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Essays on Behavioral Labor Economics

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ESSAYS ON BEHAVIORAL LABOR ECONOMICS

A Dissertation Presented

by

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To my parents

and

the memory of Judy Dietel

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ABSTRACT

ESSAYS ON BEHAVIORAL ECONOMICS

SEPTEMBER 2010

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Overview

Economists typically understand the firm as an organization comprised of a series of incomplete contracts among input suppliers (e.g. Coase, (1937), Williamson, (1985)). The ultimate right to make decisions that are not subject to a pre-existing contractual arrangement – hereafter referred to as *decision-control rights*, are assigned to some person or group associated with the enterprise. The entity with decision-control rights has the final say over how to organize essential firm operations that range from the determination of production techniques, to deciding how to monitor or compensate the firm's members.[1] To

[1] As an example, if management is held accountable by all of the laborers (or shareholders as the case

the extent that firm members have competing interests or are asymmetrically affected by such decisions, those members with decision-control rights may be confronted with important normative issues regarding which firm objectives should be pursued. In my dissertation, I employ a behavioral economic perspective in order to examine how workplace governance practices interact with both the level of satisfaction and motivation of workers.

In the first essay of the dissertation, I collected data from a real-effort experiment to compare changes in the performance of research participants that were subjected to an identical set of wage incentives that were either implemented (1) endogenously by the group to which subjects belong through a simple majority vote, (2) endogenously by only one member of the group who had all decision-control rights, or (3) a random process completely exogenous to the group. The 3 (3 distinct decision-control rights regimes) X 2 (2 distinct incentive contracts) between-subjects design allows for a clean comparison of performance under different decision-control rights treatments. I report evidence suggesting that the decision-control rights arrangement used to select the compensation contract can significantly influence the subsequent level of performance of research subjects.

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may be) in the firm insofar that they can override or replace management of the firm, then the decision-control rights are ultimately held by labor and not the management.

The second essay (co-authored with Michael Carr), analyzes the relative effects of voice, autonomy, and wages in explaining job satisfaction using subjective evaluations of work conditions and satisfaction recorded in the 2004 wave of the Workplace Employment Relations Survey (WERS). We show that the amount of autonomy and voice that a worker has over the firm is an important omitted variable, biasing the estimated coefficient on the wage upwards. And, conditional upon having a job, voice and autonomy are considerably more important determinants of job satisfaction than the wage.

The final essay offers a critique of the traditional economics of work organization in consideration of the literature developed in behavioral and experimental economics. I argue that many models of worker motivation developed using the rational choice model (RCM) carry the cost of ignoring common sentiments and behaviors that have been systematically demonstrated in experimental studies. After providing an extensive review of the experimental economics literature as it may inform various workplace organizational faculties, I conclude that the literature suggests that establishment of work teams and incentive schemes that reward teams for collective success would carry the expectation of sustained satisfaction and productivity of workers more than firm environments that rely on employee competition as a motivational device.

TABLE OF CONTENTS

Page

LIST OF TABLES

Table **Page**

LIST OF FIGURES

Figure **Page**

CHAPTER 1

INTRODUCTION

Economists typically understand the firm as an organization comprised of a series of incomplete contracts among input suppliers (e.g. Coase, (1937), Williamson, (1985)). The ultimate right to make decisions that are not subject to a preexisting contractual arrangement – hereafter referred to as *decision-control rights*, are assigned to some person or group associated with the enterprise. The entity with decision-control rights will have the final say over how to organize essential firm operations that range from the determination of production techniques, to deciding how to monitor or compensate the employees.[1] To the extent that different firm members have competing interests or are asymmetrically affected by such decisions, those members with decision-control rights may be confronted with important normative issues regarding which firm objectives should be pursued. For example, it may be the case that policies that pursue firm profits for shareholders would be distinct from those that maximize net-income per worker (Vanek, 1970), or similarly, policies intended to promote higher short term growth rates might be in conflict with various "quality of work-life" goals that would otherwise be pursued by workers with control

[1] As an example, if management is held accountable by all of the laborers (or shareholders as the case may be) in the firm insofar that they can override or replace management of the firm, then the decision-control rights are ultimately held by labor and not the management.

rights. Of course, different decision-control rights distributions (e.g. board of directors consisting of shareholders, labor-managed firms, employee representation, co-governance etc.) can be expected to arise in practice depending on various factors including the willingness for firm members without decision-control rights to consent to those with decision-control rights, the legal restrictions outlined by local or national governments, and the procedural norms common among firm members.

