ECONOMIC RESEARCH REPORTS

CORPORATE INCENTIVES AND LONG-TERM BONUSES: AN EXPERIMENTAL STUDY

by

Andrew Schotter

and

Keith Weigelt

R.R. #87-32 September 1987

C. V. STARR CENTER FOR APPLIED ECONOMICS

NEW YORK UNIVERSITY
FACULTY OF ARTS AND SCIENCE
DEPARTMENT OF ECONOMICS
WASHINGTON SQUARE
NEW YORK, N.Y. 10003

CORPORATE INCENTIVES AND LONG–TERM BONUSES: AN EXPERIMENTAL STUDY

by

Andrew Schotter*
Keith Weigelt**

June 1987

* Department of Economics, New York University
** Department of Management, New York University Graduate School of Business
This project was funded in part by a grant from the New York University Fund to Develop University/Industry Linkages. Other research support was offered by the C.V. Starr Center for Applied Economics at New York University. The authors would like to thank Clive Bull for his valuable advice in the early stages of this project's development. Our thanks also go to Ionnis Loumakis for his programming assistance and Ken Rogoza for his research assistance.

CORPORATE INCENTIVES AND LONG–TERM BONUSES: AN EXPERIMENTAL STUDY

Andrew Schotter and Keith Weigelt

June 1987

The task of providing the proper set of managerial incentives is a complex problem in principal–agent theory (see, Ross (1973) and Shavell (1979)). The problem arises because executives may discount the future at a higher rate than stockholders and hence determine policies that are too myopic from the stockholders'point of view. This high discount rate may induce corporate decisions which yield flashy short–run profits today at the expense of long run profits tomorrow. To align these two conflicting sets of preferences, corporations have developed incentive bonus programs which try to motivate executives toward the corporation's goals. (See Rock (1984) for a description of the rationale for long–term bonus plans.)[1] Using short and long–term bonus plans,managerial compensation is partially put "at risk" because compensation is made a function of corporate (or business unit) performance. It is our contention that many times these plans fail to reconcile the divergent preferences of the corporation and its managers. To illustrate this point we ran a set of laboratory experiments with paid volunteers using graduate students at the New York University Graduate School of Business. In these experiments we attempted to replicate the compensation program of one Fortune 500 corporation (Corporation X) which agreed to share with us the mechanics of their executive compensation program. Our aim was to investigate the incentive effects of their newly instituted long–term bonus program to see if it yielded less mypoic behavior amongst laboratory subjects than did their short–term program.

---

[1] A recent Wall Stree Journal article (4/10/87) indicated that as much as 30% of executive compensation was now in the form of long–term bonuses.

Further, to demonstrate that the preference divergence problem discussed earlier is not irreparable, we devised and tested an alternative long–term compensation program. Our conclusions substantiate our original suspicions. The long–term bonus program offered by Corporation X was behaviorally identical to its short–term bonus program when a stylized version of both plans were tested in the laboratory. However, when we attempted to modify behavior by instituting an alternative plan, we found that such a modification did occur and, the resulting behavior was consistent with our predictions. While this alternative plan is not one that we are necessarily advocating, its existence does indicate that other, possibly more attractive, alternatives may exist.

We will proceed as follows: In Section 2 we motivate our paper by intuitively discussing the incentive program of Corporation X and the tradeoffs it offers its executives. In Section 3 we describe the experiments and their design. In Section 4 we present the predictions of our experiments by deriving the compensation maximizing behavior of our laboratory subjects under the schemes defined in Section 3. Section 5 presents our hypotheses and results and in Section 6 our conclusions are presented.

Section 2: Corporation X and its Executive Incentives

For expositional simplicity we will present a stylized version of Corporation X's executive incentive program. In addition to a base salary, B, executives are offered two bonus programs: a short–term bonus program where the bonus paid is an increasing function of the current performance (call it profit) of the executive's business unit, and a long–term program in which the bonus paid is a function not only of current profit but also of the arithmetic mean of profit over the past four periods. To represent this plan as simply as possible, assume that under the short–term bonus program if π_t is some meaure of corporate performance in period t then the short–term bounus in that period is $S_t = \gamma\pi_t$, i.e. the short–term bonus in period t is proportional to the performance (profit) of the corporation in that period. The long–term bonus in period t will be $L_t = \gamma((\pi_t+\pi_{t-1}+\pi_{t-2}+\pi_{t-3})/4)$.[2] Total compensation in period t will then be $C_t = B_t + S_t + L_t = B_t + \gamma(\pi_t) + \gamma(\pi_t + \pi_{t-1} + \pi_{t-2} + \pi_{t-3}/4)$. Finally, let δ^c be the discount factor for the corporation indicating how it values future profits and δ^E be the discount factor for the executive, with $\delta^c > \delta^E$, i.e., executives discount the future at a greater rate than do the stockholders of the corporation. [The discount "factor" of the corporation and its executives are $\delta^c = 1/(1+r)$ and $\delta^E = 1/(1+\rho)$ respectively, where r and ρ are the discount "rates" they use.] Since such incentive programs are only offered to those executives whose decisions substantially affects the firm's performance, we make the simplifying assumption that Corporation X contains only one executive whose decisions, along with elements of chance, uniquely determine the performance of the corporation.[3]

Assuming that a link exists between the decisions made by executives today to corporate profits in the future, an executive would, given δ^E and a desire to maximize his discounted compensation stream over his tenure at the corporation, determine a set of decisions which would define a path of expected profits (whose expectations depend on chance) over the executive's tenure. If $\delta^E > \delta^c$, then these decisions will yield a path of expected profits which, when viewed by the corporation, are too high in the present and and too low in the future. The question then arises as to whether Corporation X's long–term bonus program does anything to rectify the situation. The answer, as we will formally see in Section 4, is no. Depending upon how the executive perceives the plan, the compensation maximizing executive decisions are either identical to the ones made under the short–term plan or are more myopic. In other words,

---

[2]Of course it is not typically current profits to which bonuses are linked but rather some other index of corporate performance such as the price/earnings ratio of the corporation. Still, the point remains that no matter what these indices are, the short–run and the long–run plan differ only in that the long–run bonuses are calculated as arithmetic means of the same indices used to calculate short term bonuses.

[3]This assumption, of course, abstracts from any group dynamics that may affect executive behavior in large corporations.

either the plan is ineffective or it makes things worse. The reason is that, ignoring start–up and terminal problems, any set of decisions which maximize the executive's short–term bonus will also maximize his long–term bonus since it is simply a weighted average of the former. Hence we believed that if laboratory subjects performed a task equivalent to the one described above, we would either see no difference in the behavior of those subjects who were compensated using only the short term program and those that were compensated using both the long–term and the short–term programs, or, if start–up and terminal conditions are considered, subjects would behave more myopically under the combination of long and short–term programs. This conjecture furnished the main hypothesis for our experiment.

Corporations X's long–term compensation plan fails to change the executive's trade–offs between current and future performance over and above that already reflected in the company's short–term bonus program and the executive's own discount rate. A successful long–term bonus program alters the executive's perception of the future, either by changing the objective trade–offs he faces or the subjective rate at which he discounts the future. To pursue this further we developed and tested an alternative laboratory compensation scheme. The rationale and description of our plan are as follows: If an executive discounts the future at discount rate ρ and if that rate is greater than that of the corporation, it means that the executive cares less about the future than the corporation does. To induce him to care more about future profits, the corporation could make the value of future profits greater to the executive by magnifying or inflating his compensation in later periods. This may be done by making his compensation in period t (t years after he joined the firm) a function of the discounted present value of those profits discounted back to the period in which the executive joined the firm at a predetermined rate of λ.(λ may or may not be equal to r). What this says is that profits earned t years after an executive enters the corporation are worth more to him than profits earned at earlier times. This fact alters the way the executive views the future (i.e. it alters his effective discount rate) because it presents him with a new tradeoff. Since the corporation will pay him more for profits earned later on in his tenure, he will view profits earned today less favorably. Hence, if the corporation sets the incentive parameters correctly, they can alter the executive's behavior to bring it into line with their own desires.

