \text{if } Y_1 < Y_2 , \end{cases}$$

where TR is a transfer made from the winning team to the losing team. A similar payoff function can be defined for workers on Team 2.

Note that as formulated above and as implemented in the lab, the Competitive Team scheme is played as a non-cooperative game with each worker choosing its effort level in isolation and without knowledge of the choice made by its team members. In the experiments only the teams revenues were announced to the subjects after each round of the experiment and not any individual member's effort. Also note that we specified the mechanism as defining a transfer made from the winning team to the losing team. Clearly we could have easily specified the winning team as receiving a bonus paid by the firm and not as an intra-firm transfer from one branch to another.

Economically, but perhaps not psychologically, they are equivalent.

Let $E_1 = \sum_{i \in T_1} e_i$ for all workers on Team 1 and $E_2 = \sum_{i \in T_2} e_i$ for all workers on Team 2. Then

for any given E_1 and E_2 we can define the probability that any given team, say team 1, wins the transfer, TR, as[2]:

$$\Pr(\text{Team1wins}) = \begin{cases} 1/2 + \dfrac{(E_1-E_2)}{2(40)} - \dfrac{(E_1-E_2)^2}{8(40)^2}, & \text{if } E_1 > E_2 \text{ and } E_1-E_2 \leq 80 \\ \left[\dfrac{1}{2} - \dfrac{(E_2-E_1)}{2(40)} + \dfrac{(E_2-E_1)^2}{8(40)^2}\right] & \text{if } E_2 \geq E_1 \text{ and } E_2 - E_1 \leq 80 \end{cases}.$$

Once E_1 and E_2 are set, the marginal probability of winning the transfer can be defined for any change in the effort level of worker i on team 1 as:

$$\frac{\partial \Pr(\text{Team 1 wins})}{\partial e_i} = \frac{1}{2(40)} - \frac{(E_1-E_2)}{4 \times (40)^2}$$

where 40 is one half the length of the support of the random variable ϵ.

To demonstrate that the competitive Team mechanism is capable of implementing Pareto-

[2]See Schotter and Weigelt (1994, Forthcoming) for a derivation of this probability. Although the analysis in that paper is for two-person tournaments, all arguments presented there go through for a two team tournament as well.

optimal outcomes as Nash equilibria, assume that all members of each team choose an effort level of 75. Given these choices, the expected revenue for each team is 675 and the probability of each team winning the transfer is 1/2. Now consider one worker, say on team 1, who contemplates a change in his or her effort by a marginal amount. Increasing effort marginally increases the probability of winning by $\partial Pr/\partial e_i$. The benefit of winning is (2xTR)/6 + $(\partial R/\partial e_i)/6$, i.e., the

difference between winning the transfer and losing it, 2TR, and one's share in the marginal revenue generated for the team $(\partial R/\partial e_i)/6$. The marginal cost of changing one's effort is $2e_i/100$. Hence, in order for each worker to be willing to choose an effort level of 75 as a Nash equilibrium we must have

$$\partial Pr(\cdot)/\partial e_i \cdot [(2xTR)/6] + (\partial R/\partial e_i)/6 = 2e_i/100$$

Given our experimental parameters ($\epsilon \in [-40,+40]$, p = 1.5, and $C(e_i) = e_i^2/100$) , we see that if TR is set equal to 360 a Nash equilibrium of 75 results.

3.5: Individualistic Schemes: Monitoring

Our final incentive scheme is not a group incentive scheme at all but rather an individual wage-cum-supervision mechanism (see Calvo and Wellisz (1978) and Calvo (1987)). Under this mechanism our firm offers its workers a wage **W** greater than their opportunity wage w if they put out an effort of e* when on the job. The firm will check the effort level of the worker with a probability of p each period; if the worker is caught working at an effort level lower than e*, he or she will be fired. Again, effort is assumed to be costly for the worker as defined by the cost function

$C(e_i) = e_i^2/100$. In short, the worker is offered an efficiency wage if he or she will put out an effort level of e*.

With correctly set parameters, this scheme will determine Pareto-optimal effort levels on the part of the workers. To demonstrate this result, assume that a worker is contemplating shirking. Obviously if shirking is contemplated then the optimal shirk is $e_i = 0$ since a worker will be fired no matter how severe the shirk is: the worker might as well go all the way. The expected return from shirking is then,

$$E\pi_i(\text{shirking}) = p(w) + (1-p)W - 0^2/100 = p(w) + (1-p)W .$$

The expected return from exerting e* units of effort is,

$$E\pi_i(e_i = e*) = W - (e*^2)/100.$$

Obviously, a worker will shirk or not depending on whether $E\Pi_i(e_i = e^*) \gtreqless E\Pi_i(\text{shirking})$ or

$$p(w) + (1-p)W \geq W - (e*^2)/100$$

or

$$(W-w) \geq [(e_i^2)/100] \times \frac{1}{p}$$

Setting p = 0.70, **W** = 112.5, w = 18.75 and e* = 75, we see that the worker would prefer to work rather than shirk. When $p = 0.30$, the opposite conclusion arises. There is a direct relation between the intensity of supervision and the size of the efficiency wage required to deter shirking.

To summarize our results we present Table 3.1 which also furnishes us predictions for the experiments to be reported on in Sections 4 and 5. Note that for all schemes, the Pareto-Optimal effort level is 75 for each worker which yields and individual payoff of 56.25, a group output of 450, a group revenue of 675, and a group profit (group revenues minus group effort costs) of 337.5.

Table 3.1

Theoretical Predictions**

Experiment	Effort e_{i*}	Payoff π_i	Group Output (Revenue)	Profit (Revenue-Group Effort Cost)
1) Revenue Sharing	12.5	17.18 (112.5)	75	103.1
2) Forcing Contract (75)	75	3.5	450 (675)	337.5
2) Forcing Contract (40)	40	19.41	240 (360)	264
3) Profit Sharing $\begin{Bmatrix} B=0 \\ Y*=112.5 \end{Bmatrix}$	19.1	25.0	114.6 (171.9)	150.01
4) Gain Sharing $\begin{Bmatrix} B=0 \\ Y*=? \end{Bmatrix}$	*	*	*	*
5) Competitive Teams TR=360	75	56.2	450 (675)	337.5
6) Monitoring $\begin{Bmatrix} p=.70\ (.30) \\ W=112.5 \\ w=18.75 \end{Bmatrix}$				
p=0.7	75	56.25	450 (675)	337.5
p=0.3	0	84.32	0 (0)	-506.25

NOTE:

** Note that for all mechanisms there are 6 subjects facing a cost of effort function of the form $c(e_i) = (e_i)^2/100$. Production is of the form $x = \Sigma e_i + \varepsilon$. ε will be distributed uniformly over the interval [-40, +40], and all subjects will choose their effort from the closed interval [0,100]. Finally, when subjects divide team revenue they do so equally so that each worker gets a share of 1/6.

Since we do not know what Y* will be in the Gainsharing experiment until we have some historical data upon which to calculate it, we will not know what the Nash equilibrium will be in the Gainsharing experiment until we actually do it. That fact is indicated by the asterisks in the table.

Section 4: The Experiments and Experimental Design

4.1: The Experiment

To investigate our various group incentive formulae, we ran a set of 13 different experiments using 588 college undergraduates recruited in groups of 12 from undergraduate economics courses at New York University. Students were requested to come to an experimental computer laboratory at the C.V. Starr Center for Applied Economics.[3] They were paid $3.00 for showing up and engaged in an experiment lasting about 1 hour and 20 minutes. Their average final payoffs for this amount of time was about $14.00-$15.00, which seemed more than sufficient to motivate them.

The experiments engaged in were a direct implementation of the incentive plans described in Section 3. For instance, in all experiments (except the monitoring experiments) the 12 subjects recruited were randomly divided into two different groups of 6 subjects who remained anonymously grouped during the entire experiment. After reading the experiment's instructions (and having them read aloud by an experimental administrator, who answered any questions they had), subjects were asked to type a number between 0 and 100 into their computer terminals. Such a number can be interpreted as their unobservable effort levels, although in the instructions only neutral language was used. After these numbers were entered by each subject, the program guiding the experiment added up all of these numbers for each group separately and drew a random number uniformly distributed between -40 and +40 independently for each group. The random numbers for each group were added to the sum of their effort levels. In the instructions subjects were told that the higher the decision number they chose the higher their costs would be and they were given a cost table illustrating the cost of each integer between 0 and 100. (This table was an integer representation of the cost function $e_i^2/100$).

The payoffs for each experiment were then determined according to the incentive plan being used as discussed in Section 3. After each round subjects could see only their own effort levels and the output

[3] The instructions for the experiments are contained in the Appendix of the paper.

levels of their own group. (In the Competitive Team Experiment each team could also see the output of their opposing team after each round). No information about the individual effort levels of other subjects was ever revealed.

When round 1 of the experiment was over, round 2 started and was identical to round 1. Each experiment lasted for 25 rounds which we felt was a sufficient length of time to foster learning if any was to occur. After the first 25 rounds was over, new instructions were handed out for a second experiment. (Subjects were not told about the second experiment before the first). In the second experiment all subjects stayed with their same group . The payoffs at the end of the experiment were simply the sum of the payoffs of the subjects over the 50 rounds of their experience. Payoffs in each round were made in terms of points which were converted into dollars at a rate which was known in advance by all subjects.

4.2: Experimental Design - Phase I and Phase II

Since our original intent in running these experiments was to investigate the incremental impact of group incentive formulae on poorly functioning work units, Phase I of our experimental design involved running each group first in a Revenue Sharing experiment with the expectation that we would observe the low effort equilibrium in which each subject chose an effort of 12.5. Using this as a baseline and a basis for comparison, we then had each group perform a second experiment where one of our four other group incentive plans (Profit Sharing, Gainsharing, Forcing Contracts and Competitive Teams) was used. Hence in Phase I we ran a set of experiments all of which started with 25 rounds of Revenue Sharing followed by 25 rounds of some other group incentive experiment. Because we were also interested in observing how history affected group performance, in Phase II of our experiment we reversed the process and ran other groups of subjects for 25 rounds of some non-Revenue Sharing experiment first followed by 25 rounds of a Revenue sharing experiment. For monitoring experiments in Phase I we ran 25 rounds of a monitoring experiment with a detection probability of .70 followed by 25 rounds of the same experiment but with a monitoring probability of .30. In Phase II experiments the order of these experiments were reversed. Finally, for reasons to be discussed later, we ran two additional sets of Forcing Contract experiments one containing a Pareto-optimal Revenue target of 675 (75 per person) and no uncertainty, i.e., with $\epsilon = 0$, while the other contained uncertainty ($\epsilon = 40$) and a revenue target of 360 (40 per person). This experiment was not preceded or followed by any Revenue Sharing experiment. Our design is presented in Table 4.1 below.

Note that in our experimental design all cost and production parameters are held constant so that in moving from one experiment to another only the incentive **formula** changes. This allows us to attribute all changes in behavior to the formula and not to differing cost or prodcution parameter levels. We compare purely institutional changes.

Table 4.1
Experimental Design

Experiment Number	Rounds 1-25	Rounds 25-50	Number of Groups	Number of Subjects
		PHASE I		
1	Revenue Sharing	Forcing Contract (75)	7	42
2	Revenue Sharing	Competitive Teams	10	60
3	Revenue Sharing	Profit Sharing	6	36
4	Revenue Sharing	Gains Sharing	10	60
5	Monitoring .70	Monitoring .30	2	15
6*	Revenue Sharing	Forcing Contract (75) (Equilibrium 0)	10	60
		Phase 2		
7	Forcing Contracts(75)	Revenue Sharing	10	60
8**	Forcing Contracts(40)		6	36
9	Competitive Teams	Revenue Sharing	10	60
10	Profit Sharing	Revenue Sharing	10	60
11	Monitoring .30	Monitoring .70	2	15
12*	Revenue Sharing	Forcing Contract (75) (Equilibrium 0)	8	48
		OTHER EXPERIMENTS		
13*	Forcing Contract with Certainty, $\epsilon = 0$		6	36

* In this Forcing Contract experiments parameters were set for which the only Nash equilibrium is the zero effort Nash. Since behavior here was not found to be different from the other Forcing Contract 75 experiment, we do not reporting the results ?