Investment that expands the capital stock or towards technological development generally lead to higher levels of firm efficiency, but productivity is not determined exclusively by technologies and small changes in the productivity of labor could potentially have a large impact. Thus the viability of a firm in a competitive economy is contingent upon its ability to sustain sufficiently high levels of labor productivity. The dissertation investigates the interaction between the organizational and decision-making processes and productivity of labor. This question has received a fair amount of attention by economists over the years (e.g. Alchien and Demsetz, (1972); Holmstrom, (1982); Levin and Tyson, (1990)), but the existing literature has at least three significant shortcomings.

First, the vast majority of the theoretical analysis in labor and personnel

economics is based on the rational choice model (RCM) where all agents rationally optimize their private material gains across all social contexts. The study of price incentives in the RCM has produced valuable models of incentives, compensation, and job search. These developments, however, have come at the cost of ignoring common behaviors and psychological tendencies such as loss aversion, guilt, procrastination, envy, consent to authority, happiness, altruism, or solidarity. Those aspects of behavior, which are excluded from the RCM, surely influence workplace performance. The investigation of these issues, however, has received surprisingly little attention in Economics; including Behavioral Economics.

Second, empirical work that investigates how different decision-control rights arrangements affect firm performance face difficult challenges. It is hard to adequately control for firm idiosyncrasies in production and organization, the level of industrial concentration, the presence or absence of price-controls or subsidies, state-specific tax structures, and general labor market conditions, when investigating a specific behavioral relationship. These difficulties suggest that controlled lab experiments might provide a testing ground to evaluate behavior under various organizational structures. Although experiments can only claim to be stylized representations of real-world economies, experiments

often reveal systematic behavioral tendencies typically not accounted for by standard assumptions. Despite the many empirical studies that have sought to investigate how the distribution of decision-control rights affects productivity, to the best of my knowledge, none have used the experimental methodology to test this directly.

Finally, while research in neighboring behavioral sciences has advanced our collective knowledge of human motivation, satisfaction, and other important job attitudes, economists have generally not operated within this interdisciplinary space. For example, in dealing with the canonical 'agency problem,' the economic perspective has primarily focused on how best to use monetary incentives to align the interests of managers and capital owners. It is likely, however, that additional psychological, social, or political factors influence principal-agent outcomes. The large body of work dedicated to investigating these issues in Organizational Behavior, Management Studies, and Industrial Psychology, are only cross-referenced by Economists in exceptional cases. While it is certainly a challenge to organize and translate concepts across disciplines in a systematic and coherent manner, the potential benefits from interdisciplinary study are large.

In the dissertation, I contribute to filling these deficiencies. In the first

essay, I collected data from a real-effort experiment to compare changes in the performance of research participants that were subjected to an identical set of wage incentives that were either implemented (1) endogenously by the group to which subjects belong through a simple majority vote, (2) endogenously by only one member of the group who had all decision-control rights, or (3) a random process completely exogenous to the group. I compare performance across treatments (between-subjects design) and report evidence suggesting that the decision-control rights arrangement used to select the compensation contract can significantly influence the subsequent level of performance of research subjects.

The second essay conducts an econometric exercise that analyzes the relative effects of voice, autonomy, and wages in explaining job satisfaction using subjective evaluations of work conditions and satisfaction recorded in the 2004 wave of the Workplace Employment Relations Survey (WERS). We show that the variability of employee satisfaction is driven by subjective evaluation scores of worker autonomy and voice. Our findings suggest that while wages may be an important determinant of job choice, it is not the most important determinant of job satisfaction, particularly when compared to autonomy and employee voice.

The final essay reviews experimental economic work that has aimed to test personnel models of incentive contracts, the role of general labor market

conditions, loyalty, job satisfaction, intrinsic motivation, and the effect of different firm governance structures. Because the firm is partly a kind of marketplace -- with institutions that support specific contractual exchange relations -- and partly a place where people interact expressively as whole personalities, economists need to go beyond the RCM to understand the effects of organizational structures on the productivity and motivation of workers. The review shows that a substantial gap remains between the work in industrial psychology and that done within economics on examining the motivational effects of different workplace governance practices and asset ownership distributions. Experimental methodologies, however, offer a method to test several behavioral claims made outside of economics, and also facilitate their inclusion into economic practice.

CHAPTER 2

AN EXPERIMENTAL INVESTIGATION OF PERFORMANCE UNDER HIERARCHICAL AND PARTICIPATORY DECISION-MAKING PROCEDURES

2.1 Introduction

Firms constantly make decisions that affect the conditions of work for their employees. The economics literature shows strong disagreements over both *how* such decisions should be reached, and *which* firm members should be included in making these decisions. The most common model of firm governance employs a centralized decision-making authority, but participatory models that delegate decision-making responsibilities to workers can be regularly observed. For example, Kruse et al. (2008) find that up to 40% of employees in the U.S. report having a lot of influence on firm decisions or say they often participate with other workers in job decisions. Historically, the push for "employee empowerment initiatives" and similar programs have often been seen as responding to general concerns of fairness, job satisfaction, workplace trust, and perhaps most importantly for economists, as promoting the productivity of workers (e.g. Freeman and Rogers (2006), Dow, (2003)).