Section 3: The Experiment and its Design

A set of four experiments were run to test the properties of Corporation X's executive program. Sixty two graduate business students were recruited each of whom earned, on average, about \$20.00 for a $1^{1/2}$ hour session.[4]

a) The Experimental Task:

Subjects reported to a computer lab and were given written instructions describing the experiment and their decision task (see Appendix A). After the instructions were read, an experimental administrator read them aloud and answered any questions. In all experiments, the decision task was identical, although the payoff structure differed between experiments with each structure mimicking a different type of compensation program. Subjects had 400 "units" to allocate over a ten round decision problem. In round 1 they would decide how many of their 400 units to allocate, and enter that number into personal computer. Then, the subject generated a random number from a uniform distribution whose range went from −30 to +30 by hitting a key on the computer . The computer then took this random number, added it to the subject's decision number, (determining what was called the subject's "total" number) and found its square root. This square root was called the subject's "output" for that round. Depending upon the incentive program being replicated, this output was then transformed into a unit payoff which was later converted into dollars. After round 1

[4]We chose to use graduate business students since this is a population of potential corporate executives and inferences made from these subjects might have greater external validity than those made using undergraduates, which is the usual practice amongst experimentalists.

was over, round 2 began in an identical manner except that now the subject could allocate any amount from zero up to their remaining units. This procedure was repeated for ten periods. In the last period, all unused units were automatically allocated. Finally, in any period, if a subject's total number (i.e. the sum of his allocated units and random number) was negative, his output was the negative of the square root of the absolute value of that negative number. For instance, if a subject chose a decision number of 8 and had a random realization of −17, his total number would be −9. His output for that period would therefore be $\sqrt{|-9|} = -\sqrt{|9|} = -3$. This procedure allowed us not to have to discuss the concept of imaginary numbers in our instructions. When the ten rounds of the experiment were complete, another identical one began with the subject again given 400 units. Each subject did twenty such ten−round experiments.

b) Payoffs

The four experiments differed only with respect to the way payoffs were determined.

1) Experiment 1

In Experiment 1, in each round, after a subjects output is determined its value is multiplied by a number called the "multiplier". The values of the multiplier differ depending upon the period in which the output is earned. In Experiment 1, (and all other experiments) the multipliers were:

Table 1: Multipliers

Round	Multiplier
1	5.15
2	4.29
3	3.58
4	2.98
5	2.48
6	2.21
7	1.71
8	1.44
9	1.20
10	1.00

A subject's dollar payoff is found by multiplying his output in each period by its relevant multiplier, adding up the resulting ten products, and converting the resulting amount into dollars at some prespecified rate. If output is negative, its value is not multiplied.

The experiment captured the salient characteristics of Corporation X's short-term bonus plan in the following way. One can think of the 400 "units" as an input which if spent today will yield profits today but which if spent tomorrow will yield profits tomorrow. The trade-off for the subjects between output today and tomorrow is represented by the multiplier which is derived by assuming a 20% discount rate for the subject which we use to discount forward to the end of the experiment any output he earns. Hence, one unit of output earned in round 1 is worth 5.15 units if allowed to compound at 20% interest for the remaining nine periods, while the same unit earned in period 5 is worth only 2.48 units if allowed to compound at 20% for five periods. In addition, the concave output function exhibits a diminishing marginal product which bounds the optimal amount of units to allocate in any period. The element of managerial luck (or bad luck) is captured by the use of our random number. Asking how these units will be allocated over the ten rounds of the experiment is then equivalent to asking how executives in our stylized version of Corporation X will make decisions which yield profits to the corporation over the horizon of their tenure.

All output accumulated over the course of the experiment was converted into dollars at a prespecified rate.

2)Experiment 2: Short and Long Term Bonuses

In of Experiment 2, in addition to receiving the output of the round, subjects were also paid the mean of their outputs over the past four rounds.(During the first three periods, these means were taken over one, two, and three periods, respectively.) The sum of their current output plus this moving average was then multiplied by an identical set of multipliers as used in Experiment 1. This experiment replicated Corporation X's present combination of long–and short–term bonus programs.

3) Experiments 3 and 4: An Alternative Program

Experiments 3 and 4 attempted to test alternative incentive programs. In these experiments, output earned in any round was first multiplied by a number called a magnifier, and then this product was multiplied by a multiplier. For Experiments 3 and 4 these multiplier and magnifiers are listed below:

Table 2: Multiplier and Magnifier in Experiments 3 and 4

Experiment 3			Experiment 4		
Round	Magnifier	Multiplier	Round	Magnifier	Multiplier
1	1.00	5.15	1	1.00	5.15
2	1.20	4.29	2	1.08	4.29
3	1.44	3.58	3	1.16	3.58
4	1.72	2.98	4	1.26	2.98
5	2.21	2.48	5	1.36	2.48
6	2.48	2.21	6	1.47	2.21
7	2.98	1.72	7	1.59	1.72
8	3.58	1.44	8	1.71	1.44
9	4.29	1.20	9	1.85	1.20
10	5.15	1.00	10	1.99	1.00

The multipliers were identical to thise used in Experiments 1 and 2 and induced a discount rate of 20% The magnifiers induce behavior consistent with the corporation's discount rate. They magnify output earned in later rounds of the experiment by discounting that output at a discount rate of λ back to the first round of the experiment. To illustrate how this works, assume that the corporation does not discount the future at all. Then, the corporation values output earned in any period as equivalent to that earned in any other period. If the executive discounted the future at 20% (as our multiplier induces him to do), then by setting λ=.20 (Experiment 3), we can induce subjects to value output equally across all rounds. (Notice that in Experiment 3,the product of the multiplier and magnifier is constant and equal to 5.15 in all rounds. Since output in all rounds is multiplied by the product of these two numbers, this treatment induces a zero discount rate on the subject). In Experiment 4 we set λ=.08. Here the magnifier does not completely offset the subject's discount rate.

Table 3 presents our experimental design.

Table 3: Experimental Design

Exp	Exp. length	no. of trials	endowment per.10 rds.	incentive program	no. of Subjects
1	10 rds.	20	400	short–term	12
2	10 rds.	20	400	long and short–term	16
3	10 rds.	20	400	alternative plan λ=.20	18
4	10 rds.	20	400	alternative plan λ=.08	16

As can be seen from Table 3, each experiment was conducted with a separate group of subjects who performed their experiment using one and only one set of parameters.

Section 4: The Experiment's Predictions

In this section we present the optimal payoff maximizing solutions to our experimental incentive programs. These solutions generate our theoretical predictions.

Experiment 1

Experiment 1 poses the following problem for a risk neutral subject:

Max,

$$\sum_{t=1}^{10} [E(x_t+\epsilon_t)^{1/2}(1.20)^{T-t}], \qquad \text{s.t.}$$

$$\sum_{t=1}^{10} x_t = 400, \qquad E(\epsilon_t) = 0$$

where ϵ_t is an i.i.d. random variable distributed uniformily over the interval [−30,+30]. This static maximization problem yields the following familiar first–order condition:

$$\frac{1/2E(x_t+\epsilon_t)^{1/2}(1.20)^{T-t}}{1/2E(x_{t-j}+\epsilon_{t-j})^{1/2}(1.20)^{T-(t+j)}} = 1$$

or

$$E(x_t+\epsilon_t)^{1/2} = E(x_{t-j}+\epsilon_{t-j})^{1/2}/(1.20)^j$$

In short, the subject should allocate his units so that the expected discounted marginal product of these units is equal across periods. This yields the following optimal allocation:

Table 4: Optimal Allocation Experiment 1

Round	Allocation
1	129.44
2	88
3	60.80
4	42.70
5	25.80
6	20.70
7	12.90
8	9.06
9	6.47
10	3.88

Experiment 2

The maximization problem to be solved in Experiment 2 depends upon the way subjects interpret the incentive scheme. For instance, a plan in which long-term bonuses are a four period moving average of previous outputs must have a different set of rules to govern how long-term bonuses are determined in the first three years of the program before there is a four period history over which to take averages. In Corporation X, the long term bonus of a new employee in year 1 is simply equal to his short-term bonus that year. His long-term bonus in year 2 is the mean of his short-term bonus in the first two years etc.. When the total horizon is not long and payoffs are discounted, the rules governing this start-up period can have a significant impact on the decisions of subjects. Hence, the subjrct's maximization problem depends upon whether the subject perceives this start-up problem or not. If they do, then they would choose to maximize:

$$V = [(2E(x_1+\epsilon_1)^{1/2}](1.20)^{T-1} + [E(x_2+\epsilon_2)^{1/2}+1/2(E(x_1+\epsilon_1)^{1/2} + E(x_2+\epsilon_{2)}{}^{1/2})](1.20)^{T-2}+[E(x_3+\epsilon_3)^{1/2} + 1/3(E(x_1+\epsilon_1)^{1/2}+E(x_2+\epsilon_2)^{1/2}+E(x_3+\epsilon_3)^{1/2})](1.20)^{T-3} + \sum_{t=4}^{10} [E(x_t+\epsilon_t)^{1/2}+1/4(E(x_t+\epsilon_t)^{1/2}+E(x_{t-1}+\epsilon_{t-1})^{1/2} + E(x_{t-2}+\epsilon_{t-2})^{1/2}+E(x_{t-3}+\epsilon_{t-3})^{1/2})](1.20)^{T-t}$$