** In this experiment no Revenue Sharing experiments followed the Forcing Contract 40 experiments.

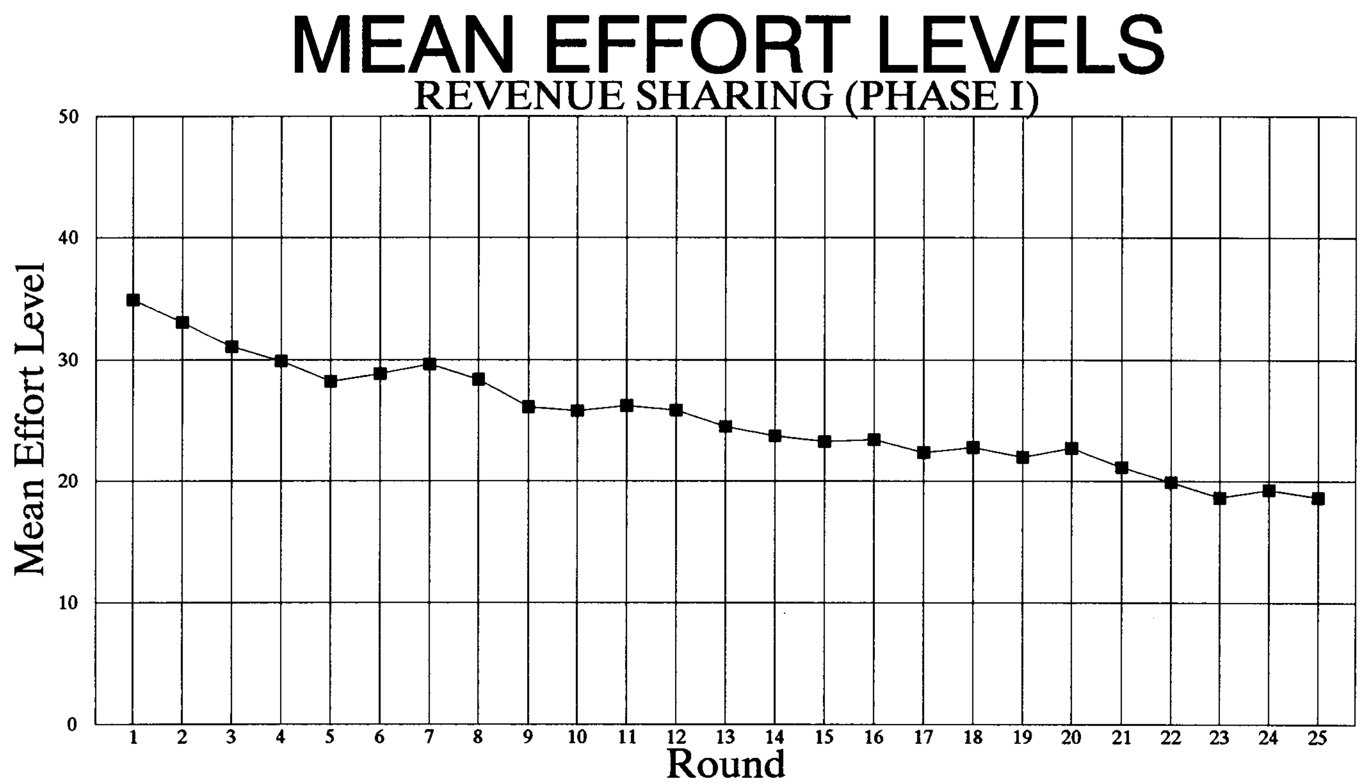


FIGURE 5.1a

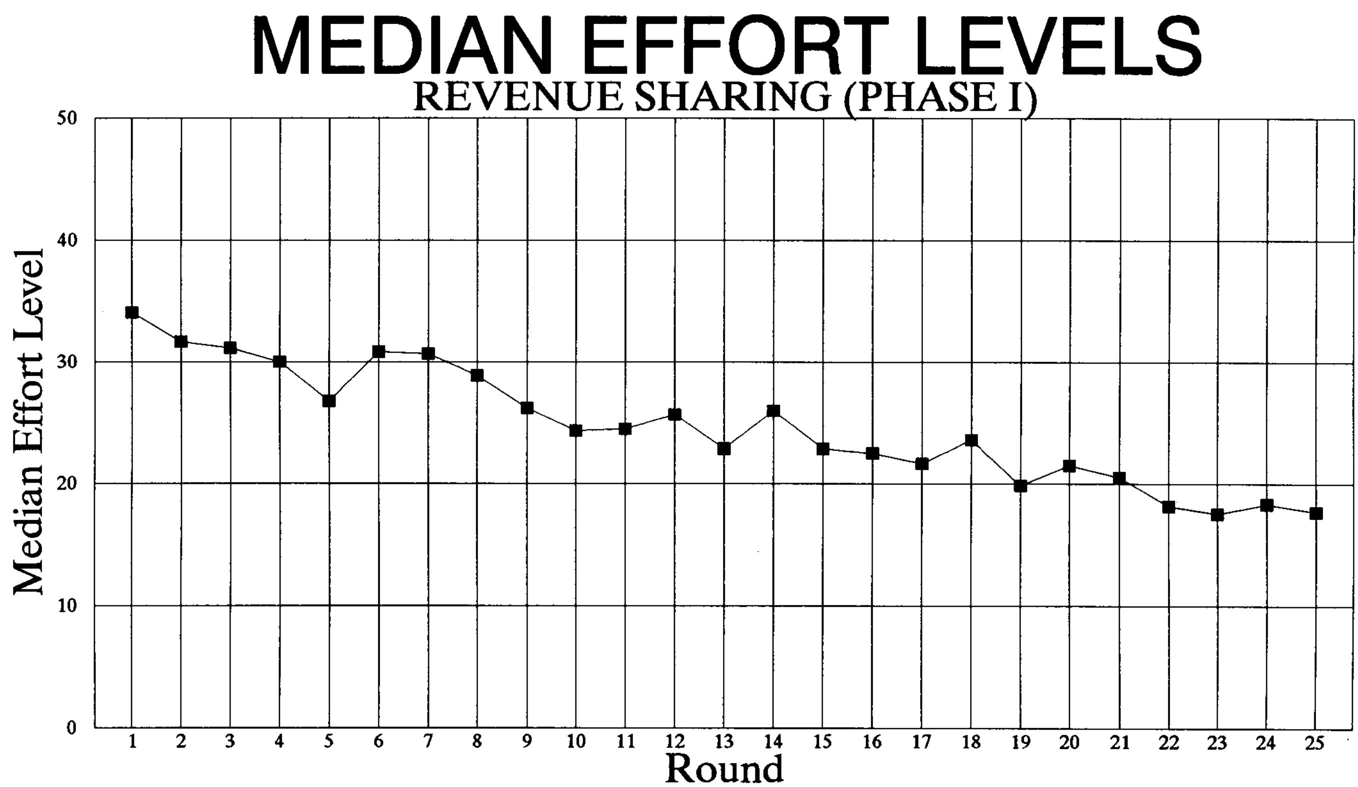


FIGURE 5.1b

Section 5: Results

As stated in Section 1, we will present the results of our experiments by substantiating observations 1-5.

Observation 1: Shirking Happens

When experimental subjects are placed under an incentive plan which provides strong incentives to shirk (i.e., in a Revenue Sharing partnership), their effort levels will approach the shirking equilibrium as they near the end of the experiment.

To illustrate this observation we are mostly interested in the behavior of our subjects in the Revenue Sharing experiments we ran. The reason for this is that we consider these experiments to be classic examples of pure shirking or free-riding experiments presenting subjects with dominant strategies of choosing 12.5 which leads to Pareto-inferior outcomes. These experiments furnish us with a baseline from which we can measure the effectiveness of our other plans; hence it is crucial to establish behavior here first .

To substantiate our first observation consider Figures 5.1a and 5.1b which present the mean and median effort levels of our Revenue Sharing Experiments when they were performed first (Phase I).

FIGURES 5.1a and 5.1b HERE

As we can see, while both the mean and median effort levels of subjects in round 1 start off at 34.86 and 34.08 respectively, by round 25 they converge toward the equilibrium effort level of 12.5. While they remain above this level (18.63 and 17.67 for the mean and median respectively in round 25) there is a clear downward tendency in the data.

We take these results as supporting the hypothesis (as summarized in Observation 1) that subjects will take advantage of shirking opportunities when they function under a plan that at least poses these opportunities in the form of dominant strategies.

Observation 2: History Matters

The performance of a group using any particular group incentive formula depends on the effort norm established by this group in its previous experience with other group incentive schemes.

To illustrate this observation we first compare the mean and median effort levels of groups engaged in our Profit Sharing, Forcing Contract, and Competitive Team experiments in Phases I and II of our experimental design (i.e. before and after subjects have a history with Revenue Sharing schemes). Note that two of these mechanisms (Forcing Contracts and Competitive Teams) implement Pareto-optimal effort levels as Nash equilibria, while the other, Profit Sharing, entails a sub-optimal Nash equilibrium. Hence we will be comparing the effort levels of subjects in Experiments 3 and 10, 1 and 7 and finally 2 and 9 and look to see if these before and after experiences differ. Figures 5.2a - 5.2f illustrate our conclusions here.

FIGURES 5.2a - 5.2f HERE

As we can see, for both the median and the mean, effort levels in practically all rounds were higher in those experiments run before Revenue Sharing. In other words, the experiences of groups in the Revenue Sharing experiment (where effort levels tended to move toward the low-effort Nash equilibrium level of 12.5), tended to lower the effort levels of groups in their second non-Revenue Sharing experiment below what they were when those same schemes were run first in Phase II.

To investigate exactly how history matters we ran a set of ANOVA tests. First a two factor ANOVA test was run in an attempt to explain the mean effort level of groups in the first 5 rounds of the second experiment they participated in as a function of their history (mean effort level) in the last five rounds of their first experiment and the current plan being used (or the previous plan used if the current plan is Revenue Sharing as it would be in a Phase II experiment). Second, a three factor ANOVA test

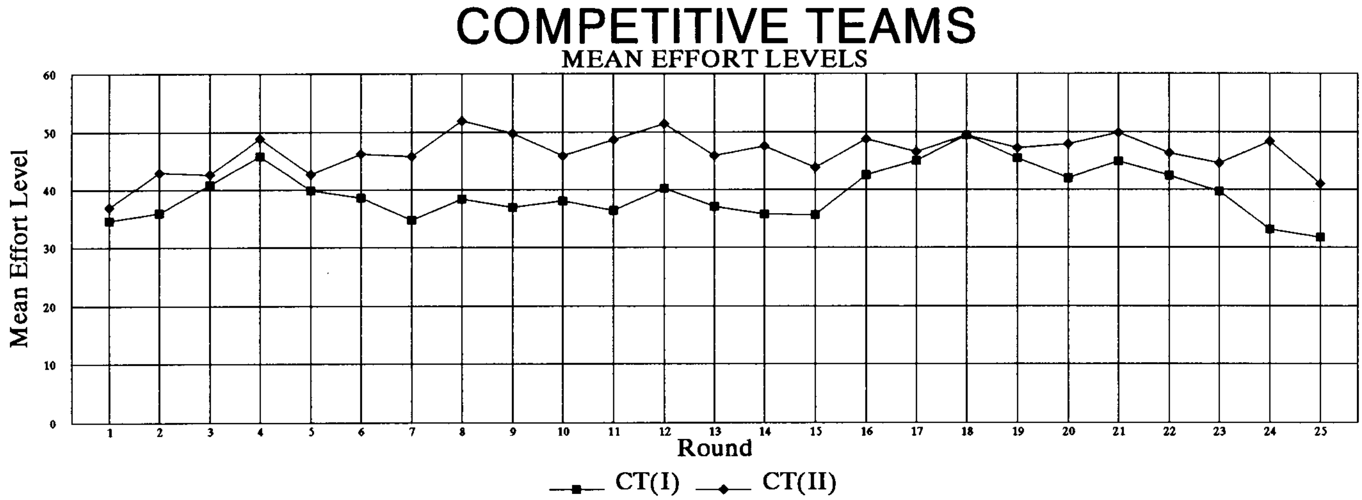


FIGURE 5.2a

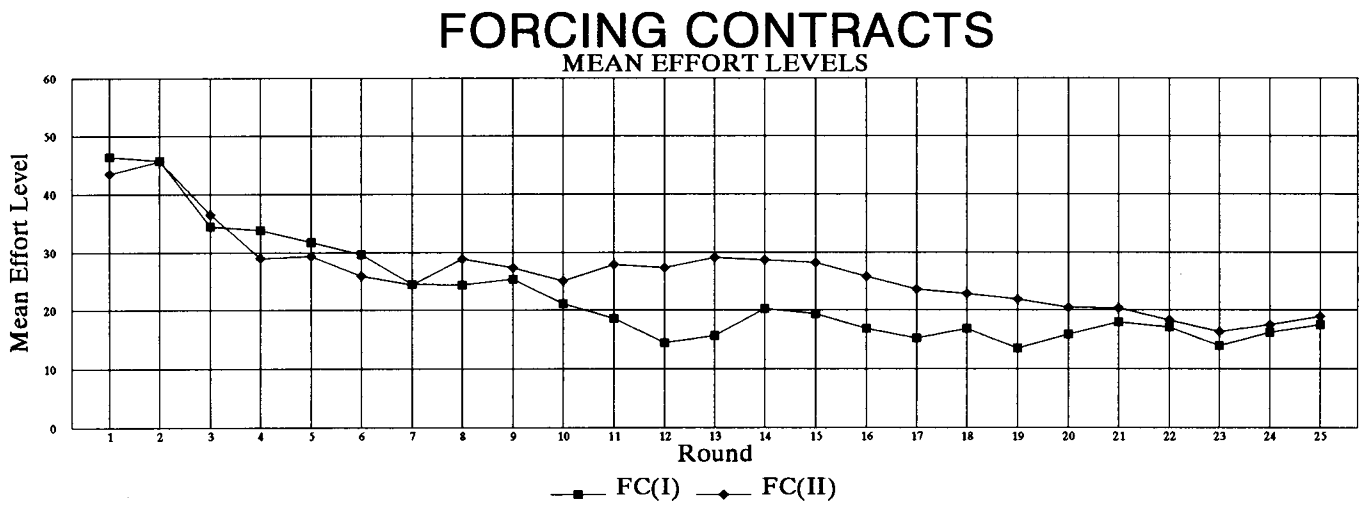


FIGURE 5.2b

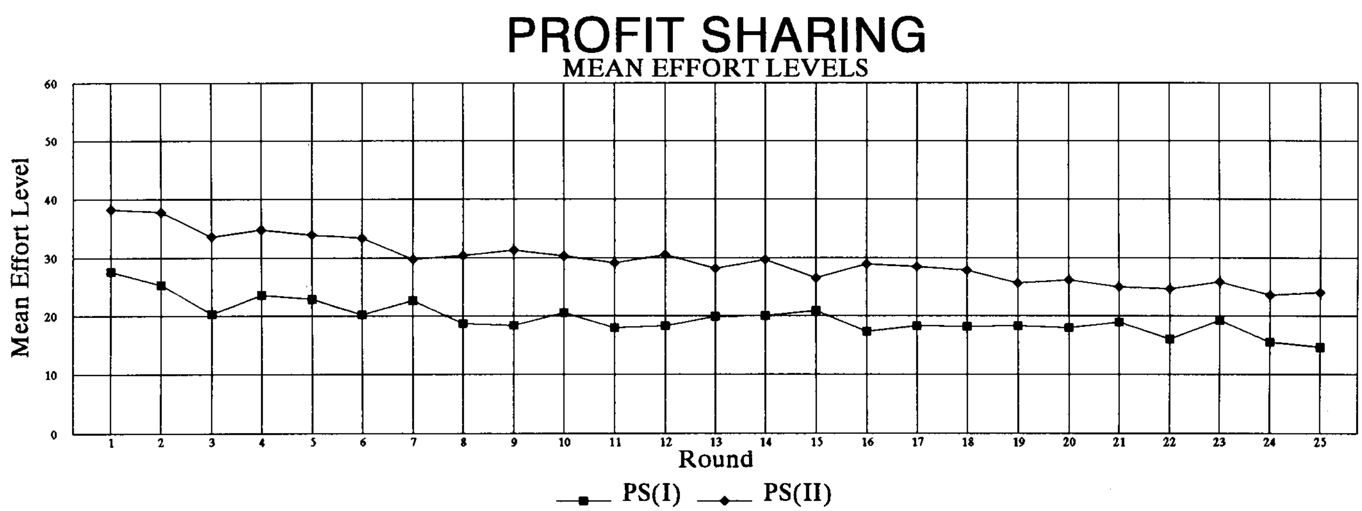


FIGURE 5.2c

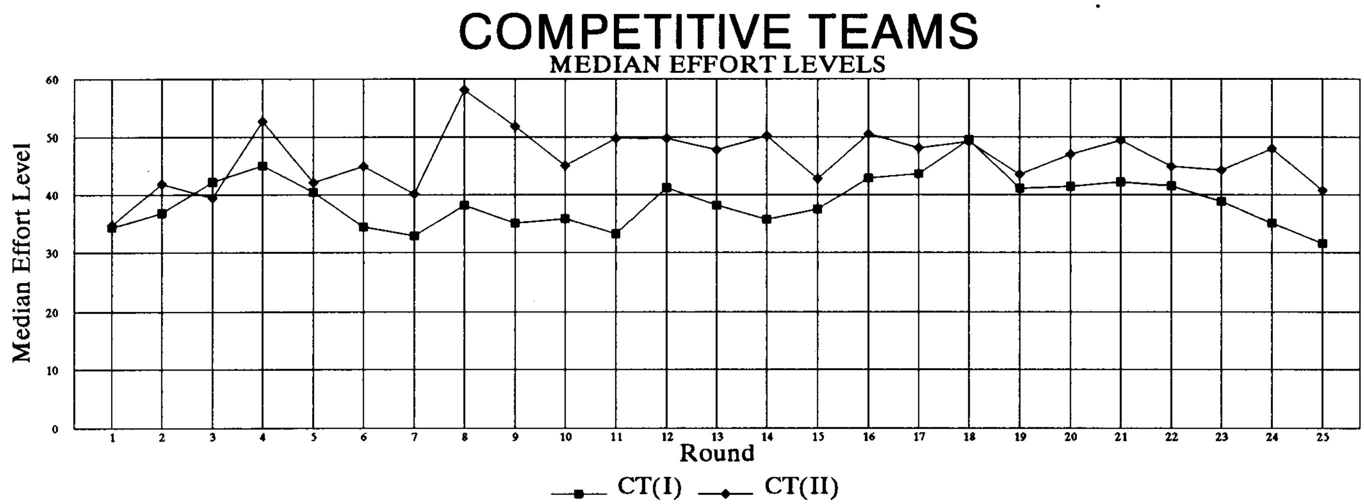


FIGURE 5.2d

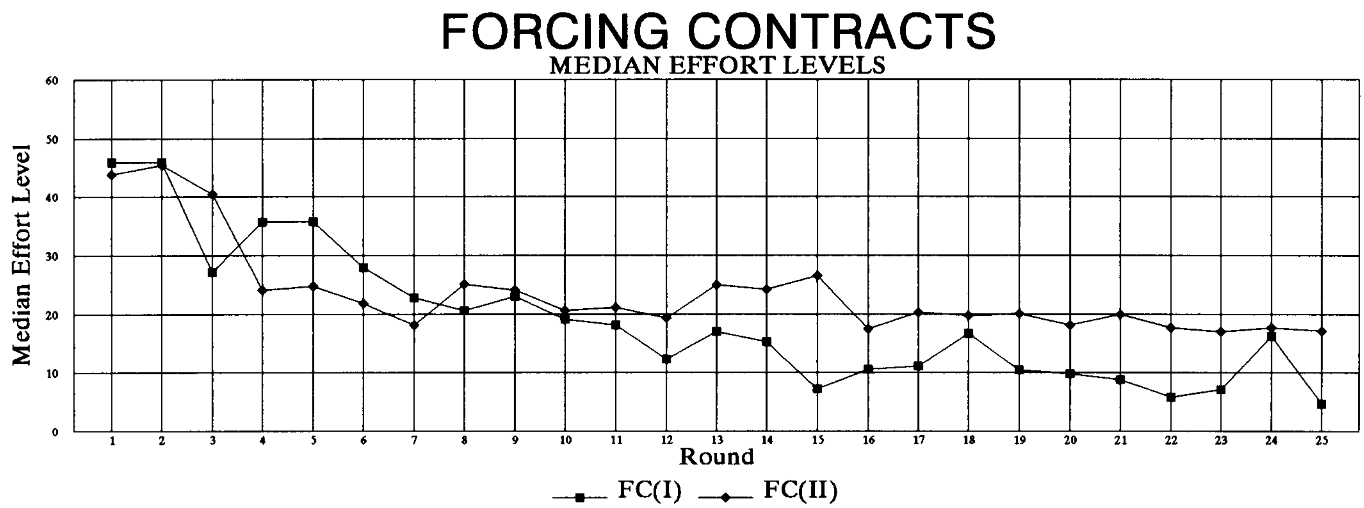


FIGURE 5.2e

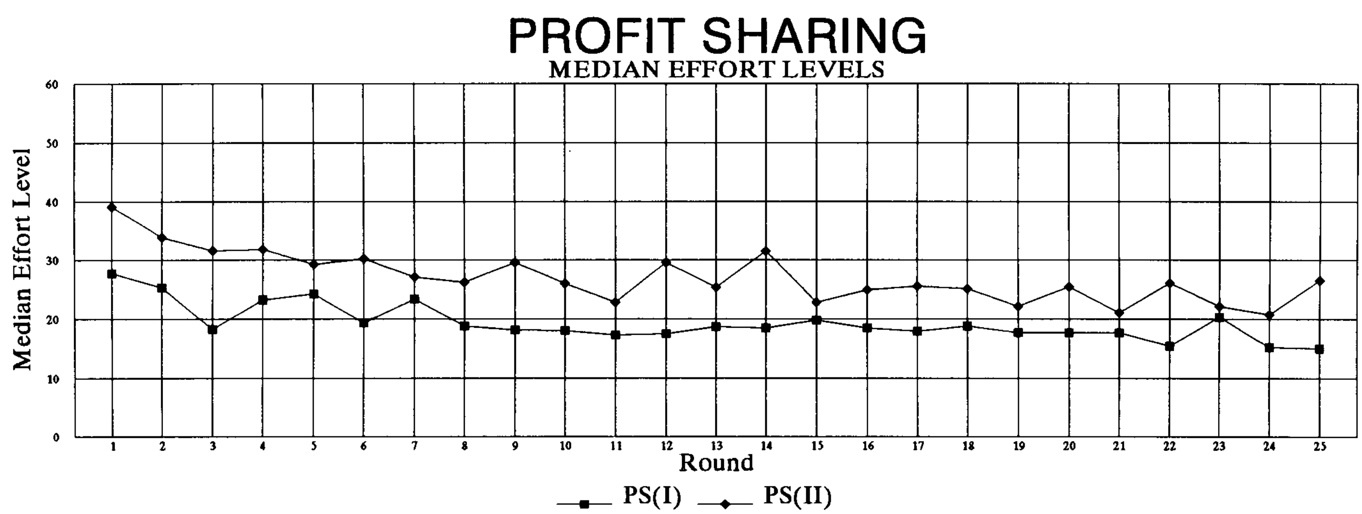


FIGURE 5.2f

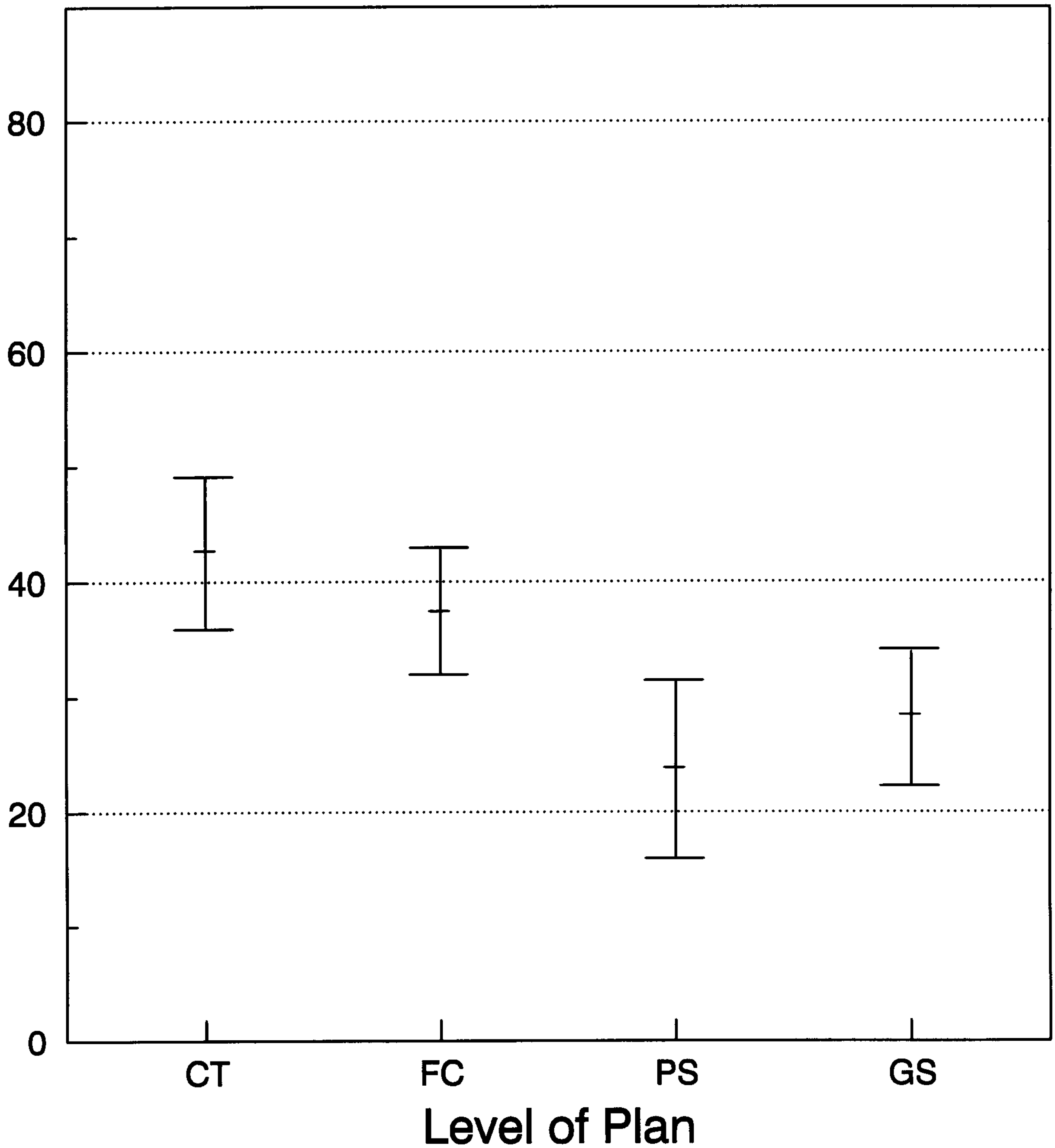
was run in an attempt to explain the mean effort level of groups in the last 5 rounds of the second experiment as a function of the plan used, the mean effort levels of the group during the last five rounds of the previous experiment, and the mean effort levels of the first five rounds of the current one. These results are presented below:

Our first Analysis of Variance test looks at the set of non-Revenue Sharing experiments which were run in Phase I where they were run after participation in a Revenue Sharing experiment. The variable to be explained is the mean effort level of groups in the first five rounds of the non-Revenue Sharing experiment and two factors are used for this explanation: the mean effort levels experienced by these groups in the last five rounds of the Revenue Sharing experiments and the current plan being used. In essence we are trying to separate the influence that experience has (as captured by mean effort levels in the last five rounds in a Revenue Sharing experiment) from the influence of the incentives incorporated in the plan.

Using the notation that PLAN signifies the current non-Revenue Sharing plan used (Competitive teams =1, Forcing Contracts = 2, Profit Sharing = 3 and Gainsharing = 4) in the second experiment and EXPERIENCE signifies the mean of a group at the end of the first Revenue sharing experiment (where EXPERIENCE was discreetized so that a final five period mean between 0 and 10 was given a score of 1, a final five period mean between 10 and 20 a score of 2 etc.), we have the following results for our two-factor ANOVA test:

Figure 5.3a: Two Factor Anova Significance Interval for Mean Effort

First Five Rounds (Factor = Plan)



CT = Competitive Teams

FC = Forcing Contracts

PS = Profit Sharing   GS = Gain Sharing

Figure 5.3b: Two Factor Anova
Significance Interval for Mean Effort

First Five Rounds (Factor = Experience)

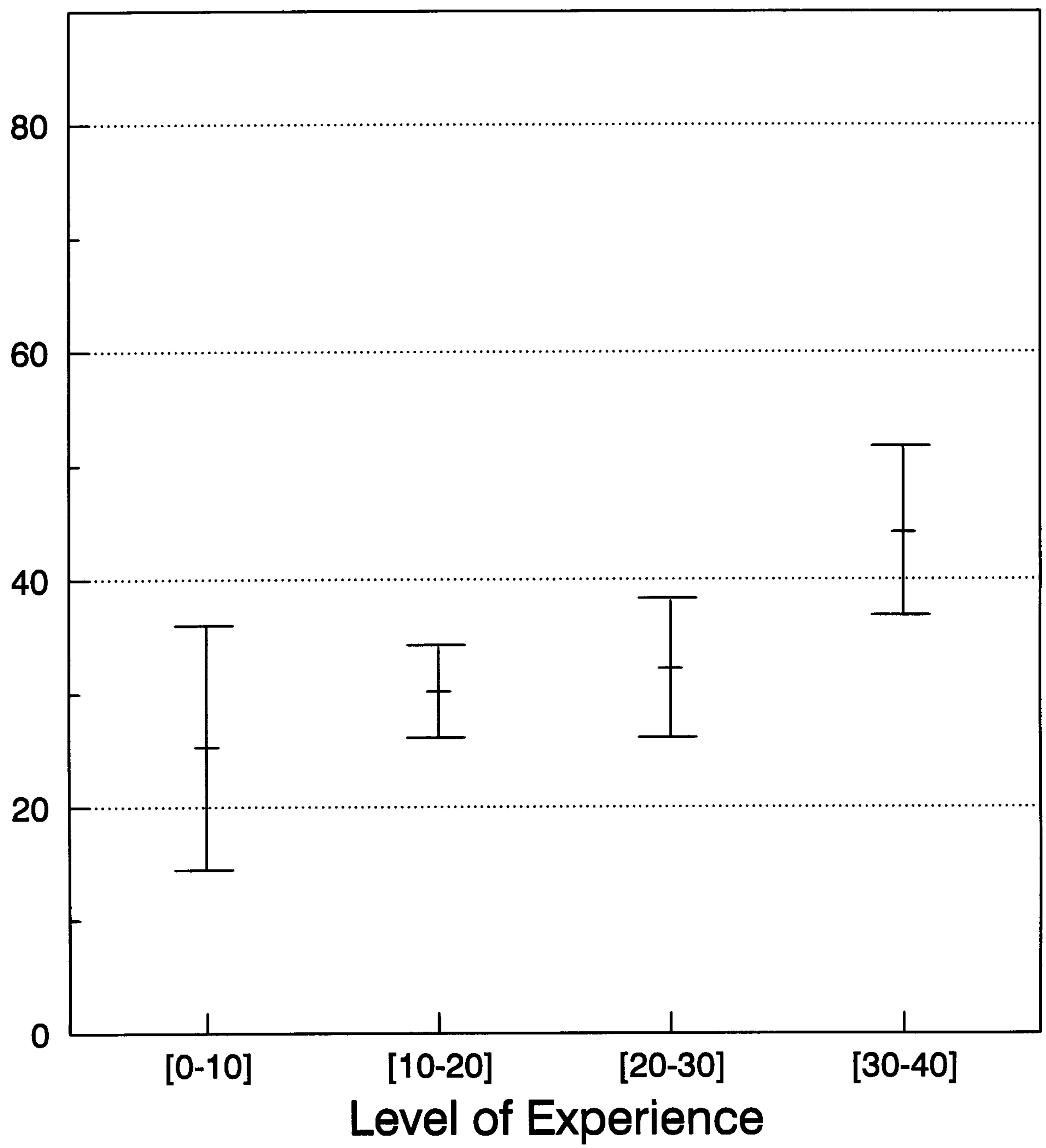


Table 5.1

Two Factor Analysis of Variance

Mean First Five Rounds of Non-Revenue Sharing Experiment

MAIN EFFECT	Sum of Squares	d.f.	M.S	F-ratio	Sig. Level
PLAN	1841.88	3	613.96	8.295	0.0003
EXPERIENCE	1031.51	3	343.83	4.645	0.0079
RESIDUAL	2516.55	34	74.01		
TOTAL (CORRECTED)	5588.94	40			

As Table 5.1 indicates, both factors appear to be significant in explaining the mean effort level of groups in the first five rounds of the second non-Revenue Sharing Experiment. To disaggregate these results even further consider the Figures 5.3a and 5.3b which present the factor means along with their significance intervals.