The first-order conditions here can be written as functions of $E(x_1+\epsilon_1)$, and satisfy the following set of ten equations.

$$E(x_t+\epsilon_t)^{1/2} = \frac{E(x_1+\epsilon_1)^{1/2}}{(1.20)^{(t-1)}}\gamma(t), \qquad t = 2,\ldots,10$$

$$\sum_{t=1}^{10} x_t = 400$$

Here, $\gamma(t)$ is a variable that is a function of time. For the parameters in Experiment 2 we find,

$\gamma(2)=.8699, \gamma(3)=.854, \gamma(4) = \gamma(5) = \gamma(6) = \gamma(7) = .80,$

$\gamma(8) = .753, \gamma(9) = .697, \gamma(10) = .636$, and the optimal allocation of units is found in column 2 of Table 5:

Table 5: Optimal Allocations Experiment 2

Optimal Allocation Complete Problem		Optimal Allocation Steady State Problem	
Round	Allocation	Round	Allocation
1	159.48	1	129.44
2	83.55	2	88
3	55.95	3	60.80
4	34.53	4	42.70
5	23.98	5	25.80
6	16.65	6	20.70
7	11.56	7	12.90
8	7.02	8	9.06
9	4.18	9	6.47
10	2.42	10	3.88

If the start–up problems discussed above are not perceived, then the problem is transformed into its fictious "steady state" form by the subjects and formulated as if it started in period 4 and continued for ten more periods. More precisely, they would maximize

$$\sum_{t=4}^{14} E(x_t+\epsilon_t)^{1/2} + 1/4[E(x_t+\epsilon_t)^{1/2}+E(x_{t-1}+\epsilon_{t-1})^{1/2} +E(x_{t-2}+\epsilon_t)^{1/2} +E(x_{t-3}+\epsilon_t)^{1/2})](1.20)^{T-t},$$

s.t.

$$\sum_{t=4}^{14} x_t = 400.$$

In this case the first-order conditions become identical to the ones in Experiment 1, yielding an identical solution (column 4 in Table 5). Hence, if the problem is perceived in its complete form, we expect the addition of a long-term bonus program will make our subjects act as if they were more myopic, while if it is perceived in its steady state form, it yields identical behavior to that of Experiment 1.

Experiments 3 and 4

In experiments 3 and 4 our subjects maximize the following objective function:

$$\sum_{t=1}^{10} [(E(x_t+\epsilon_t)(1.\lambda)^t)^{1/2}](1.\rho)^{T-t}$$

s.t.

$$\sum_{t=1}^{10} x_t = 400$$

where, ρ is the discount rate induced on the subjects, and λ is the rate used to affect behavior in our alternative program. The first-order conditions here are a generalization of those in Experiment 1. They take the form,

$$E(x_{t+j}+\epsilon_{t+j})^{1/2} =E(x_t+\epsilon_t)^{1/2}[(1+\lambda)^j/(1+\rho)^j].$$

Note that when $\rho=\lambda$, the optimal allocation is one in which 40 is allocated in each round, while when $\lambda=0$, the first-order conditions are identical to those in Experiment 1. In Experiment 4, we set $\lambda=.08$ yielding the following optimal allocation:

Table 6: Optimal Allocation Experiments 3 and 4

Experiment 3 (λ=.20)

Round	Allocation
1	40
2	40
3	40
4	40
5	40
6	40
7	40
8	40
9	40
10	40

Experiment 4(λ=.08)

Round	Allocation
1	87.2
2	70.7
3	56.6
4	46.3
5	35
6	30.2
7	24.6
8	19.95
9	16.18
10	13

The allocation schedules presented in Tables 4, 5, and 6 present our predictions for the different laboratory incentive programs we examined. These predictions are presented in Figures 1 through 5 below.

[FIGURE 1–5 HERE]

As we can see, if subjects maximize their earnings in the experiments we can expect to observe no difference in the allocations of subjects in experiments 1 and 2 if start–up and terminal conditions are ignored (compare Figures 1 and 2). If start–up and terminal conditions are considered, Experiment 2 should determine an allocation which is more heavily skewed toward earlier periods (compare Figures 2 and 3). If our alternative scheme is used with λ=.20, then output should be constant at 40 over the length of the experiment (see Figure 4), while if λ=.08 as it is in Experiment 4, the path chosen should be intermediate between the constant path and the short–term allocation path (compare Figures 4 and 5).

Section 5: Hypotheses and Results:

a) Hypotheses:

FIGURES 1-5

Figure 1: Optimal Allocations
Experiment 1

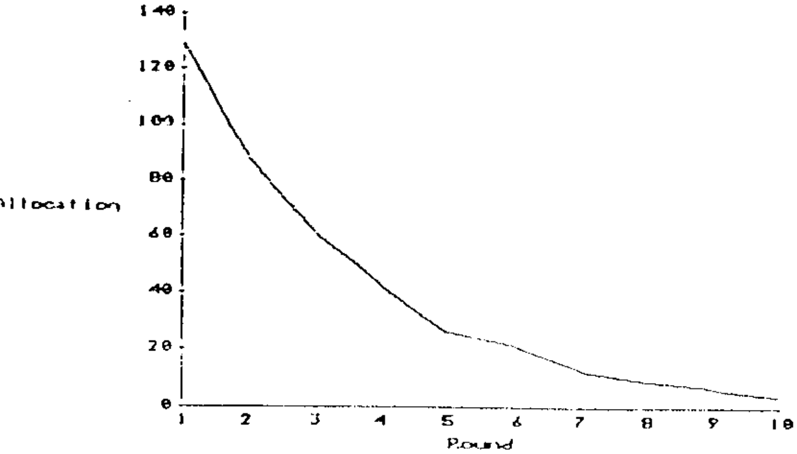


Figure 2: Optimal Allocations
Experiment 2
Complete Problem

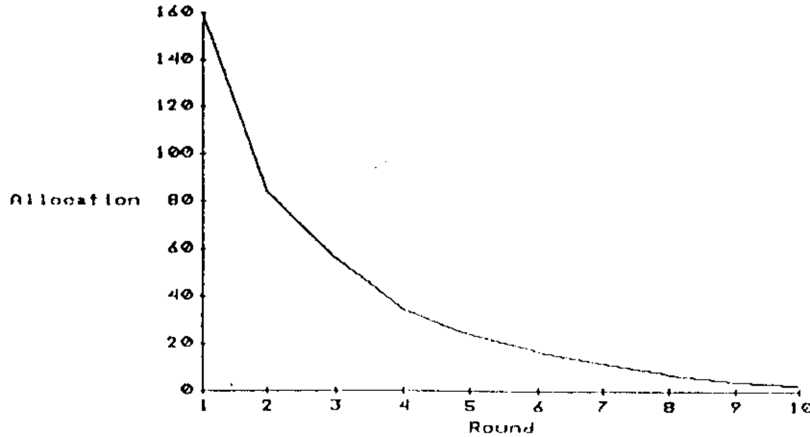


Figure 3: Optimal Allocations
Experiment 2
Steady State Problem

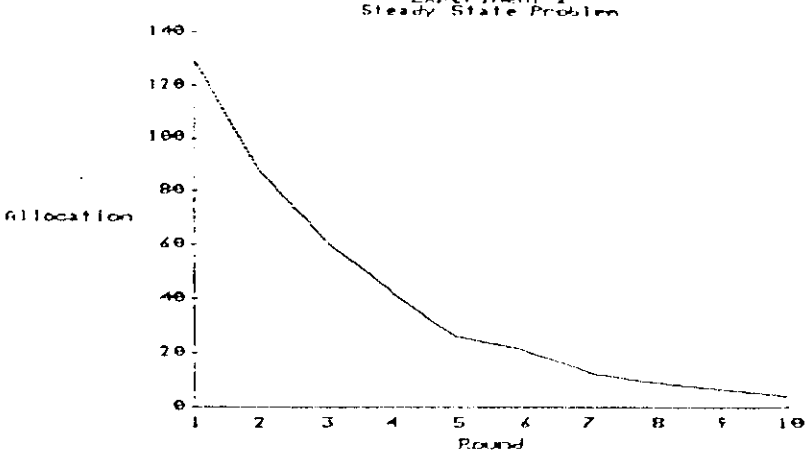


Figure 4: Optimal Allocations
Experiment 3

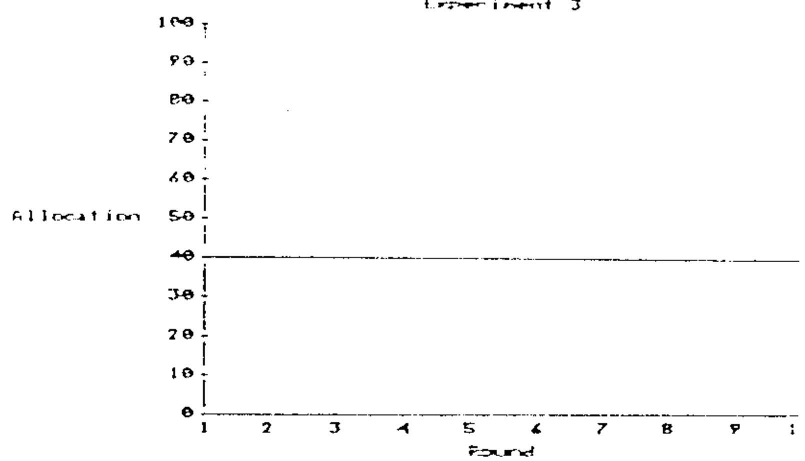
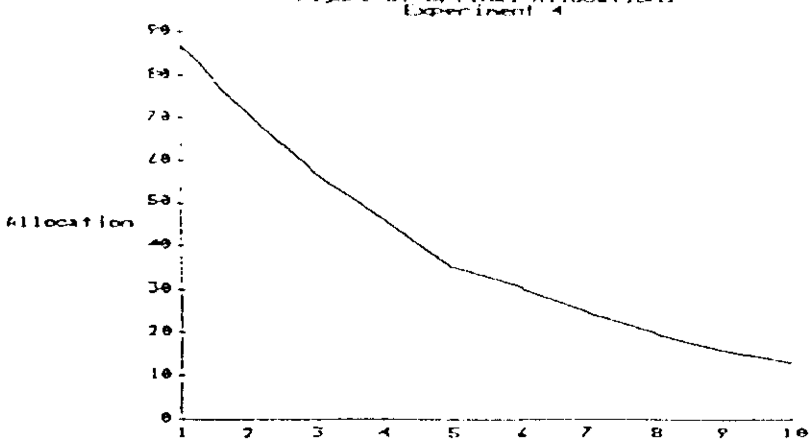


Figure 5: Optimal Allocations
Experiment 4



The predictions described above furnish testable hypotheses. Each prediction generates two forms of a hypothesis –– a strong form and a weak form. In the strong form hypothesis, we investigate whether the observed behavior of subjects conforms to the predictions made above. The weak form hypotheses tests whether the behavior of subjects changes as we vary our experimental incentive program. Hence, our data may fail to confirm some or all of our strong form hypotheses, yet offer support for the weak form. We perform all of our statistical tests using pooled data from the 20^{th} iteration of our ten–round experiments.

Hypothesis 1: Strong Form Hypotheses

In Experiments 1–4, the mean allocation path observed in the 20^{th} iteration of our ten–round allocation problem will not differ from the allocations presented in Tables 4, 5, and 6.

This hypothesis tests the goodness of fit between subjects' observed and predicted behavior.

Hypothesis 2: Long and Short–Term Bonus Plans

The allocation path observed in Experiment 1 (Short–Term Bonus Program) does not differ from the allocation path observed in Experiment 2 (Corporation X's Short–Term plus Long–Term Bonus Program).

This hypothesis states our major conjecture. It was our belief that if start–up and terminal conditions were ignored by our subjects, their behavior under our joint long–term/short–term bonus condition (Experiment 2) would not differ significantly from the behavior of subjects in our short–term bonus condition (Experiment 1).

In Hypotheses 3a and 3b, we test if our alternative plan was effective in altering the behavior of our laboratory subjects. In Experiment 3 we set the parameters of the experimental version of our plan so as to induce a zero rate of time preference on the part of our subjects. According to the maximizations carried out in Section 4, this should determine an allocation path that was flat and allocated a constant of 40 units per period. The consequence of this path is that subjects should act less myopically than they did in Experiment 1; allocating fewer units than subjects in Experiment 1 during the first three rounds, more during rounds 5–10, and an equal amount in period 4.(A more formal definition of myopia will be offered later). (See Figure 6 below). This prediction is tested in Hypothesis 3a.

[FIGURE 6 HERE]

Hypothesis 3a: An Alternative Bonus Program With a zero discount rate

If $\mathbf{x}_t(1)$ and $\mathbf{x}_t(3)$ are the mean allocations observed in the t^{th} round of the 20^{th} iteration of Experiments 1 and 3 respectively, then

$\mathbf{x}_t(1) > \mathbf{x}_t(3)$ for $t \leq 3$,

$\mathbf{x}_t(1) = \mathbf{x}_t(3)$ for $t = 4$,

$\mathbf{x}_t(1) < \mathbf{x}_t(3)$ for $t > 4$.

In Experiment 4 our alternative laboratory incentive program was again tested this time using a different set of parameters. Using these parameters, subjects should behave more myopically than in Experiment 3 but less so than in Experiment 1. Operationally, this means that over the first three rounds of each of these experiments we should observe allocations in Experiment 1 being greater than those in Experiment 4 which in turn are greater than those in Experiment 3, while in periods five through ten we should observe allocations in Experiment 3 being greater than those in Experiment 4 which in turn are greater than those in Experiment 1. In period 4 we predict no differences in the allocations. These expectations are summarized in Hypothesis 3b and Figure 7 below.

FIGURES 6-7

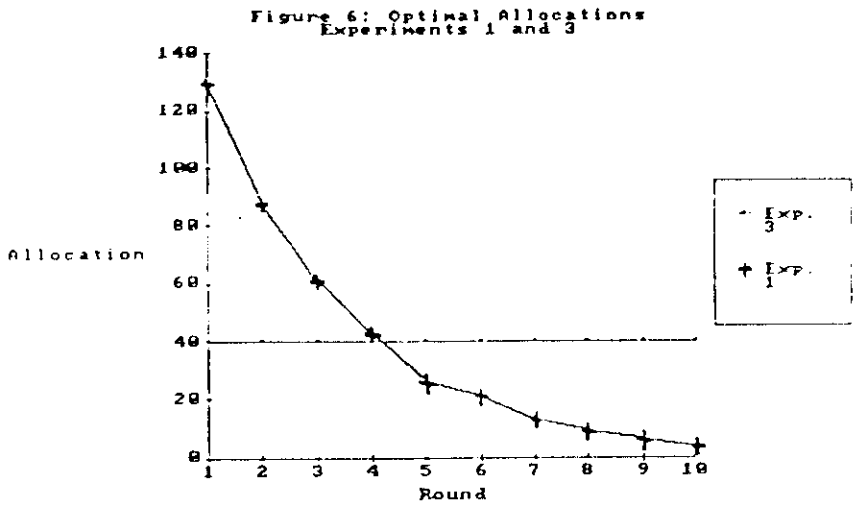


Figure 6: Optimal Allocations
Experiments 1 and 3
140
120
100
80
60
40
20
0
Allocation
1
2
3
4
5
6
7
8
9
10
Round
Exp. 3
Exp. 1

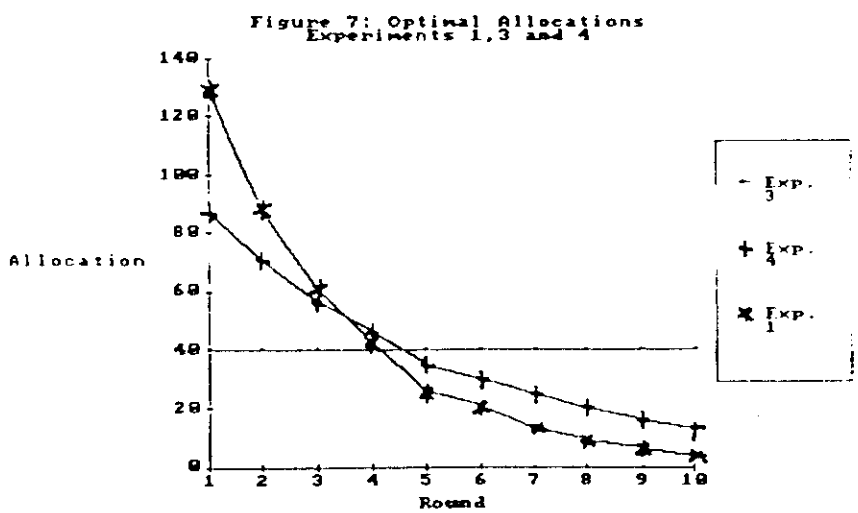


Figure 7: Optimal Allocations
Experiments 1,3 and 4
140
120
100
80
60
40
20
0
Allocation
1
2
3
4
5
6
7
8
9
10
Round
Exp. 3
Exp. 4
Exp. 1