FIGURES 5.3a and 5.3b HERE

Figure 5.3a indicates that the PLAN factor attains its significance from the fact that the Competitive Team mechanism achieves effort levels during its first five rounds which are significantly different from those of both Profit and Gainsharing while Forcing Contracts elicit effort levels during the first five rounds which are significantly higher than Profit Sharing. (Means which are significantly different have no common intersection between their significance intervals in Figures 5.3 a and 5.3b.) This implies that, independent of their experience in the previous Revenue Sharing experiment, subjects tended to choose higher effort levels during the first five periods of their Competitive Teams experiment than during the first five rounds of any other experiment. The mean effort levels of these plans were ordered as follows: Competitive Teams, Forcing Contracts, Gainsharing, Profit Sharing.

Figure 5.3b demonstrates that when we say experience or history matters what we mean is that only extremely good histories matter. For example, it is only when a group has had a very favorable experience at the end of the first Revenue Sharing experiment (i.e. only when they experienced group mean effort levels of between 30 and 40) that they choose higher effort levels at the beginning of the second experiment, regardless of its description. Any other types of experience (i.e. means between 0 and 10, 10 and 20, 20 and 30) are statistically indistinguishable in their impact on effort levels in the beginning of the second experiment.

When we look at Phase II (where non-Revenue Sharing experiments were run first followed by Revenue Sharing) and conduct the same ANOVA test we find that experience in the last five rounds of non-Revenue Sharing experiments have no significant effect on how subjects start their Revenue Sharing experiment nor does the actual experiment they participated in during that first experiment. These results are presented below in Table 5.2:

Table 5.2

Two Factor Analysis of Variance

Mean First Five Rounds of Revenue-Sharing Experiment

MAIN EFFECT	Sum of Squares	d.f.	M.S.	F-ratio	Sig. Level
PLAN	133.26	2	66.63	0.912	0.4132
EXPERIENCE	229.82	6	38.30	0.524	0.7848
RESIDUAL	2044.89	28	73.03		
TOTAL (CORRECTED)	2327.34	36			

In short, these results indicate that performance of groups in a Revenue Sharing experiment is history independent -- subjects tend to shirk no matter what mechanism they previously participated in and no matter what their experience with that plan was.

While these results above concentrate on behavior during the first five rounds of the second experiment run, we might also be interested in how that same group ended up its experience with each other during the last five rounds of the second experiment. To investigate this we conducted a three-factor ANOVA test in which the mean group effort during the last five rounds of the non-Revenue Sharing experiment was explained on the basis of three factors: the plan or incentive mechanism used (PLAN), the experience of the group (mean effort level) during the last five rounds of the first Revenue Sharing experiment (EXPERIENCE I), and the experience of the group (mean effort level) during the first five rounds of the second experiment (EXPERIENCE II). These results are presented in Table 5.3:

Table 5.3

Three-Factor Analysis of Variance

Mean Last Five Rounds of Non-Revenue Sharing Experiment

MAIN EFFECT	Sum of Squares	d.f.	Mean Square	F-ratio	Sig. Level
PLAN	2732.29	3	910.76	8.41	0.0004
EXPERIENCE I	155.38	3	51.79	0.48	0.6998
EXPERIENCE II	2268.43	5	453.69	4.19	0.0055
RESIDUAL	3139.74	29	108.27		
TOTAL (CORRECTED)	9550.87	40			

Note the when we move to a three factor analysis of variance the variable EXPERIENCE I becomes insignificant. In other words, behavior at the end of the second experiment appears only to be a function of how a group started that experiment and the actual plan under which they are functioning. However, such a conclusion is deceiving since we already know from our two-factor analysis that the variable EXPERIENCE I and EXPERIENCE II are correlated. Hence EXPERIENCE I and EXPERIENCE II are co-linear variables with Experience I having at least an indirect effect on group performance at the end of the second experiment.

A disaggregated look at these results is presented in Figures 5.4a, 5.4b, and 5.4c.

FIGURES 5.4a, 5.4b, and 5.4c HERE

Figure 5.4a presents the conditional effort means for groups in the last five rounds of their second experiment conditional only on the mechanism used during that experiment. Note that Competitive Teams has a last five period mean that is higher than the mean of all other mechanisms. Further, note that while during the first five periods of our two-factor analysis Forcing Contract groups had mean effort levels second only to those of competitive teams, during the last five periods effort levels under this plan have deteriorated and are now the lowest.

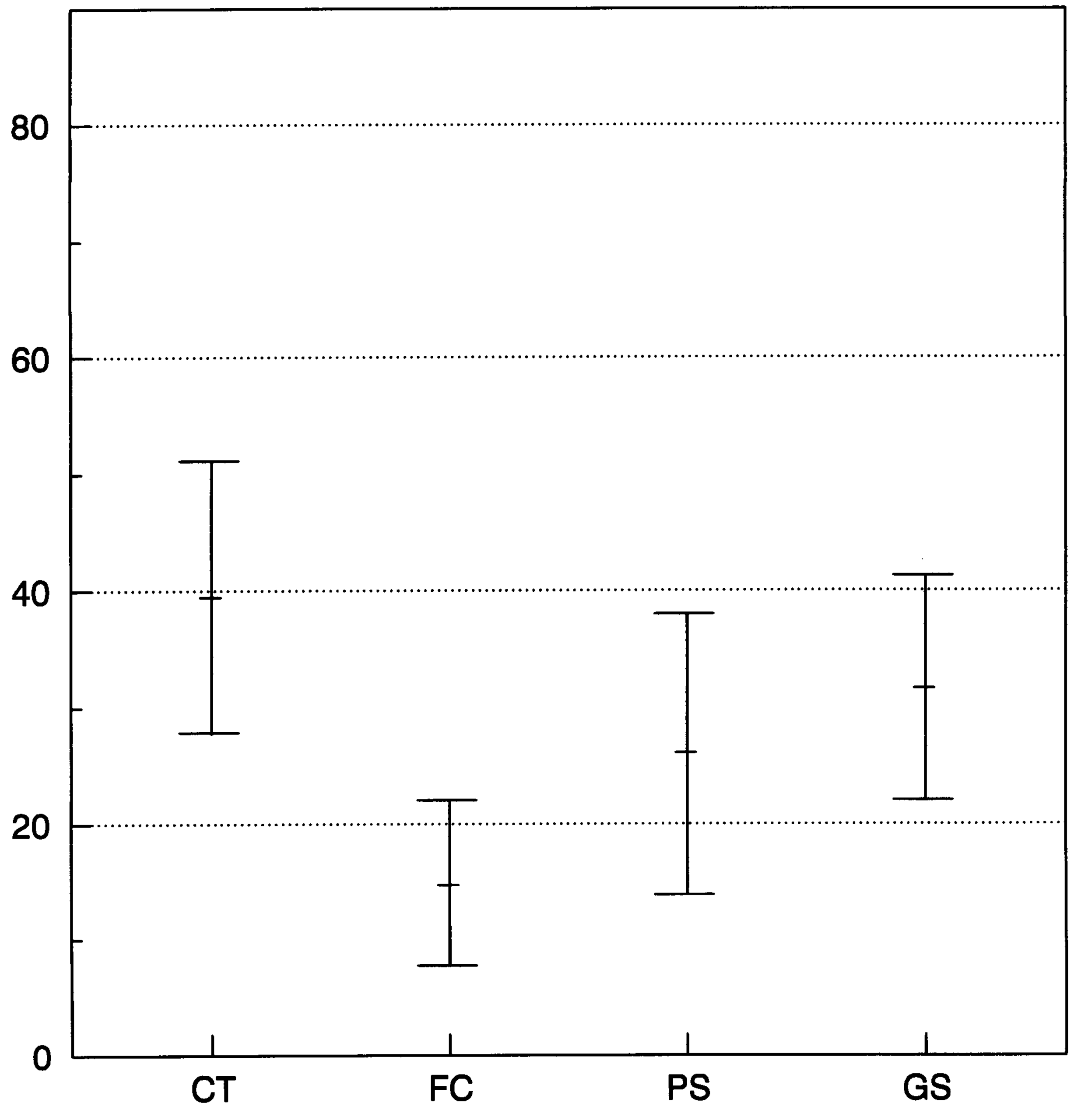
Figure 5.4b presents the conditional effort means for the last five periods of the second experiment conditional only on the effort levels at the end of the first experiment. As we see, effort levels at the end of the first experiment are not significant in determining the mean effort levels at the end of the second experiment. Finally, Figure 5.4c presents the conditional group effort means for the last five periods of the second experiment conditional on the experience of the group during the first five periods of that experiment. Again we see that only extremely high effort levels at the beginning of an experiment have significant effects on effort levels at the end of the second experiment, or at least effects significantly different from other types of experience. As was true with our two-factor analysis, no factors are significant in determining mean effort levels at the end of the second experiment in Phase II where the second experiment is a Revenue Sharing experiment.

Observation 3: A Little Competition goes a Long Long Way

Group incentive mechanisms that create competition between sub-groups in the organization for a fixed set of prizes (i.e. which create internal tournaments) determine higher mean outputs than all target based mechanisms examined and smaller variances in those outputs than many target based mechanisms.

Figure 5.4a: Three Factor Anova Significance Interval for Mean Effort

Last Five Rounds (Factor = Plan)



CT = Competitive
FC = Forcing Contracts
PS = Profit Sharing GS = Gain Sharing

Figure 5.4b: Three Factor Anova Significance Interval for Mean Effort

Last Five Rounds (Factor = Experience I)

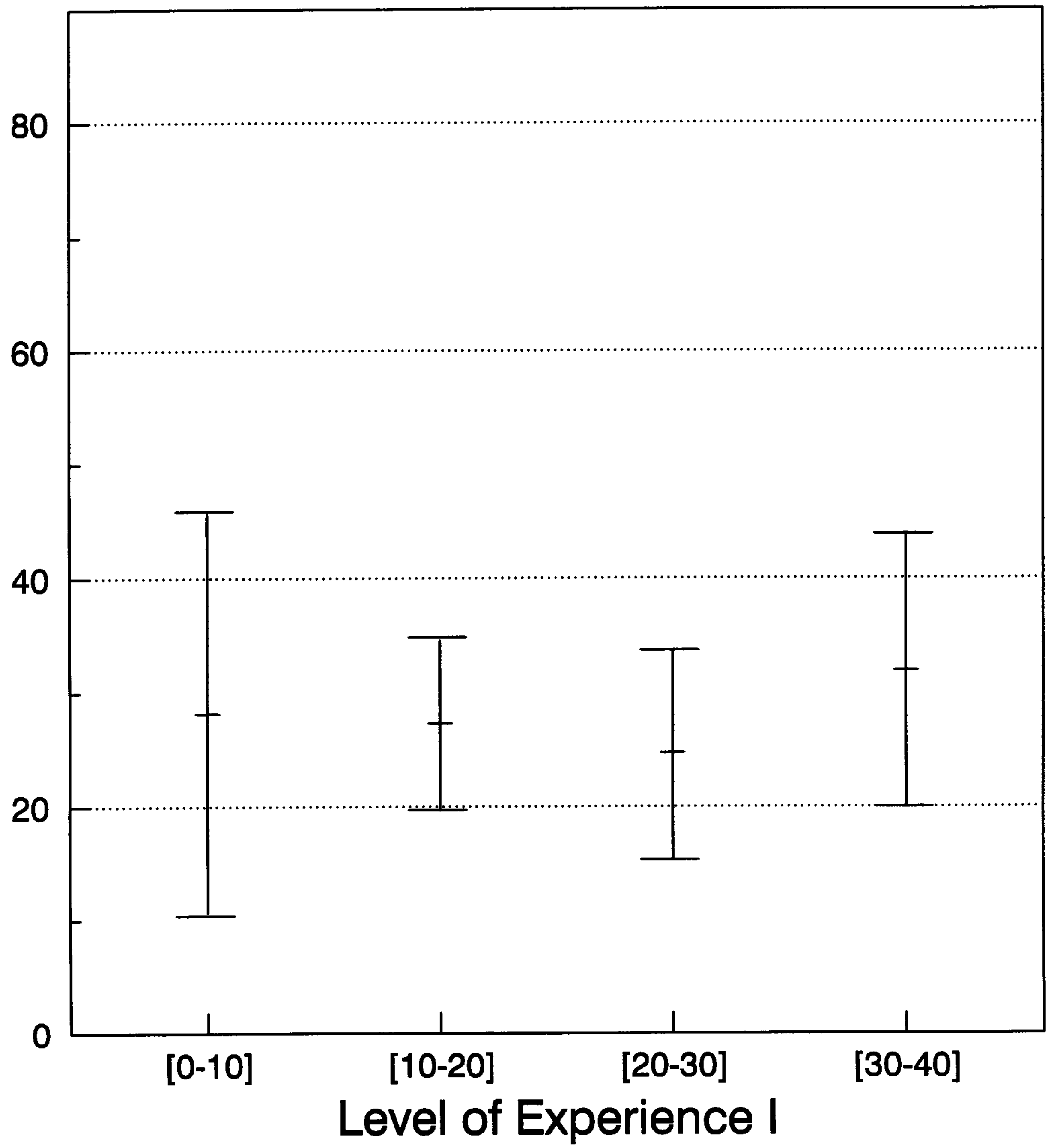
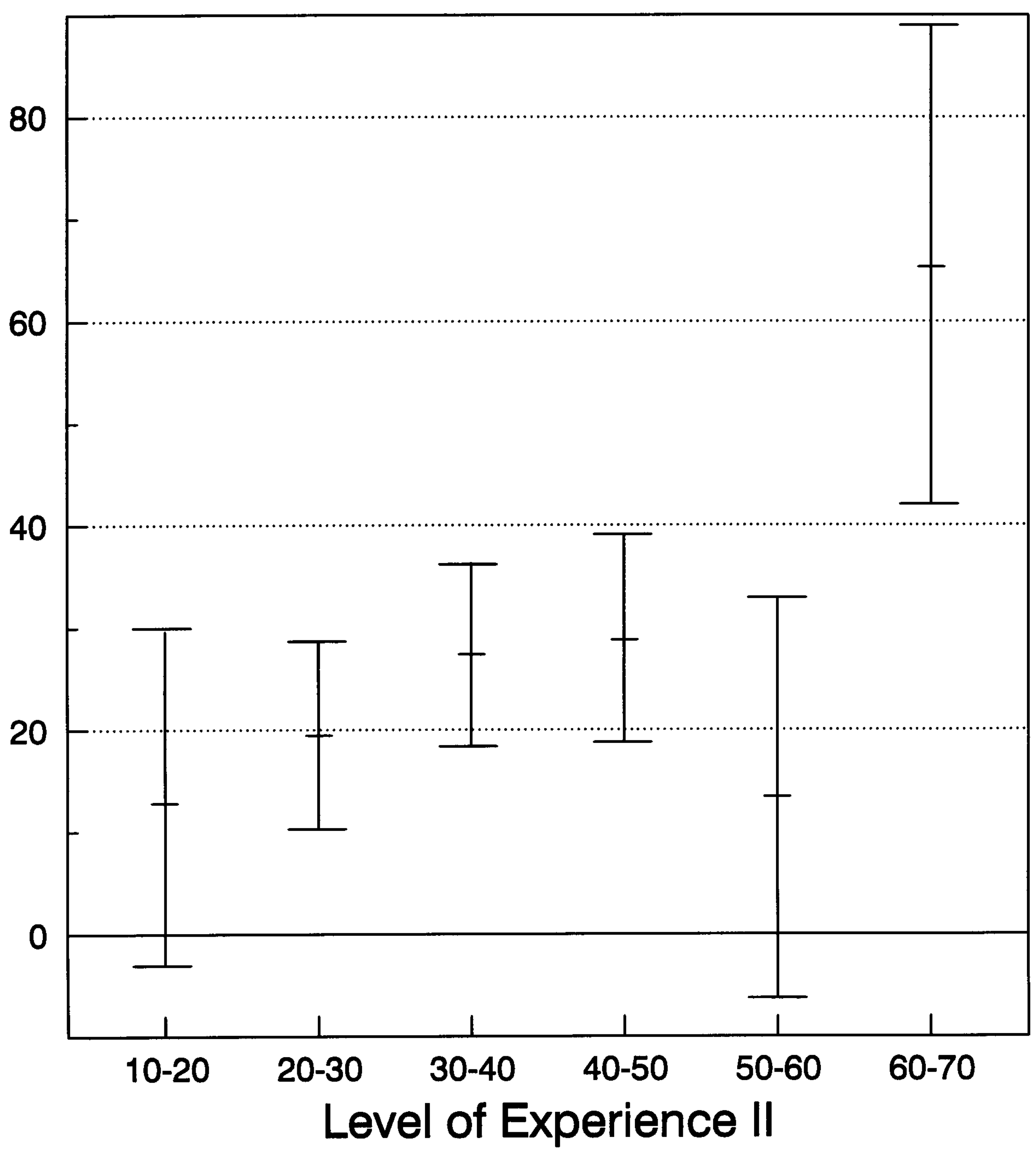


Figure 5.4c: Three Factor Anova Significance Interval for Mean Effort

Last Five Rounds (Factor = Experience II)



There are really two criteria one might want to use to evaluate any particular group incentive mechanism. First one obviously would want to see what mean effort levels this mechanism defines. This is the aim of most principal-agent analyses. However, a corporate manager might also be interested in how reliable any given mechanism is in generating these high mean outputs. For example, say that a corporation has a number of identical plants situated across the country each producing identical products. At its disposal are two group incentive plans, Plan A and Plan B. Say that if Plan A were instituted in each of the corporation's plants, its mean aggregate output would be greater than Plan B's, but Plan B has less plant-to-plant variance attached to it. In other words, Plan B is a reliable producer of reasonably good outcomes while Plan A has a higher mean but also a higher variance. Which plan one uses will obviously depend on one's attitude toward risk. However, if one plan dominates the other in the sense of generating both higher mean outputs as well as smaller group-to-group variations, then clearly it should be chosen.

Observation 4 states that on these criteria Competitive Teams practically dominates all other mechanisms tested except for the fact that Profit Sharing and Revenue Sharing, while having significantly lower means, also have a smaller variances around that mean than does the Competitive Team mechanism. When compared to Forcing Contracts or Gainsharing, however, Competitive Teams elicits both a higher mean effort level during the last five rounds of any experiment and a smaller across-group variance of effort around that mean.

Our support for Observation 4 is presented in Figure 5.5 where we present the efficiency frontier for all of the mechanisms we have investigated in mean-variance space. In other words, we look at the mean group plan for each group during the last five rounds of each Phase I and Phase II experiment and the group-to-group variance across these groups in these same periods. Each point in the mean-variance space represents the mean-variance configuration for a specific plan. All vectors on the boundary of this set are connected. Note also that Competitive Teams dominates all mechanisms except for Profit Sharing and Revenue Sharing. Further, since the mean of the Competitive Team experiment is so much higher than that of either Profit Sharing or Revenue Sharing, it is hard to conceive of a degree of risk aversion that would lead a corporate planner to actually prefer Profit Sharing over Competitive Teams. (Note, of course, that any risk neutral corporate planner would prefer the Competitive Teams mechanism over all others).[4] It is in this sense that we claim that a little competition goes a long way.

FIGURE 5.5 HERE

Observation 4: Vulnerability Matters

The output of groups functioning under incentive plans which implement Pareto-optimal effort levels as Nash equilibria will be greater the less "vulnerable" are those equilibria to deviations or mistakes by group agents.

There has been a great deal of experimental evidence presented recently by Van Huyck et. al (1991),(1990), and Cooper, DeJong, Forsythe, and Ross (1990) that the riskiness of an equilibrium and the out-of-equilibrium payoffs it determines when one's opponents tremble or make mistakes can have a dramatic effect on the likelihood that that equilibrium will be realized in any play of the game. Van Huyck et al. (1992) and Brandts and MacLeod (1991) even present evidence that an outside arbiter, with the power to suggest equilibria, may have a difficult time getting experimental subjects to coordinate on Pareto-dominant equilibria if those equilibria are too risky. In this paper we define a concept called

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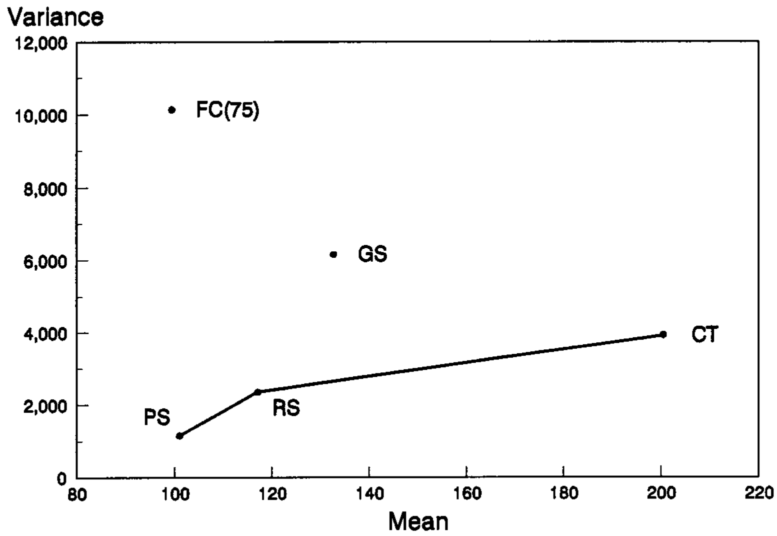
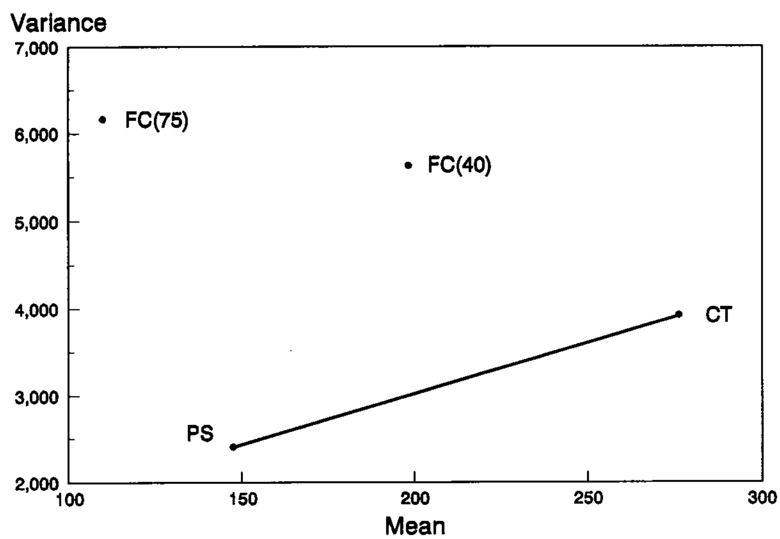
[4]Note that while the Forcing Contract 75 and Competitive Teams formulae have identical Pareto-optimal equilibria, Profit Sharing and Revenue Sharing do not. (Gainsharing has no predictable equilibrium outcome). Hence we should not expect Profit Sharing or Revenue Sharing to outperform these other mechanisms. However, since Forcing Contracts 75 is, in essence, a Profit Sharing mechanism with a Pareto-optimal target, comparing Forcing Contract 75 with Competitive Teams is equivalent to comparing a Competitive Teams mechanism to a Profit Sharing mechanism. Also, Revenue Sharing is added to illustrate the mean-variance properties of a non-target-based scheme (remember, of course, that no Revenue Sharing Pareto-optimal equilibrium exists).



Phase I



Phase II

Figure 5.5: Mean-Variance Plots of Group Output

"vulnerability" which attempts to capture what we consider to be the riskiness of a mechanism's equilibrium. It is our feeling that incentive mechanisms which hold out the prospect of workers being severely hurt financially when they put out Pareto-optimal effort levels while others shirk, are unlikely to elicit such equilibrium effort levels from their workers.

To more precisely define our concept of the vulnerability of an equilibrium consider two group incentive plans denoted plan A and plan B. It should be noted that each such plan defines a game for economic agents to play in which their strategies are their effort levels and their payoffs are defined by the group incentive formula, and depend on their own effort levels and the effort levels of their colleagues. (Recall that in all of the plans we investigate, the payoff to one agent depends on his effort level and the **sum** of the effort levels of his colleagues at work). For the sake of argument say that both group incentive schemes have two equilibria in the games they define, a **low effort** equilibrium (which we will assume involves complete shirking and zero effort levels) and a **high effort** equilibrium. Clearly the workers would like to see the high effort equilibrium chosen but it is our point that the likelihood of it actually materializing depends on its vulnerability. To define vulnerability we ask the following question: If agent i, an individual agent in the firm, were to play according to the high effort equilibrium and put out a high level of effort (with an associated high level of disutility or cost) how fast would his payoff fall if others in his firm decreased their effort levels below that associated with the high effort equilibrium. If agent i's payoff would everywhere fall more steeply under plan A than under plan B, for identical reductions in others' efforts, we say that player i is more vulnerable at the equilibrium of plan A than plan B. More simply, people may be reluctant to choose the high effort equilibrium under plan A if they fear that even a small amount of shirking by their peers will have disastrous consequences for their payoff -- they are vulnerable at the equilibrium. The problems associated with such strategic vulnerability are generally compounded when there is a stochastic element affecting the group revenue function itself.

More formally, let us denote the payoff function to agent i in a group incentive program as $\pi_i(e_i, \Sigma e_{-i})$ where e_i is the effort level of agent i and Σe_{-i} is the sum of effort levels for all agents in the group other than i. Note that all group incentive plans discussed in this paper have symmetric and anonymous payoff functions of this type in the sense that a player's payoff is a function only of his effort level and **the sum** of the efforts of others in one's group. The identity of who puts out what effort is not important. Consider now incentive plans A and B and consider the payoffs $\pi_i^A(e^*_i, \Sigma e^*_{-i})$ and $\pi_i^B(e^*_i, \Sigma e^*_{-i})$ that each agent gets at the Pareto optimal equilibrium of each plan, which we will assume exists (this is true of the Forcing Contracts experiment and the Competitive Teams experiment) and are equal for each agent. Looking at $\pi_i^A(e^*_i, \Sigma e_{-i})$ and $\pi_i^B(e^*_i, \Sigma e_{-i})$ as a function of Σe_{-i}, (holding e_i at its Pareto optimal Nash equilibrium level e^*_i), we say that player i is more vulnerable at the Pareto Optimal Nash equilibrium of plan A than plan B if $\pi_i^A(e^*_i, \Sigma e_{-i})$ is everywhere below $\pi_i^B(e^*_i, \Sigma e_{-i})$ over the domain of the function from Σe^*_{-i} to 0.

The above definition of vulnerability is absolute in the sense that it does not judge the vulnerability of the equilibrium choice of a mechanism as being relative to any other choice that the agent could make. We might want to take these other choices into account, however. For example, say that a mechanism has two equilibria, one being a "good" Pareto optimal equilibrium and one being a "bad" shirking equilibrium or even, perhaps, a secure mini-max payoff. Let the low Nash or secure payoff be $\pi^i(\text{low})$ and the payoff at the good equilibrium be $\pi^i(\text{high})$. Returning to our functions $\pi_i^A(e^*_i, \Sigma e_{-i})$ and $\pi_i^B(e^*_i, \Sigma e_{-i})$ find, for each function, that Σe_{-i} which equates $\pi i(e^*_i, \Sigma e_{-i})$ with $\pi_i(\text{low})$. $\Sigma e^*_{-i} - \Sigma e_{-i}$ would then measure the amount that others in a group could fall below their Pareto optimal equilibrium effort before player i would be better off at his or her secure or low-Nash payoff level. Defining D^{*A} and D^{*B} to be these deviations, we say that player i is more vulnerable at the Pareto Optimal Nash equilibrium of plan or mechanism A than mechanism B if $D^{*A} < D^{*B}$. In other words, under plan A player i is more vulnerable than under plan B if smaller deviations away from the Pareto optimal level by other agents would yield a payoff equal to the secure or low-Nash payoff of the mechanism. In short, we would expect that agents using plan B would be more likely to choose the good equilibrium since they are better off at it than they are at the low equilibrium for a larger set of deviations by their group members. Note, however, that this definition does not take the levels of these payoffs into account, so one might want to normalize these payoffs in some manner.[5]

To illustrate these concepts, consider two of the mechanisms we are considering here namely the Forcing Contract(75) scheme with a Pareto optimal target of 450 (75 for each player at the symmetric equilibrium), and a Competitive Teams scheme with a Nash equilibrium of 75. Note that in the Forcing Contract(75) scheme we also have an equilibrium where each subject chooses zero while such an outcome is a minimax strategy for each player in the Competitive Teams scheme. Figure 5.6 illustrates the payoff consequences for any player i in these two schemes if he or she were to keep his or her effort level at the Pareto-optimal level while teammates deviated. On the horizontal axis we have x, the fraction of the Pareto-Optimal effort chosen by the 6 members of worker i's team. On the vertical axis, we have the fraction of the Pareto Optimal payoff that i will receive given any x chosen by his teammates and assuming that i will be choosing e^*_i. Using fractions normalizes payoffs across schemes and allows for meaningful comparisons.

FIGURE 5.6 HERE

Note that according to both our definitions the Pareto optimal equilibrium defined by the

[5] Note that this concept of vulnerability is different in spirit than such concepts as risk dominance (see Harsanyi and Selten (1988)) since the focus of attention there is which equilibrium, within a game with multiple equilibria, will be selected while here we are comparing the likelihood of an identical (Pareto-Optimal) equilibrium being chosen across two different games.