Hypothesis 3b: An Alternative Bonus Program

If $x_t(1)$, $x_t(3)$, and $x_t(4)$ are the mean observed allocations of subjects in period t of experiments 1, 3, and 4, then,

$x_t(1) > x_t(4) > x_t(3)$ for all $t \leq 3$, and

$x_t(3) > x_t(4) > x_t(1)$ for all $t > 3$,

$x_t(3) = x_t(4) = x_t(1)$ for $t = 4$

b) Results

Hypothesis 1

Because standard goodness-of-fit tests do not apply to data of the type generated by our experiment, we will have to rely on visual inspection in discussing the results of Hypothesis 1. Figures 8 through 12 present the plots of the expected and mean observed allocations of our subjects in the 20^{th} iteration of each experiment. [Figures 9 and 10 present the results under the assumptions that start-up and terminal conditions are considered and ignored respectively in Experiment 2.] As we can see in all experiments, the fit is rather close between observed mean and predicted allocations. For example, in all rounds of Experiment 1 the observed 20^{th} iteration mean allocation observed never differed from that predicted by more than 14.1, while the mean deviation from the predicted across all rounds was 5.89. In Experiment 3 this maximum difference was only 6.5 while the mean difference was only 2.43. Note also that it appears that Experiment 2 subjects tended to allocate less in round 1 than is dictated by their compensation maximizing allocation when start-up and terminal conditions are not ignored.

[FIGURES 8-12 HERE]

The observed data fit our predictions least well in Experiment 4 in which we tested our alternative plan with $\lambda = .08$. While our results under hypothesis 3b indicate that our alternative plan did modify subjects' the behavior in the right direction. It does

FIGURES 8-12

Figure 8: Optimal and Mean Observed Allocations Experiment 1 (20th Iteration)

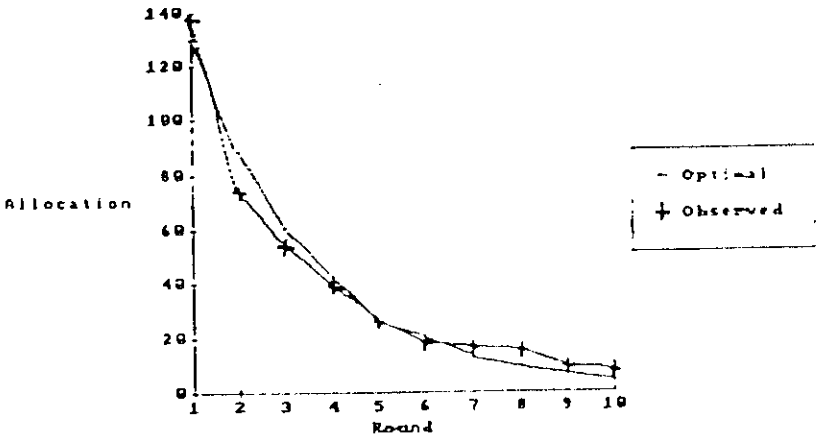


Figure 9: Optimal and Mean Observed Allocations Experiment 2 (20th Iteration) Complete Problem

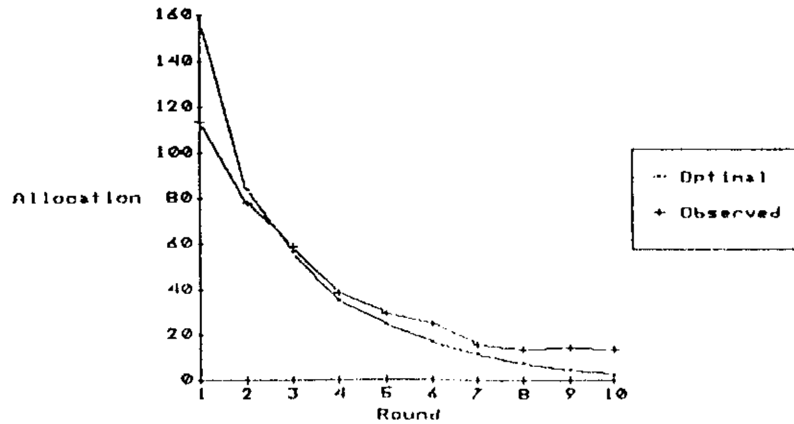


Figure 10: Optimal and Mean Observed Allocations Experiment 2 (20th Iteration) Steady State Problem

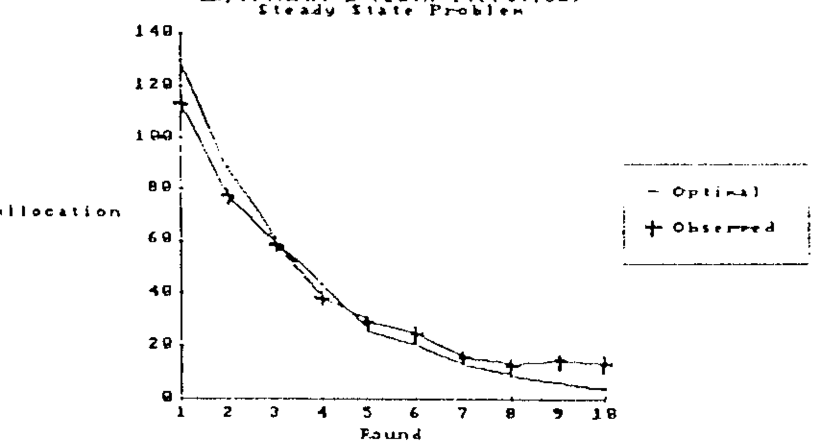


Figure 11: Optimal and Mean Observed Allocations Experiment 3 (20th Iteration)

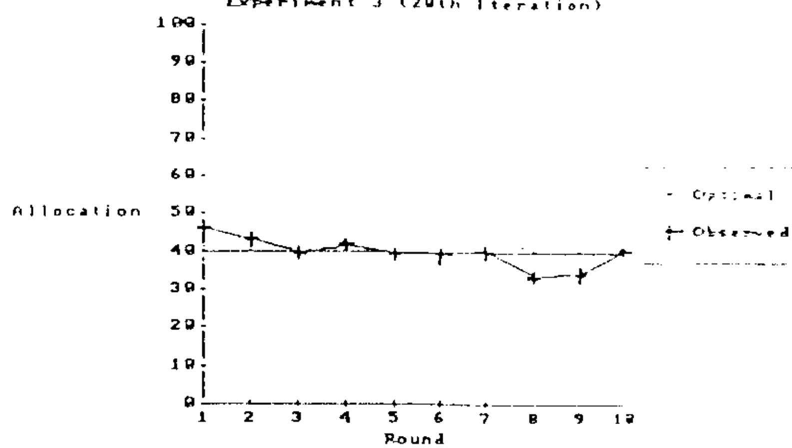
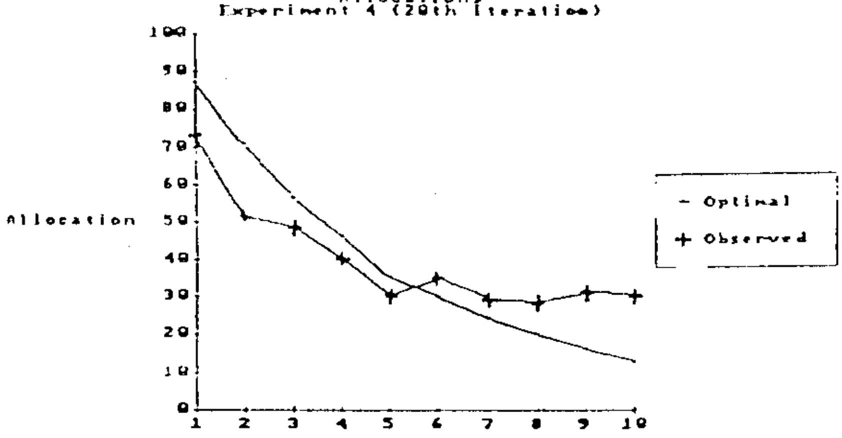


Figure 12: Optimal and Mean Observed Allocations Experiment 4 (20th Iteration)



appear that the fit of our observations to predictions was less impressive than in other experiments.