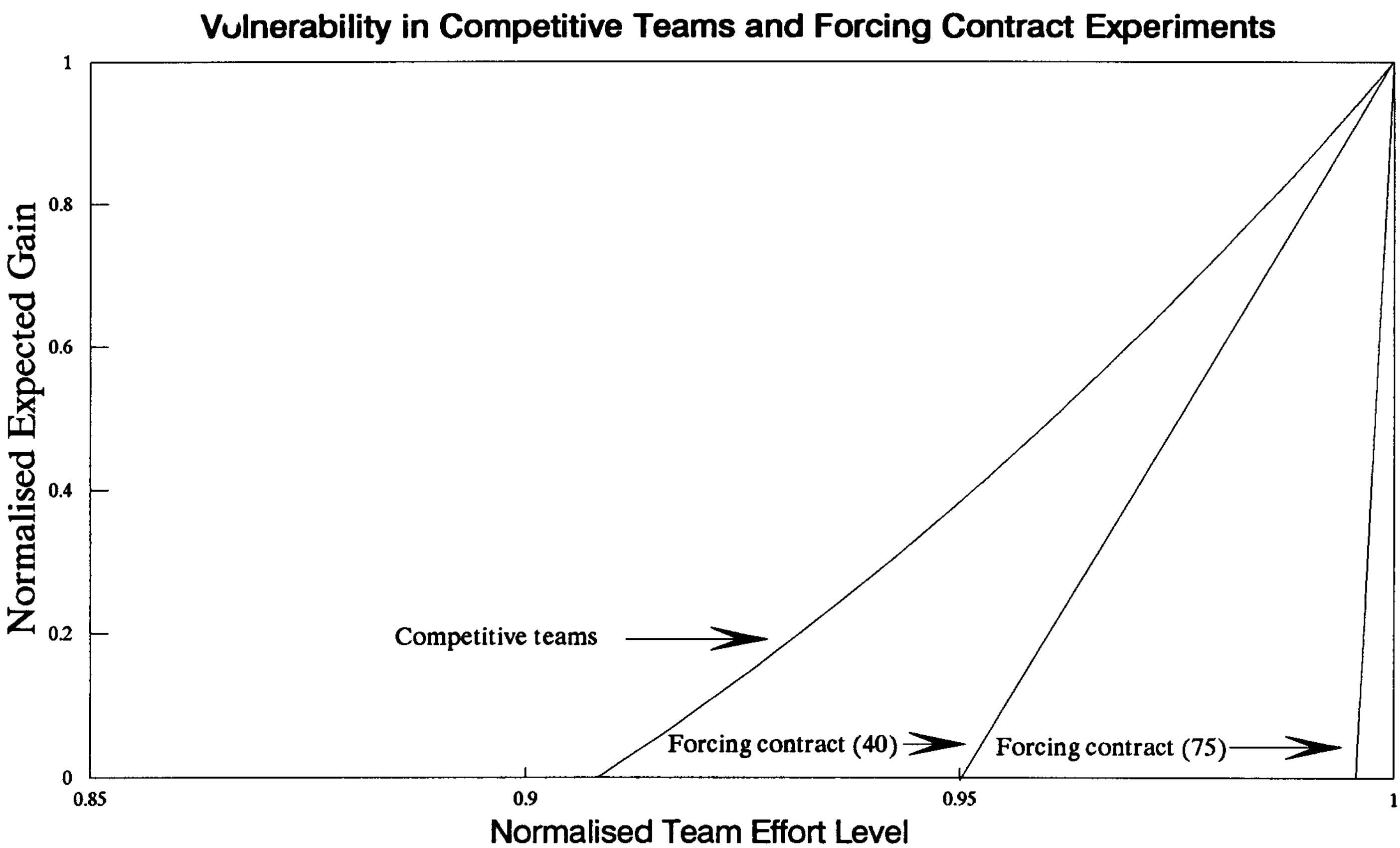

Vulnerability in Competitive Teams and Forcing Contract Experiments
Normalised Expected Gain
1
0.8
0.6
0.4
0.2
0
0.85
0.9
0.95
1
Competitive teams
Forcing contract (40)
Forcing contract (75)
Normalised Team Effort Level

FIGURE 5.6

Competitive Teams mechanism is far less vulnerable than the same equilibrium under the Forcing Contract(75) mechanism. First for deviations of others from the Pareto-optimal level, the Competitive Team mechanism defines payoffs which are everywhere above the Forcing Contract(75) scheme. Further, if we define D^{*FC} and D^{*CT} as the critical deviations by one's teammates which make player i indifferent between remaining at the Pareto optimal effort level by himself or herself, while the others deviate by D^{*FC} and D^{*CT} (respectively), and having all subjects choose zero effort, then we can see that since $D^{*CT} \leq D^{*FC}$, the Competitive Teams mechanism is also less vulnerable under our second definition.

In fact, using this definition we see that the Forcing Contract(75) Scheme is extremely vulnerable since a deviation by one's teammates of only 2 effort units (0.53%) leads to a payoff which is less than the zero equilibrium payoff. For the Competitive Teams, a collective deviation of 42 (11.2%), is needed to reach the same indifference point. Finally, to demonstrate that vulnerability is in fact important in determining subjects' effort level choices, we devised the Forcing Contract (40) scheme in which the target (equilibrium level) output is set at 240 and the penalty wage is set at 5.10. In this scheme the critical deviation level is 24 (12%) so that this Forcing Contract (40) scheme is far less vulnerable than the Forcing Contract (75) scheme. (This scheme does not, of course, implement a Pareto optimal outcome as a Nash equilibrium.

To see that vulnerability matters, consider Figure 5.7a where we plot the mean effort levels for all groups performing under the Forcing Contract (75), Competitive Teams, and Forcing Contract (40) mechanisms. Note that in all rounds except the first three the mean effort levels of groups under these mechanisms mimic their relative vulnerabilities, i.e. Competitive Teams is the least vulnerable and most productive, followed by Forcing Contracts (40) and finally by Forcing Contract (75).

FIGURE 5.7a HERE

The comparison presented in Figure 5.7a could be criticized because the relative incentives provided by these three mechanisms to actually choose the equilibrium Pareto-Optimal effort levels differ

FORCING CONTRACTS AND COMPETITIVE TEAMS

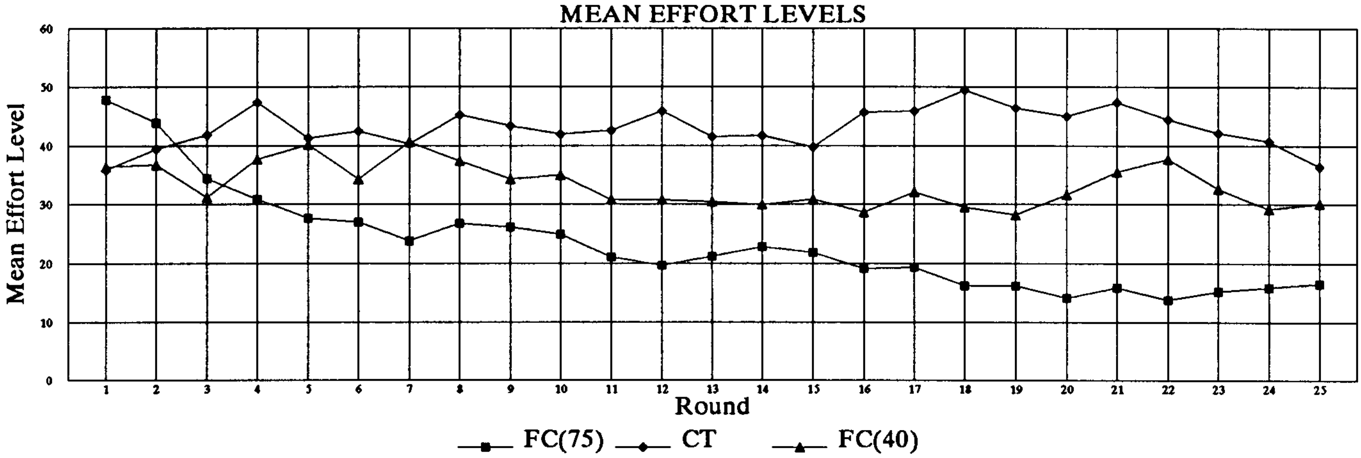


FIGURE 5.7a

COMPETITIVE TEAMS & FORCING CONTRACTS WITH CERTAINTY

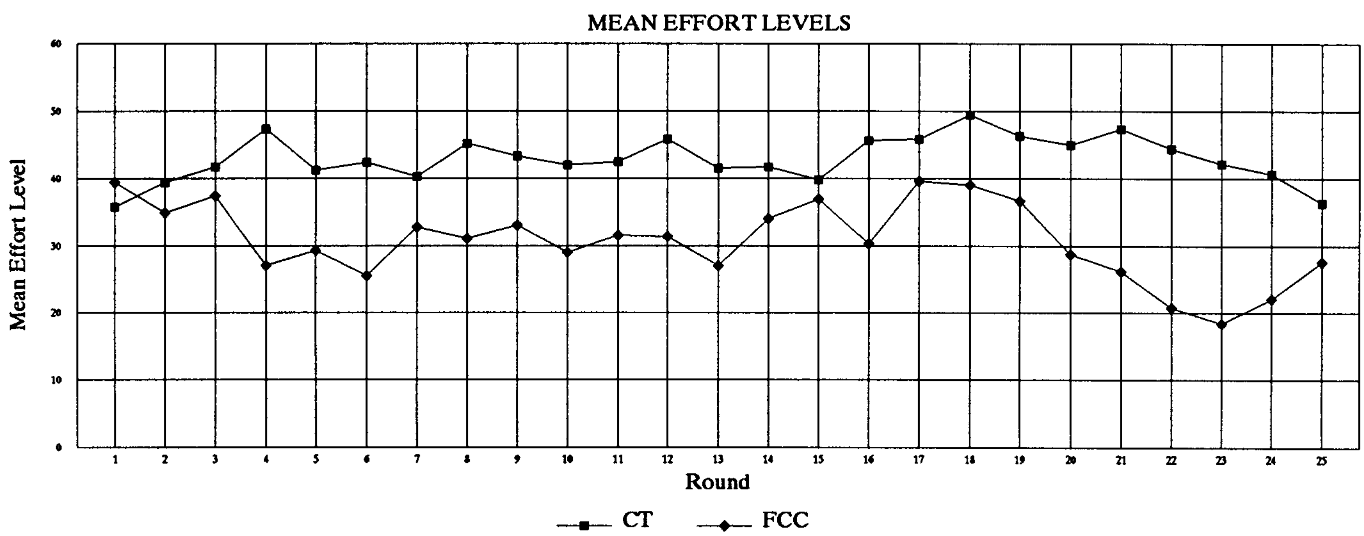


FIGURE 5.7b

greatly. For example, in the Competitive Team experiments, while the payoff to players at the Pareto-optimal equilibrium is 56.25, it is zero at the equilibrium where all shirk and choose zero effort levels. Hence, it would appear that there is a great incentive to actually choose the optimal equilibrium effort level. In addition, as we have seen, choosing a Pareto-optimal equilibrium is even less risky than doing so under either the Forcing Contract (75) or Forcing Contract (40) scheme. For the forcing contracts, however, while the payoff to the zero equilibrium is again zero, the expected payoff to subjects from choosing the Pareto-Optimal equilibrium is only 19.41 in the Forcing Contract 40 case and 3.5 in the Forcing Contract 75 case. (This is true because at the equilibrium in both of these cases, due to the exogenous stochastic uncertainty, there is only a 50% chance that the target will be surpassed and hence there is a 50% chance that despite their effort a subjects will be paid only their penalty wage). Because of these small equilibrium payoffs, one might claim that it is not vulnerability which explains the poor performance of Forcing Contracts but rather the small incentive that they provide to actually work hard rather than shirk.

To control for this problem, we ran our Experiment 13 in which the Forcing Contract 75 experiment was implemented except that we removed the exogenous stochastic uncertainty by setting $\epsilon = 0$. In such a case, if a subject group exactly reach their target (as the Nash equilibrium requires), the probability that they receive the high payoff is 1 rather than 1/2. Consequently, the payoff at the equilibrium here is the same 56.25 as it is in the Competitive Teams case and any lack of performance here should be attributable to the vulnerability of the mechanism exclusively. Such vulnerability here is extreme since any slight deviation from the equilibrium will cause the entire group to receive the penalty wage.

Figure 5.7b presents the results of this experiment along with those of the Competitive Team experiment. As we can see, despite identical incentives to increase effort levels from the zero effort to the Pareto-optimal effort equilibrium, the Competitive Team mechanism generates higher effort levels throughout the length of the experiment. In fact, the Forcing Contract with Certainty mechanism does not perform significantly different from the Forcing Contract 75 experiment except in periods 16-20 where effort levels rise and then fall. The results in these periods were driven by a few individuals in several experiments who tried to raise group effort levels by raising their own obviously hoping that others would follow. Such attempts always failed as we see lower effort levels after period 20.

Perhaps another way to see how vulnerability affects behavior is to see how much of a chance subjects are willing to take in their effort choices. In other words, let us not look at the mean effort level but at the percentage of people choosing effort levels at least two-thirds of the way from zero effort to the target (or two-thirds of the way toward the Pareto-optimal effort level). Presumably, the more vulnerable subjects will be at the Pareto-optimum or at the target of the scheme, the less willing they should be to choose such effort levels or even approach them. Figures 5.8a, 5.8b, and 5.8c 5.8d, presents the results of these calculations.

FIGURES 5.8a, 5.8b, and 5.8c, 5.8d HERE

Figures 5.8a, 5.8b, 5.8c, and 5.8d present the histograms of individual choices in the Forcing Contracts (75), Forcing Contracts (40), Forcing Contracts With Certainty, and Competitive Teams experiments. These histograms are drawn using data from the last five rounds of the Phase II experiments where these mechanisms are run without any prior experience with Revenue Sharing. Note the dramatic difference between the number of subjects risking high effort choices in the Forcing Contracts(75) experiments (with a target of 75) as compared to the Competitive Teams experiment. Such a difference we attribute to the varying vulnerabilities of these plans. While the Forcing Contracts (40) scheme looks relatively good under this measure, note that we require subjects to choose only an effort of 26.33 as opposed to the choice of 50 for schemes implementing Pareto-optimal outcomes.

FORCING CONTRACTS, PHASE II
INDIVIDUAL EFFORT LEVELS, LAST 5 ROUNDS

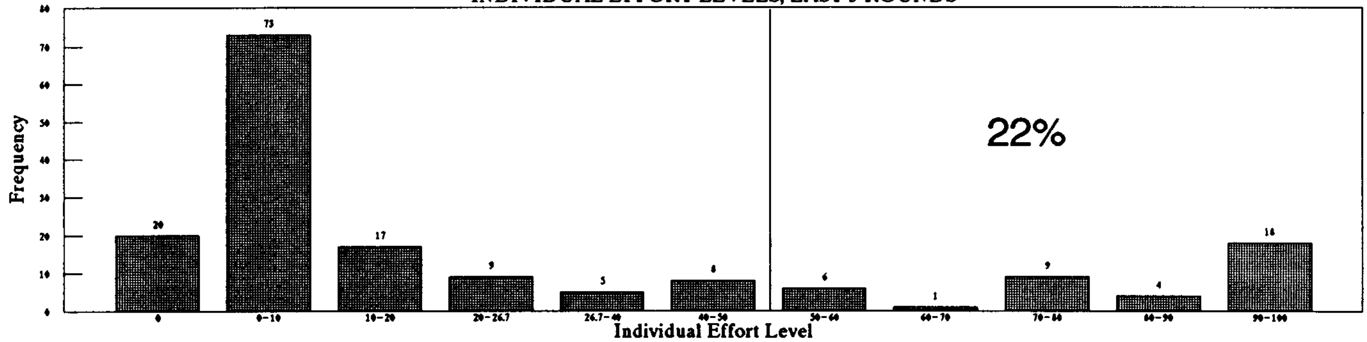


FIGURE 5.8a

FORCING CONTRACTS (40), PHASE II
INDIVIDUAL EFFORT LEVELS, LAST 5 ROUNDS

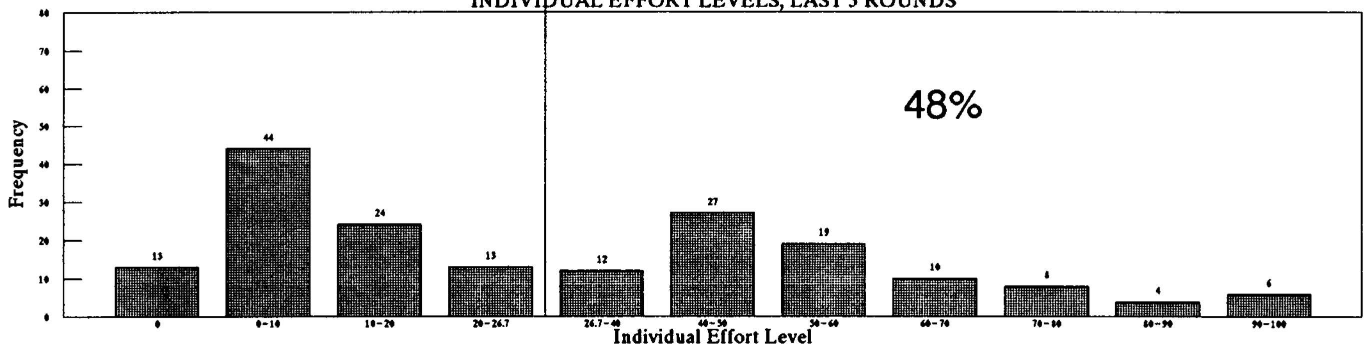


FIGURE 5.8b

COMPETITIVE TEAMS, PHASE II
INDIVIDUAL EFFORT LEVELS, LAST 5 ROUNDS

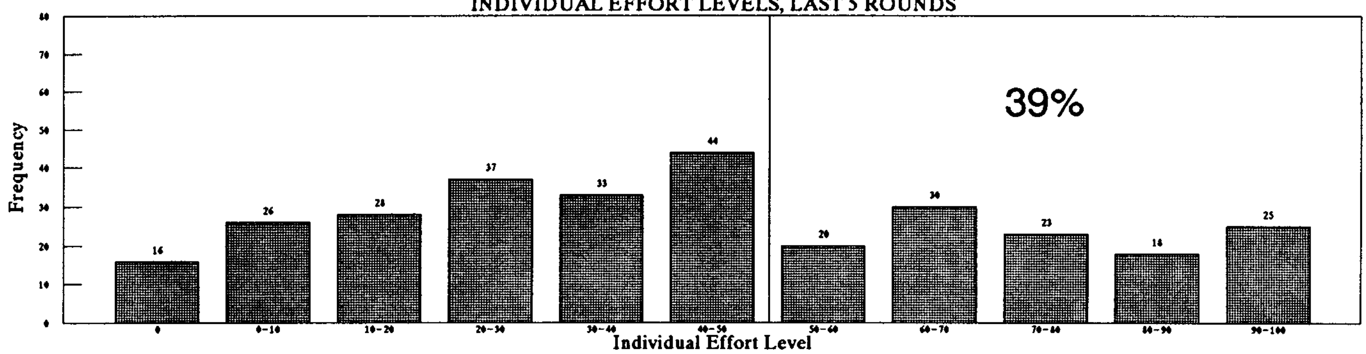


FIGURE 5.8c

FORCING CONTRACTS WITH CERTAINTY
INDIVIDUAL EFFORT LEVELS, LAST 5 ROUNDS

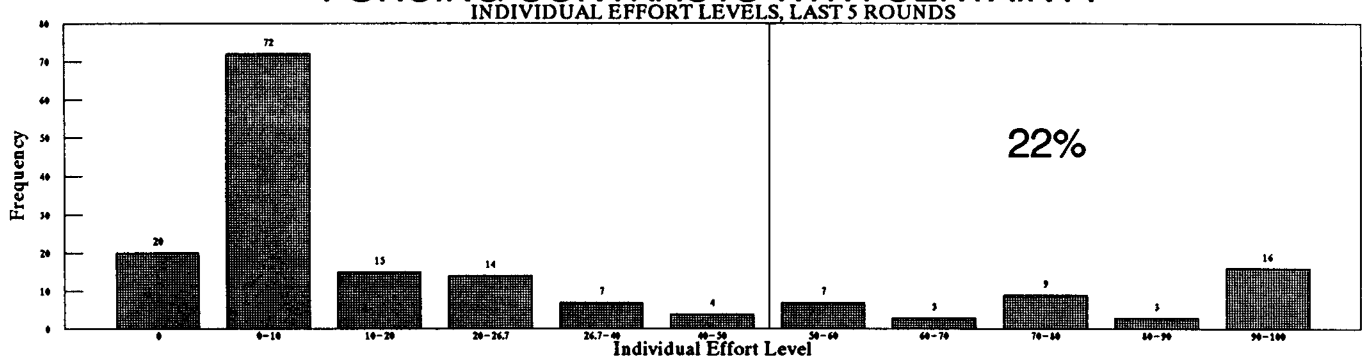


FIGURE 5.8d

Observation 5: Monitoring Works But is Costly

When monitoring is possible but not perfect, high levels of effort can be elicited from workers. However, unless the probability of detection is great (and therefore costly to maintain), such monitoring schemes are likely to fail.

As principal-agent theory tells us, if monitoring is possible it becomes quite easy to elicit optimal levels of effort from workers by simply monitoring them and firing them if they are caught shirking. When only imperfect monitoring is possible or monitoring is so expensive that workers can only be checked sporadically, the cost effectiveness of such monitoring schemes is called into question. What principal-agent theory does not tell us is how sensitive workers will be to the detection probability of shirking. For instance, will even minor detection probabilities lead to high effort levels? Do workers mis-estimate the detection probability or suffer from some type of probability bias as is evidenced in other, decision-under-uncertainty experiments, which lead them to consistently under-estimate the probability of being caught? (See the survey by Camerer (1994, forthcoming) for other instances of probability bias.)

Clearly these are questions that must be answered before we can suggest the relative superiority of monitoring schemes in corporations. Our experimental design furnishes data which gives us some insight into this question. For instance, consider Figures 5.9a and 5.9b:

FIGURES 5.9a and 5.9b HERE

In Figured 5.9a and 5.9b we see the median effort level of subjects in our Monitoring (.70) and Monitoring (.30) experiments run in Phase I and II (.i.e. in Phase I we ran our Monitoring (.70) experiment first and then our Monitoring (.30) experiment while in Phase II the process was reversed). Clearly there is a dramatic difference between the median effort levels of subjects when being monitored

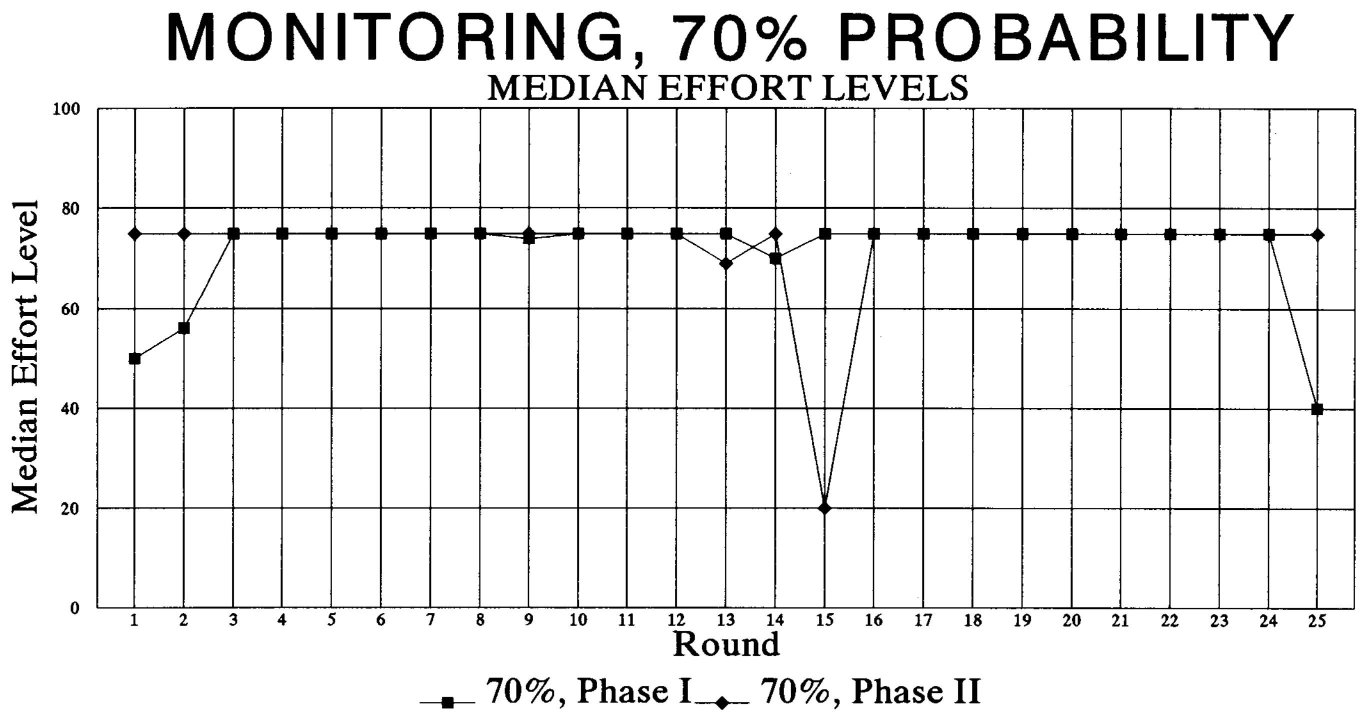


FIGURE 5.9a

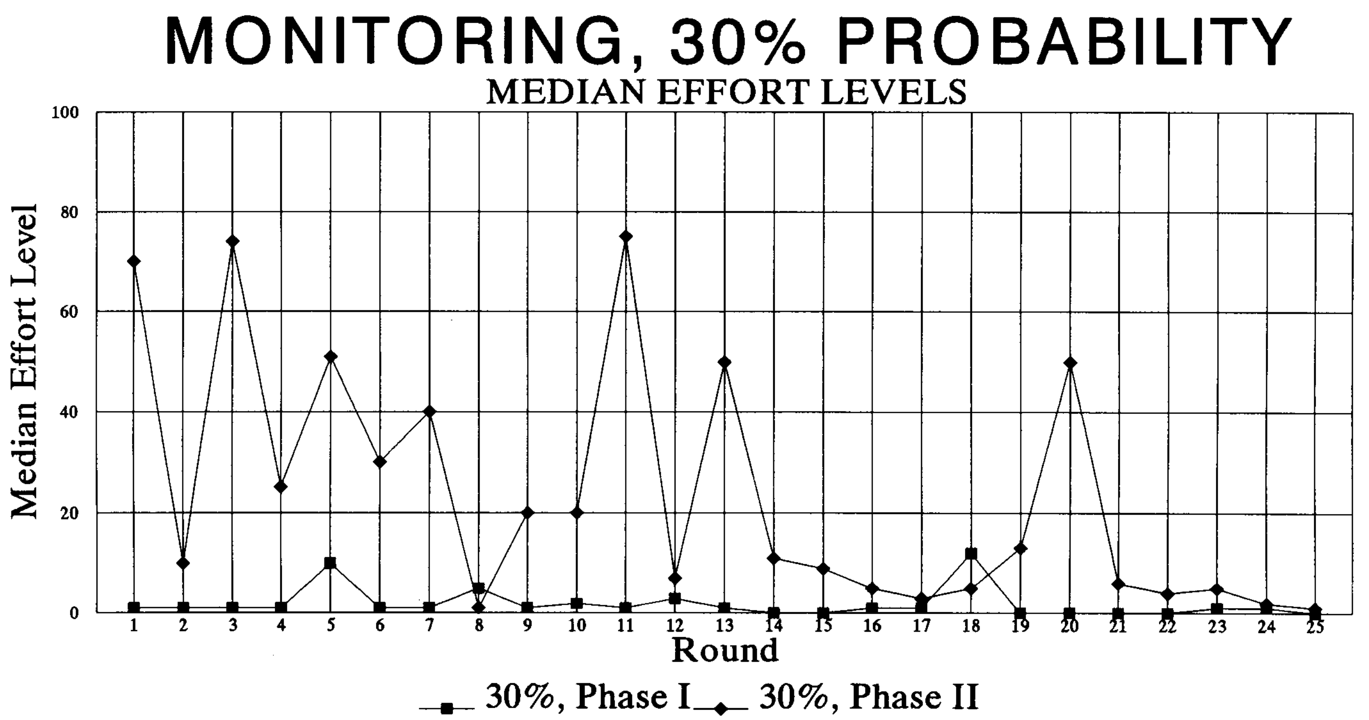


FIGURE 5.9b

with a .70 probability and a .30 probability. This is, of course, to be expected since the optimal response of subjects to a .70 monitoring probability is to choose a Pareto-optimal effort level while the optimal response to a .30 monitoring probability is to shirk. This is seen in Figures 5.9a and 5.9b where we see that groups functioning under a .70 monitoring probability choose Pareto optimal effort levels as a median while the .30 probability groups choose efforts whose median involves almost complete shirking. Furthermore, while high detection levels (.70) lead to consistently high effort levels whether the experiment was run before or after a Revenue Sharing experiment, low detection levels (.30) lead to quite different types of behavior in experiments run before and after Revenue Sharing experiments. This is clearly seen in Figure 5.9b where the median effort level for the low detection group is practically zero in all periods when the experiment is performed in Phase I (after the .70 detection experiment.) When it is performed in Phase II (before the .70 experiment) the results are quite different. What is striking is that the predictions of the theory seem to fail when subjects are not experienced and when they are subject to the low .30 monitoring probability (median effort levels are periodically above the zero effort levels). Obviously, when groups are used to high detection probabilities a drop to low probabilities seems to lead them to reevaluate their effort choices and lower them. Again, history matters.

Section 6: Conclusions

This paper has attempted to take a first step on the road to adding some empirical meat to the skeleton created by theorists working on the problem of group incentives and productivity. In our experiments we have uncovered a number of factors which we think are probably important for the proper design of group incentive mechanisms. Most important among these findings are the following observations: First we've found that the history of a group and their performance in the past is an important predictor of how that group will perform when a new incentive program is introduced. In addition, we have discovered evidence that one effective way to increase group effort is to introduce some within firm competition between work units performing the same task -- setting up an intra-firm team tournament. Finally, our findings indicate that it is not sufficient for a mechanism to implement Pareto Optimal outcomes as Nash equilibria. If those optimal outcomes are to actually be achieved it is necessary that those equilibria be relatively riskless or not "vulnerable" to slight mistakes by ones colleagues. Mechanism which attach considerable risk to selecting the efficient equilibrium may ultimately lead economic agents to opt out of the mechanism and play it safe by shirking.

While these findings strike us as interesting, we are well aware of their limitations. To begin with, our experiments provide at best a bare bones economist's view of the incentive problem. They characterize productive performance as the outcome of a non-cooperative game and are concerned solely with the incentive properties of the various reward formulae as the explanation of behavior. Psychologists, compensation practitioners and others would certainly protest that life is far different in the work place than in our experiments. The character of interactions among workers is considerably more complex than that presumed here. For one thing, work could just as well be modeled as a cooperative game in which workers communicate with each other and implicitly establish work norms which they then enforce upon each other. Results from Dawes, McTagvish and Shaklee (1977) indicate that communication between subjects dramatically affects their performance in public goods experiments. Similarly, while managers may not be able to monitor workers effectively, workers themselves may be in better position to do so. And they'd have more incentive to perform that function in a system where their rewards depend on co-worker performance than in a system where they depend on one's own performance alone. (Nalbantian 1987; Levine and Tyson (1990); Kandel and Lazear (1992)). A full and proper experimental design should allow this factor to come into play.