While the mean behavior of subjects did tend to mimic the predictions of the maximization problems underlying our laboratory incentive programs, there was considerable variance in behavior across subjects and a tendency for the median behavior to fall below the mean behavior observed. In Table 7 we present some snapshots of this behavior by presenting the mean, median and variance of subject behavior in the first, tenth, and twentieth iteration of our four experiments.

Table 7

Mean, Median, and Variance of Allocations

Round by Round in Iterations 1, 10, and 20

Experiment 1

Iteration 1

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	64	74	66	37	35	33	27	28	19	12
Median	59	80	57	40	37	32	25	20	20	7
Var–iance	869	683	693	137	111	155	75	655	142	177

Iteration 10

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	117	59	48	43	39	35	25	13	9	7
Median	90	55	50	41	40	40	22	15	7	3
Var–iance	6235	364	278	213	156	345	371	124	88	93

Iteration 20

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	137	73	54	39	26	18	16	15	9	8
Median	100	77	54	40	20	17	17	17	4	0.5
Var–iance	8385	964	685	443	309	249	190	220	139	1

Experiment 2

Iteration 1

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	87	57	43	41	37	37	27	27	22	20
Median	62	55	43	45	40	37	30	30	25	20
Var–	3213	520	234	202	193	187	206	226	228	228

iance

Iteration 10

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	102	75	59	45	27	24	17	18	13	16
Median	97	70	50	40	30	25	15	15	5	8
Var– iance	171	881	651	677	353	253	270	292	237	323

Iteration 20

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	113	77	59	38	29	24	15	13	14	13
Median	100	70	59	30	27	25	15	5	15	10
Var– iance	3215	1770	829	688	586	552	226	215	205	202

Experiment 3

Iteration 1

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	29	42	39	33	42	38	47	41	44	40
Median	37	40	40	40	40	40	40	40	40	40
Var– iance	213	1588	379	135	454	467	473	400	568	645

Iteration 10

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	69	42	33	42	37	37	38	29	30	36
Median	40	40	40	40	40	40	40	35	40	40
Var– iance	6905	538	240	635	523	386	479	678	394	821

Iteration 20

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	46	43	40	41	39	39	40	34	34	40
Median	40	40	40	40	39	40	40	37	35	40
Var– iance	424	198	284	155	277	350	694	282	277	923

Experiment 4

Iteration 1

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	65	55	42	42	34	37	35	29	28	28
Median	40	45	40	40	32	37	40	24	30	25
Var– iance	3155	1961	453	439	425	560	505	528	307	493

Iteration 10

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	78	59	51	37	33	29	28	25	27	30
Median	65	60	42	40	30	30	30	30	30	30
Var– iance	4200	549	1757	400	313	246	329	327	383	864

Iteration 20

Rounds	1	2	3	4	5	6	7	8	9	10
Mean	73	52	48	40	30	35	29	28	31	30
Median	70	45	45	40	30	36	30	30	33	30
Var–iance	780	1408	288	106	127	114	181	106	155	334

As we can see, the variance across subjects was considerable and showed no tendency to decrease as the experiment progressed.

Hypothesis 2

Hypothesis 2 tests for a significant difference between the behavior of subjects facing a short term bonus program only (Experiment 1) and those facing Corporation X's compensation plan,a combination of short and long term programs (Experiment 2). Our null hypothesis is that there is no significant difference. Figure 13 compares the observed round–by–round mean allocations in the 20th iteration of Experiments 1 and 2. As we can see, the differences between the allocations of subjects across these two experimental conditions is small. To test this hypothesis, we performed a Wilcoxon–Mann–Whitney–U test on the round–by–round data of the 20th iteration of each experiment. What we investigate is whether round by round there was any significant differences in the allocations of subjects in Experiments 1 and 2. Table 8 presents the mean allocations observed in rounds 1–10 of the 20th iteration in these experiments as well as their differences. We also present the results of our Wilcoxon test.

[FIGURE 13 HERE]

Table 8

Round	Experiment 1	Experiment 2	Difference	Wilcoxon Statistic
1	137.5	113.6	23.9*	0.491
2	73.9	77.8	–3.9*	0.209
3	54.4	59.1	–4.7*	–0.373
4	39	38.1	0.9*	0.326
5	26.6	29.5	–2.9*	–0.234
6	18.8	24.6	–5.8*	–0.0328
7	16.8	15.5	1.3*	0.357
8	15.4	13.3	2.1*	0.460
9	9.4	14.5	–5.1*	–0.703
10	8	13.6	–5.6*	–0.808

*Not significantly different at at least the 5% level of significance.

As we can see, in no round did there appear to be a significant difference between the behavior of subjects in Experiments 1 and 2, when significance was measured at the 5% level. In short, as we suspected, the addition of a long term bonus program of the type tested in Experiment 2 does not alter the behavior of subjects away from the behavior exhibited by a set of subjects operating under only a short term bonus program.

Hypothesis 3a

Hypothesis 3a tests the effect of our alternative incentive program when the parameters are set so as to induce a zero rate of time preference on the part of the subjects. Since we have already established under Hypothesis 1 that the observed behavior of our subjects was close to its predicted level in both Experiment 1 and Experiment 3, we expect that this behavior will also pass the weaker hypotheses postulated in Hypothesis 3a. To test this hypothesis we ran a Wilcoxon–Mann–Whitney–U test comparing the allocations of subjects in the first three and last seven rounds of iteration 20 to see if the rankings predicted were statistically significant. Table 9 and Figure 14 present the mean allocations round by round for these three experiments and the results of our Wilcoxon test.

[FIGURE 14 HERE]

Table 9
Test Of Hypothesis 3a

Round	Experiment 1	Experiment 3	Difference	Wilcoxon Statistic
1	137.5	46.4+	91.1	3.98
2	73.9	43.3+	30.6	3.13
3	54.4	40+	14.4	2.15
4	39	41.6*	−2.6	−0.54
5	26.6	39.6+	−13	−1.54
6	18.8	39.3+	−20.5	−2.78
7	16.8	40.3+	−23.5	−2.94

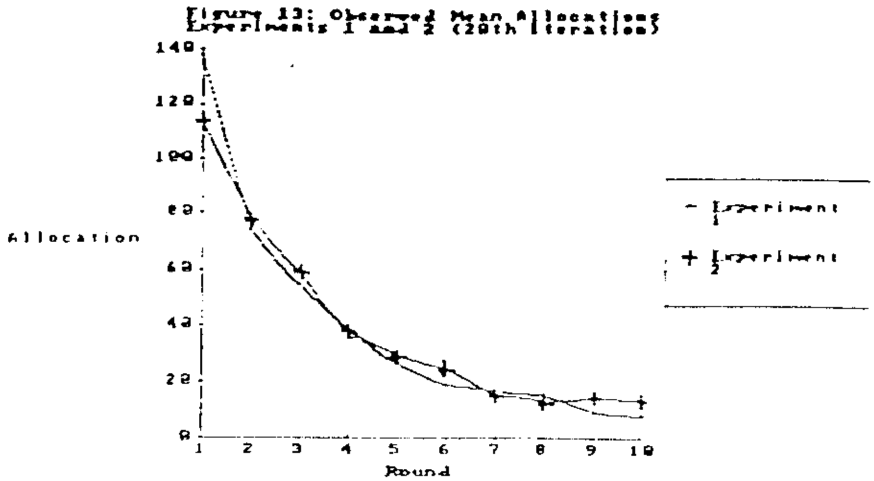


Figure 13: Observed Mean Allocations
Experiments 1 and 2 (20th Iteration)
Allocation
140
120
100
80
60
40
20
0
1 2 3 4 5 6 7 8 9 10
Round
Experiment 1
Experiment 2