These are all valid points, but we do not feel that they reduce the significance of what we have uncovered here. Our intent in these experiments was to see how far the orthodox economic model of group incentives and the "corrective" reward formulae deduced therefrom can take us in explaining behavior. As previously noted, substantial evidence from the field does indeed suggest that observed behavior under group incentives if often at variance from that predicted by standard theory. Certain other factors are clearly central to the operation of group incentives and should therefore be incorporated in experimental treatments of the problem. We intend to do so in our future research. Still, in conducting these experiments we have learned important lessons about the properties of prototypical group incentive formulae, findings that transcend the particular context in which they were revealed. We have established an economic baseline which helps determine how much more of observed behavior under group incentives needs to be explained.

APPENDIX

The enclosed appendix presents the instructions for the Profit Sharing Experiments. Other instructions are identical except, of course, for the section entitled "**How Your Payment is Determined**".

INSTRUCTIONS: EXPERIMENT PS1

Introduction

You are about to partake in an experiment on group decision making. A number of research foundations have provided funds to run these experiments. Depending on the decisions you and other participants in the experiments make, you may be able to earn a considerable payoff which will be given to you as you leave.

Your task in this Experiment

As you walked into the room you were randomly assigned to a group of six subjects. You will be in this group for the entire experiment which will last for 25 rounds.

When you sit down at your computer terminal, your screen will appear as follows:

Round	**Decision No.**	**Group Rev.**	**Target Rev.** (112.5 fr.)	**Payment**	**Cost**	**Earnings** paym - cost
1						
2						
3						
4						
5						
6						
7						
8						
9						
10						
.						
.						
.						
.						
.						
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.
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.
.
25

DO NOT TOUCH any computer key until we instruct you to.

In round 1 of the experiment, you and the other five subjects in your group will be asked to type in a number between 0 and 100. The computer will prompt you to do so by stating:

"Please enter a number between 0 and 100"

We call the number you enter your decision number. You enter your decision number by typing it on the number keys and hitting the return key when you are finished. The computer will then confirm your choice by stating:

"You have chosen ___. Is that what you wanted?

If this is, in fact, the decision number you want to enter, push the Y (Yes) key. Your participation in this round of the experiment will then be over. If you wish to change your mind, or you made a mistake in your typing, type N (No), and you will be prompted to choose another number. When you have successfully decided upon a decision number and entered it, your participation in this round of the experiment will be over.

Round-By-Round Payoffs

In each round of the experiment you will receive a payment in a fictitious currency called "francs." (The francs you earn will be converted into dollars at the end of the experiment at a rate to be described shortly). The payment you receive will depend on your decision number and those of the other members of your group as well as the realization of a random number. Precisely how the random number influences your payment is described in the next section. Your actual payoff (or earnings) in any round is the difference between the **payment** you receive and the direct **cost** to you of the decision number you selected as given by the cost schedule of Table 1. In other words:

Earnings = Payment - Decision Cost.

Let us see specifically how both these components determine your earnings.

How Your Payment is Determined

When you and the other members of your group have entered your decision numbers (in column 2), the computer will add them up. We will call the resulting number the **Group Total**. The computer will then randomly choose a number between -40 and +40 and add it to the group total. When we say "randomly," we mean that each number in the interval -40 to +40 has an equal chance of being chosen. Hence the chance of -30 being chosen is equal to the chance of +15 being chosen which in turn is equal to the chance of +5 being chosen, and so on. Finally, the sum of this random number and the **Group Total** will be multiplied by the number 1.5 (francs) to get what we call **Group Revenue** which will appear in column 3 on your screen as "Group Rev." For example, say that the decision numbers of the six members of your group are z_1, z_2, z_3, z_4, z_5, and z_6, where z_1 is the decision number of subject #1, z_2 is the decision number of subject #2 and so on. Further, suppose that the random number generated by the computer is +5. Then the **Group Total** would be: $(z_1 + z_2 + z_3 + z_4 + z_5 + z_6 + 5)$ and the **Group Revenue** would be $1.5(z_1 + z_2 + z_3 + z_4 + z_5 + z_6 + 5)$. As you can see, Group Revenue will thus reflect both the choices of each group member regarding his/her decision number **and** the realization of the random number, namely, pure chance.

Group Revenue (Group Total) is the basis of your individual payment. Specifically, in each round of the experiment your group will be given a target Group Revenue of 112.5 francs. (Note: this corresponds to a Group Total of 75, i.e., 75 x 1.5 = 112.5). If your Group Revenue turns out to be less than 112.5, your payment for the round will be zero. If your Group Revenue precisely equals 112.5, your payment for the round will be the fixed amount of 18.75 francs. On the other hand, if your Group Revenue exceeds 112.5 francs, your personal payment will be the sum of 18.75 francs and one sixth of the difference between your Group Revenue and 112.5 francs. In other words, in addition to the fixed amount, 18.75 francs, you personally will be paid one sixth of the excess of actual Group Revenue over the target Group Revenue. For example, say that your Group Revenue is 172.5 francs which exceeds the target of 112.5 by 60 francs. Your payment for this round would then be 18.75 + 1/6(172.5 - 112.5) = 28.75 francs.

Clearly, above the 112.5 franc threshold or "group target," the larger is Group Revenue, the greater your payment will be, though as you will see, you will have to deduct from your payment the cost associated with your decision number. Below the group target, your payment is a fixed amount (0 francs) independent of Group Revenue.

The group target of 112.5 francs is indicated in column 4 on your screen. Your payment for each round of the experiment will be calculated by the computer and appear in Column 5 on your screen.

How Your Earnings are Determined

Your payoff or earnings in any round will equal the payment you receive, as described above, **minus** the cost of your decision number. Decision costs are presented in Table 1. You will note that for each decision number you might choose over the range 0 to 100, there is an associated cost to be incurred. You can read your cost table by looking down the first column and finding the decision number you are contemplating. The second column will then inform you what it will cost you to choose that decision number. For example, a decision number of 25 has an associated cost of 6.75 francs, while the decision number 50 has a cost of 25 francs. Several important features of this cost schedule are evident in this example and are especially noteworthy. First, the larger the decision number, the higher the cost you must incur. Second, the cost of decision numbers increases **at an increasing rate**. Hence, the cost of choosing decision number 50 is more than twice the cost of choosing 25; The cost of choosing 100 is more than twice the cost of choosing 50. You can verify this characteristic of costs of decision numbers by considering other examples from the cost schedule.

The cost of the decision number you choose will be deducted from the payment you are due in that round to determine your actual earnings for the round. Again,

Earnings = Payment - Decision Cost.

The cost of your decision number for each round will appear in column 6 on your screen.

To illustrate how your earnings will be determined, suppose that Group Revenue in round 1 of the experiment is calculated at 200 francs and that the decision number you selected in that round was 40. Since 200 is greater than 112.5 (the group target), your payment would then be calculated as: 18.75 + 1/6 [200 - 112.5] = 33.33 francs. From Table 1 we find that the cost of decision number 40 which you chose is 16 francs. Therefore, your earnings for round 1 would be: 33.33 - 16 = 17.33 francs. Suppose, on the other hand, that Group Revenue in a given round is 112.5 francs, and that your decision number is again 40. Since the group target is precisely attained, your payment will be 18.75 from which your decision cost must be deducted; your earnings are then calculated as 18.75 - 16 = 2.75 francs. Finally, suppose your Group Revenue is 110 while your decision number remains 40. Since the Group Target has not been achieved, your payment is 0 francs. Thus your earnings for this round would be 0 - 16 francs = -16 francs. (Negative earnings will be deducted from your accumulated earnings at the end of the experiment).

Your earnings or payoff for the round are calculated by the computer and appear in column 7 on your screen.

Final Payoffs

Your final payoff in the experiment will be equal to the sum of the francs received over the twenty five rounds of the experiment. Each franc will be converted at the rate of 1 franc =.71 **cents**. In addition to this payoff, you will receive a fixed payoff of $3.00 just for showing up at the experiment.

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APPENDIX

The enclosed appendix presents the instructions for the Profit Sharing Experiments. Other instructions are identical except, of course, for the section entitled "**How Your Payment is Determined**".

INSTRUCTIONS: EXPERIMENT PS1

Introduction

You are about to partake in an experiment on group decision making. A number of research foundations have provided funds to run these experiments. Depending on the decisions you and other participants in the experiments make, you may be able to earn a considerable payoff which will be given to you as you leave.

Your task in this Experiment

As you walked into the room you were randomly assigned to a group of six subjects. You will be in this group for the entire experiment which will last for 25 rounds.

When you sit down at your computer terminal, your screen will appear as follows:

Round	Decision No.	Group Rev.	Target Rev. (112.5 fr.)	Payment	Cost	Earnings paym - cost
1						
2						
3						
4						
5						
6						
7						
8						
9						
10						
.						
.						
.						
.						
.						
.						
.						
.						
.						
.						
.						

25

<u>DO NOT TOUCH any computer key until we instruct you to.</u>

In round 1 of the experiment, you and the other five subjects in your group will be asked to type in a number between 0 and 100. The computer will prompt you to do so by stating:

"Please enter a number between 0 and 100"

We call the number you enter your decision number. You enter your decision number by typing it on the number keys and hitting the return key when you are finished. The computer will then confirm your choice by stating:

"You have chosen ____. Is that what you wanted?

If this is, in fact, the decision number you want to enter, push the Y (Yes) key. Your participation in this round of the experiment will then be over. If you wish to change your mind, or you made a mistake in your typing, type N (No), and you will be prompted to choose another number. When you have successfully decided upon a decision number and entered it, your participation in this round of the experiment will be over.

Round-By-Round Payoffs

In each round of the experiment you will receive a payment in a fictitious currency called "francs." (The francs you earn will be converted into dollars at the end of the experiment at a rate to be described shortly). The payment you receive will depend on your decision number and those of the other members of your group as well as the realization of a random number. Precisely how the random number influences your payment is described in the next section. Your actual payoff (or earnings) in any round is the difference between the **payment** you receive and the direct **cost** to you of the decision number you selected as given by the cost schedule of Table 1. In other words:

Earnings = Payment - Decision Cost.

Let us see specifically how both these components determine your earnings.

How Your Payment is Determined

When you and the other members of your group have entered your decision numbers (in column 2), the computer will add them up. We will call the resulting number the **Group Total**. The computer will then randomly choose a number between -40 and +40 and add it to the group total. When we say "randomly," we mean that each number in the interval -40 to +40 has an equal chance of being chosen. Hence the chance of -30 being chosen is equal to the chance of +15 being chosen which in turn is equal to the chance of +5 being chosen, and so on. Finally, the sum of this random number and the **Group Total** will be multiplied by the number 1.5 (francs) to get what we call **Group Revenue** which will appear in column 3 on your screen as "Group Rev." For example, say that the decision numbers of the six members of your group are z_1, z_2, z_3, z_4, z_5, and z_6, where z_1 is the decision number of subject #1, z_2 is the decision number of subject #2 and so on. Further, suppose that the random number generated by the computer is +5. Then the **Group Total** would be: $(z_1 + z_2 + z_3 + z_4 + z_5 + z_6 + 5)$ and the **Group Revenue** would be $1.5(z_1 + z_2 + z_3 + z_4 + z_5 + z_6 + 5)$. As you can see, Group Revenue will thus reflect both the choices of each group member regarding his/her decision number **and** the realization of the random number, namely, pure chance.

Group Revenue (Group Total) is the basis of your individual payment. Specifically, in each round of the experiment your group will be given a target Group Revenue of 112.5 francs. (Note: this corresponds to a Group Total of 75, i.e., 75 x 1.5 = 112.5). If your Group Revenue turns out to be less than 112.5, your payment for the round will be zero. If your Group Revenue precisely equals 112.5, your payment for the round will be the fixed amount of 18.75 francs. On the other hand, if your Group Revenue exceeds 112.5 francs, your personal payment will be the sum of 18.75 francs and one sixth of the difference between your Group Revenue and 112.5 francs. In other words, in addition to the fixed amount, 18.75 francs, you personally will be paid one sixth of the excess of actual Group Revenue over the target Group Revenue. For example, say that your Group Revenue is 172.5 francs which exceeds the target of 112.5 by 60 francs. Your payment for this round would then be 18.75 + 1/6(172.5 - 112.5) = 28.75 francs.

Clearly, above the 112.5 franc threshold or "group target," the larger is Group Revenue, the greater your payment will be, though as you will see, you will have to deduct from your payment the cost associated with your decision number. Below the group target, your payment is a fixed amount (0 francs) independent of Group Revenue.

The group target of 112.5 francs is indicated in column 4 on your screen. Your payment for each round of the experiment will be calculated by the computer and appear in Column 5 on your screen.

How Your Earnings are Determined

Your payoff or earnings in any round will equal the payment you receive, as described above, **minus** the cost of your decision number. Decision costs are presented in Table 1. You will note that for each decision number you might choose over the range 0 to 100, there is an associated cost to be incurred. You can read your cost table by looking down the first column and finding the decision number you are contemplating. The second column will then inform you what it will cost you to choose that decision number. For example, a decision number of 25 has an associated cost of 6.75 francs, while the decision number 50 has a cost of 25 francs. Several important features of this cost schedule are evident in this example and are especially noteworthy. First, the larger the decision number, the higher the cost you must incur. Second, the cost of decision numbers increases **at an increasing rate**. Hence, the cost of choosing decision number 50 is more than twice the cost of choosing 25; The cost of choosing 100 is more than twice the cost of choosing 50. You can verify this characteristic of costs of decision numbers by considering other examples from the cost schedule.

The cost of the decision number you choose will be deducted from the payment you are due in that round to determine your actual earnings for the round. Again,

Earnings = Payment - Decision Cost.

The cost of your decision number for each round will appear in column 6 on your screen.

To illustrate how your earnings will be determined, suppose that Group Revenue in round 1 of the experiment is calculated at 200 francs and that the decision number you selected in that round was 40. Since 200 is greater than 112.5 (the group target), your payment would then be calculated as: 18.75 + 1/6 [200 - 112.5] = 33.33 francs. From Table 1 we find that the cost of decision number 40 which you chose is 16 francs. Therefore, your earnings for round 1 would be: 33.33 - 16 = 17.33 francs. Suppose, on the other hand, that Group Revenue in a given round is 112.5 francs, and that your decision number is again 40. Since the group target is precisely attained, your payment will be 18.75 from which your decision cost must be deducted; your earnings are then calculated as 18.75 - 16 = 2.75 francs. Finally, suppose your Group Revenue is 110 while your decision number remains 40. Since the Group Target has not been achieved, your payment is 0 francs. Thus your earnings for this round would be 0 - 16 francs = -16 francs. (Negative earnings will be deducted from your accumulated earnings at the end of the experiment).

Your earnings or payoff for the round are calculated by the computer and appear in column 7 on your screen.

Final Payoffs

Your final payoff in the experiment will be equal to the sum of the francs received over the twenty five rounds of the experiment. Each franc will be converted at the rate of 1 franc =.71 **cents.** In addition to this payoff, you will receive a fixed payoff of $3.00 just for showing up at the experiment.