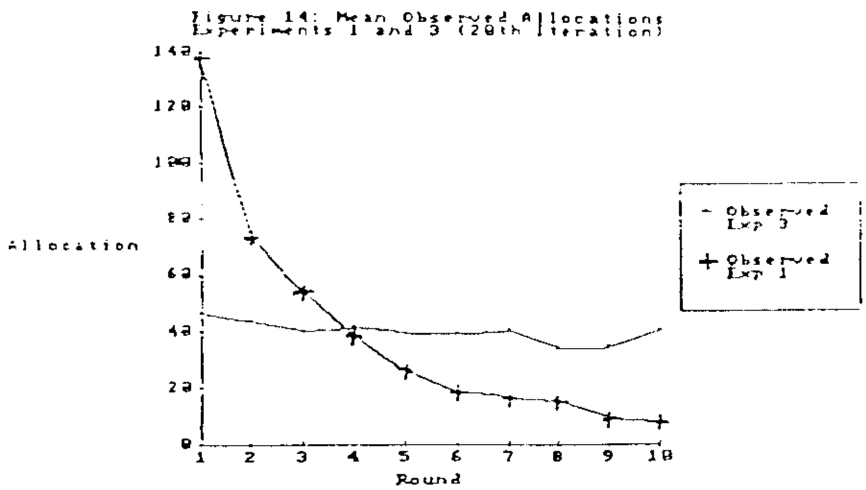


Figure 14: Mean Observed Allocations
Experiments 1 and 3 (20th Iteration)
Allocation
140
120
100
80
60
40
20
0
1 2 3 4 5 6 7 8 9 10
Round
Observed Exp 3
Observed Exp 1

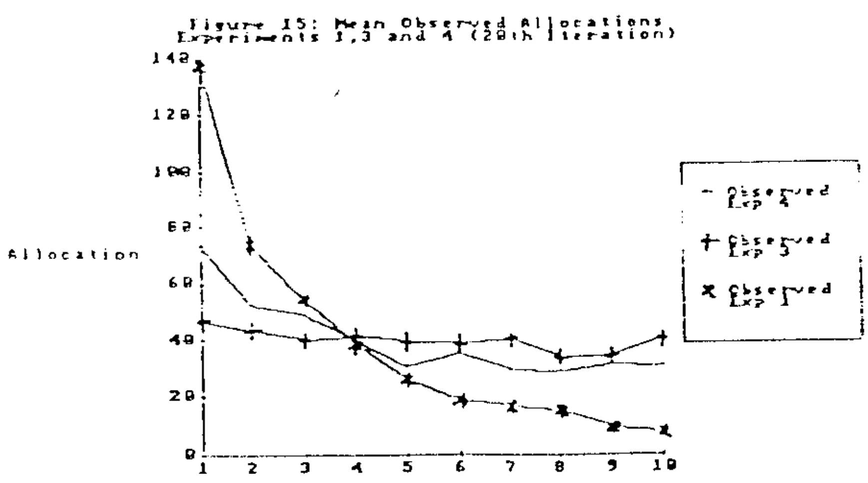


Figure 15: Mean Observed Allocations
Experiments 1,3 and 4 (20th Iteration)
Allocation
140
120
100
80
60
40
20
0
1 2 3 4 5 6 7 8 9 10
Round
Observed Exp 4
Observed Exp 3
Observed Exp 1

8	15.4	34.0+	−18.6	−2.65
9	9.4	34.5+	−25.1	−3.35
10	8	40.7+	−32.7	−3.89

*Not significantly different at the 5% level of significance

+Significantly different from zero using a 1−tailed test at the 5% level of significance.

From this table and figure we can easily see that the expected rankings were in fact realized and in a statistically significant manner. In statistical terms a Wilcoxon−Mann−Whitney test indicates a significant difference in the predicted direction between allocations in Experiments 1 and 3 in all rounds except round 4, where theory predicted they should be the same.

Hypothesis 3b

Hypothesis 3b aims to demonstrate that our alternative incentive plan changed the behavior of subjects in the hypothesized direction in a statistically significant fashion. Figure 15 below presents the observed mean allocation paths of subjects in Experiments 1, 3, and 4 during the 20^{th} iteration of these experiments. As we can see, the observed means were all ranked in the expected manner (compare this figure to Figure 7). To put this data to a statistical test, we performed two tests. One was a Kruskal−Wallis test run on the round by round data generated in the 20^{th} iteration . This test investigates whether we can reject the hypothesis that the data generated round−by−round in the 20^{th} iteration of Experiments 1, 3, and 4 came from the same populations. We also performed a set of pair−wise Wilcoxon tests on the round−by−round means between Experiments 1 and 3, 1 and 4, and 3 and 4. Our results are presented in Table 10.

[FIGURE 15 HERE]

Table 10

Kruskal–Wallis Test for Significant Difference in Behavior Between Experiments 1, 3, and 4.

Round	Test Statistic
1	17.2+
2	7.8+
3	3.0+
4	−2.8+
5	0.15*
6	5.8+
7	6.97+
8	5.7+
9	11.2+
10	11.3+

*No significant difference at the 5% level.

+Significant differences at the 5% level.

Pair–Wise Wilcoxon Tests
Experiments 1 v.s. 3, 1 v.s. 4, and 3 v.s. 4

Rd.	Experiment 1 (Mean)	Experiment 3 (Mean)	Experiment 4 (Mean)	Wilcoxon Test 1v.s.3	Wilcoxon Test 1v.s.4	Wilcoxon Test 3v.s.4
1	137.5	46.4	73	3.98+	2.49+	−2.92+
2	93.9	43.3	52	3.13+	2.28+	−1.3+
3	54.4	40	48.8	2.15+	1.12*	−1.82+
4	39	41.6	40.1	−0.54*	−0.52*	0.03*
5	26.6	39.6	30.5	−1.54+	−0.78*	1.40+
6	18.8	39.3	35.1	−2.78+	−2.43+	0.56*
7	16.8	40.3	29.7	−2.94+	−2.19+	1.40+
8	15.4	34.0	28.8	−2.65+	−2.26+	1.28+
9	9.4	34.5	31.25	−3.35+	−3.32+	0.36*
10	8	40.7	30	−3.38+	−3.14+	1.20*

* No significant difference at the 5% level.
+ Significant difference at the 5% level.

From the Kruskall–Wallis test we clearly see that except in round 5 there is a significant difference between the behavior in subjects of these three experiments . Using our pairwise tests we can see where these differences appear. For instance, there were significant differences between behavior in Experiments 1 and 3 in all rounds except round 5, between Experiments 1 and 4 in all rounds except rounds 3, 4, and 5, and between Experiments 3 and 4 in all 'rounds except 4, 6, 9, and 10.

Myopic Behavior

A main purposes of our laboratory incentive programs is to change the myopic behavior of subjects. Hence we wnated to try to investigate whether our laboratory programs had their expected impact of the "myopia" of subjects. To do this we have defined myopia, in the context of our experiments, as follows:

Definition:

A subject (or group of subjects) A is (are) more myopic than a subject (or group of subjects) B if

$F_A(t) > F_B(t)$, for round t = 1,2,.....,10, where:

$F_A(t)$ and $F_B(t)$ are the cumulative amounts of units (cumulative mean amount of units) allocated by subjects (groups) A and B respectively by round t.

Hence, if one subject or group of subjects have a cumulative distribution of units which is everywhere above the cumulative distribution of another subject or groups of subjects, we will say they have revealed themselves to be more myopic.

From our discussion above, if we call M_i our myopia measure for Experiment i, i = 1,2,3,4, then, given our experimental treatments we would expect that in terms of myopia, $M_1 \sim M_2 > M_4 > M_3$. To investigate this conjecture we have plotted in Figure 16 the cumulative distributions of the mean allocations in the 20th iteration of Experiments 1, 2, 3,and 4.

[FIGURE 16 HERE]

FIGURES 16-20

Figure 16: Cumulative Mean Allocations
Experiments 1,2,3 and 4 (20th Iteration)

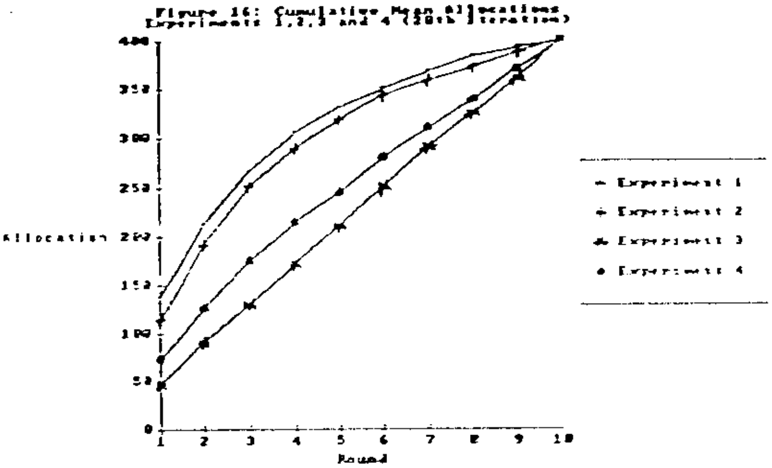


Figure 17: Cumulative Mean Allocations
Experiment 1
1st, 10th and 20th Iterations

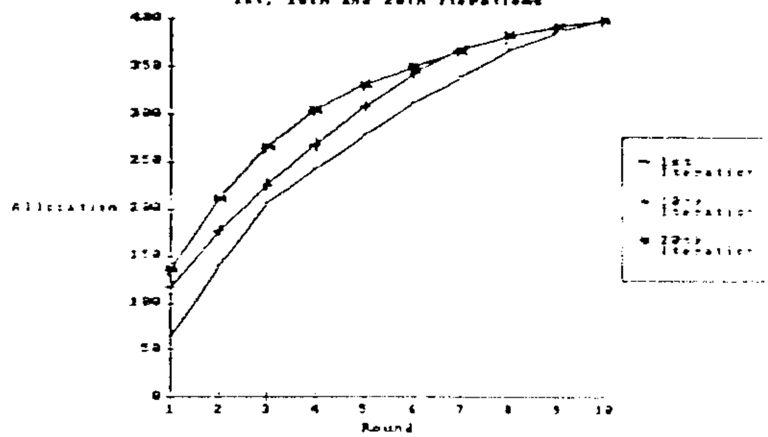


Figure 18: Cumulative Mean Allocations
Experiment 2
1st, 10th and 20th Iterations

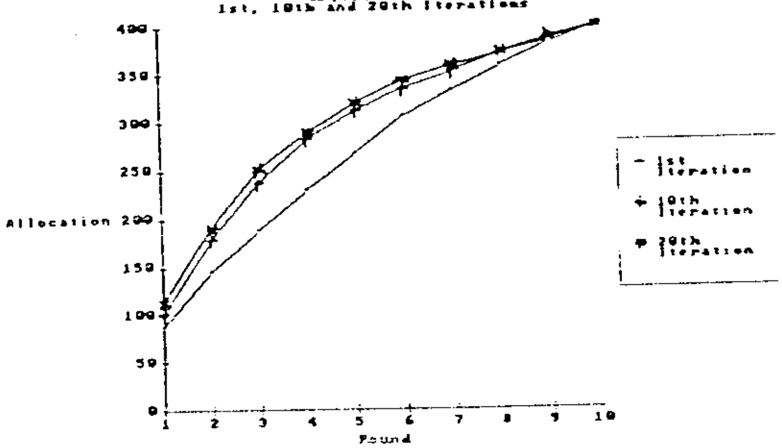


Figure 19: Cumulative Mean Allocations
Experiment 3
1st, 10th and 20th Iterations

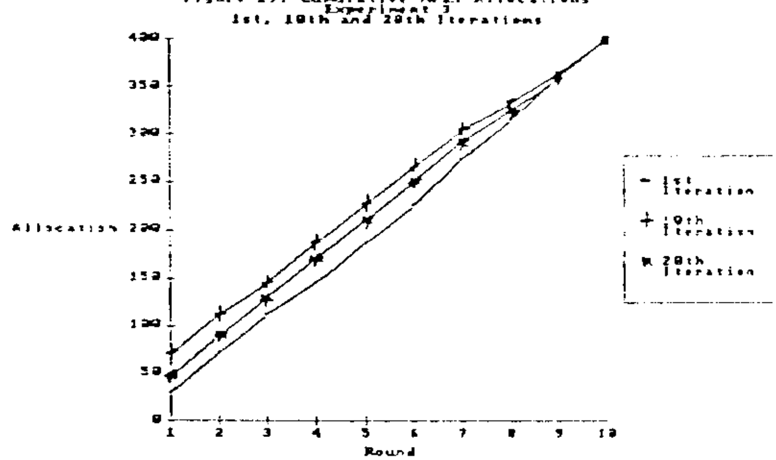
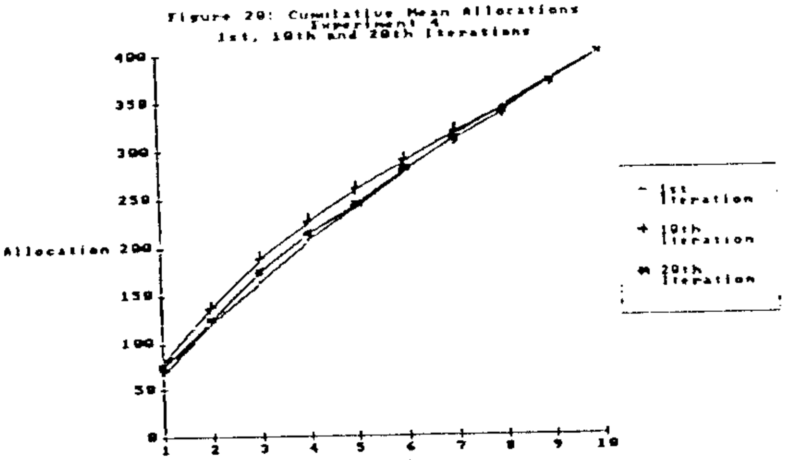


Figure 20: Cumulative Mean Allocations
Experiment 4
1st, 10th and 20th Iterations



As we can see, according to our definition of myopia, we would call the subject behavior in Experiment 1 most myopic, followed closely by the behavior of subjects in Experiment 2 and then by subjects in Experiments 4 and 3. These myopia rankings are what we had predicted.

Learning:

One final question that remains is how is it that subjects learn from their experience with the laboratory incentive program they functioned under. To investigate this question, we present in Figures 17 through 20 the mean cumulative unit distributions for our subjects in each experiment in the 1^{st}, 10^{th}, and 20^{th}, iterations of our experiments.

[FIGURES 17–20 HERE]

These figures present something interesting. In Experiments 1 and 2, subject experience make them more myopic over time. By this we mean that in these two experiments, the cumulative unit distributions of the 1^{st} round is everywhere below that of the 10^{th} round, which is everywhere below that of the 20^{th} round. Hence, by our definition we would say that experience with these particular laboratory incentive programs teaches subjects to be consistently more myopic and that inexperienced subjects, new hires to the corporation, tend to be more far sighted.

In Experiments 3 and 4 this pattern does not hold. Here we see that while subjects may start out with an initial level of myopia, this myopia tends to increase by round 10 and then decrease by round 20 . Here, while inexperienced (round 1) subjects are least myopic, subjects with medium amounts of experience tend to be the most myopic. Subjects with maximum experience tend to occupy an intermediate position.

Section 6: Conclusions

This paper has attempted to investigate the long–term bonus program of Corporation X (a Fortune 500 corporation) by replicating its long and short–term bonus programs on a set of sixty two graduate business students. Our aim was to investigate whether the long–term bonus program offered by this corporation is successful in reconciling the divergent time preferences of the corporation's managers and it stockholders. On the basis of our results it appears that any long–term bonus program that does not affect the trade–offs that executives face between current and future profits will not change their behavior away from that observed when the corporation offers only a short–term bonus program paying bonuses strictly on the basis of current performance. This conclusion was reached by replicating the short and short–plus–long–term bonus programs of Corporation X in a laboratory with paid student subjects and testing for significant differences in the behavior of our subjects across these experiments. No significant differences were found. However, when an alternative incentive program was tested which did present executives with a new trade–off between the value of current and future corporate profits, we did observe significant changes in the subjects behavior. These results imply that subjects are capabable of understanding various laboratory versions of incentive programs and when presented with them act as if they were maximizing their expected discounted earnings. This finding offers hope for the resolution of the preferecne divergence problem that motivated this paper since if executives maximize their discounted compensation through their choice of corporate decisions, then these decisions can be altered by offering the executives an incentive program that makes it in their interest to act as if they were maximizing the earnings of stockholders. In an attempt to demonstrate this point we offered two versions of one such alternative plan which appeared to induce predicted behavior quite accurately. Whether these alternatives are feasible for Corporation X or any other corporation is not a question we wish to answer here. However, it does appear that similar, possibly more acceptable, plans could and perhaps should be seriously considered.

REFERENCES

1) Bennett, Amanda,"Executives Will Gain Over Time As Lucrative Stock Plans Multiply",Wall Street Journal, 4/10/87.

2) Rock, Milton, Handbook of Wage and Salary Administration, 2nd Edition,Mc Graw Hill,New York 1984

3) Reibstein, Larry, "More Employers Link Incentives to Unit Results", Wall Street Journal,4/10/87.

4) Ross, S., "The Economic Theory of Agency", Amercian Economic Review,Vol. 63, 1973, pp. 134–139.

5) Shavell, S., " Risk Sharing and Incentives in the Principal and Agent Relationship", The Bell Journal of Economics, Vol. 10, Sp. 1979, pp. 55–74.