While scores of studies have sought to compare how different firm-governance structures help or hinder the attainment of firm goals, it has proven difficult to resolve whether the decision-making architecture of the firm is associated with a systematic behavioral effect on the motivation and subsequent performance of workers.[2] Much of the ambiguity in the empirical literature may be due to the difficulty of controlling for confounding factors such as distinct production technologies, market conditions, monitoring structures, and compensation methods that complicate identification of worker performance. Another hurdle that labor economists are traditionally confronted with is how to control for employee ability when evaluating how a policy or governance structure affects worker performance. Self-reports, attitudinal surveys, IQ tests, years of education, quality of education, resume quality, years of job experience, and managerial evaluations have been used in many labor studies to approximate unobservable worker characteristics like skill and intrinsic motivation. These proxy measures can be noisy and this may also contribute to mixed findings in the literature.

In an effort to mitigate the confounding issues, this paper uses a controlled

[2] The meta-analysis conducted by Levin and Tyson (1990) analyzing the effects of participation on productivity conclude that, "Our overall assessment of the empirical literature from economics, industrial relations, organizational behavior and other social sciences is that participation *usually* leads to small, short-run improvements in performance and *sometimes* leads to significant, long-lasting improvements in performance" (emphasis in original).

laboratory experiment to investigate whether the performance of workers is sensitive to the provision of decision-control rights over a meaningful decision – the determination of workers' compensation structure.

We collected data from a real-effort experiment to compare changes in the performance of research participants subjected to an *identical set* of wage incentives that were either implemented (1) endogenously by the group to which subjects belong through a simple majority vote, (2) endogenously by only one member of the group who had all decision-control rights, or (3) a random process completely exogenous to the group. By design, we control for all issues that pertain to monitoring, punishment, threats, or other forms of coercion that might also accompany many types of systems of control in real-world firms.

The data collected in our experiment limit our investigation to an examination of observed behavioral differences that arise under different decision-making processes for group decisions. We further strip down the effort task so that it is not reliant on team production technologies to minimize confounds that could arise in social dilemmas (i.e. trust, reciprocity, reputation etc) and restrict the menu of potential compensation schemes, to two where all claims on residual profits are held by labor. Our real-effort task additionally allows us to collect measures of both *effort* (trying hard) and *effective effort*

(quality of work).

We use a 3 (3 distinct decision-control rights regimes) X 2 (2 distinct incentive contracts) between-subjects design in order to have an apples-to-apples comparison of performance under different decision-control rights treatments. All subjects in each session of our experiment participated in 3 periods. In Period 1, all subjects were paid a flat-wage to solve simple addition problems for 5 minutes. The instructions in Period 1 made clear that all subjects would receive a flat-wage that was independent of their effort in Period 1. In Period 2 of the experiment, all subjects were randomly and anonymously put into groups of 3, and were told that they would be again be solving problems for 5 minutes, but that the method by which they would be compensated for their performance in Period 2 was contingent upon the implementation of one of two possible incentive contracts – either a rank-order tournament with endogenous prize creation (referred to as CS1 in the experiment instructions), or a group revenue-sharing contract (referred to as CS2 in the experiment instructions). Depending on the treatment that subjects were in, the decision over which compensation scheme would be implemented for the duration of Period 2 was either made 1) endogenously by vote where all 3 group members had equitable decision-control rights (hereafter referred to as the *Voting* treatment), 2) endogenously by a single

group member determined at random with all decision-control rights (*Authority* treatment), or 3) exogenously by the computer (*Control* treatment). Period 3 in our experiment was identical to Period 1, where subjects were again paid a flat-wage for 5-minutes to solve addition problems.

We report evidence suggesting that the performance of subjects in our experiment is sensitive to the decision-control rights arrangement used to select the compensation contract. Consistent with intuition, allowing groups of workers to participate in determining the compensation scheme for their group increases effort significantly. While this may not be surprising, ours is the first study to confirm the intuitive result for group level decisions. Specifically, we find that both endogenous decision-making arrangements in our study result in significantly higher performance -- measured both in total effort provided, and effective effort-- than decisions made by a random process completely exogenous to the group. Further, these effects persist even after controlling for gender, compensation scheme, expressed preferences for a compensation scheme, and ability.

2.2 Related Research

Labor costs account for the vast majority of production costs, implying that small increases in effort per worker can have a significant influence on output production (Blinder (1990)), but the level of effort that workers provide cannot be directly contracted (Bowles and Gintis, (1988)). This familiar agency dilemma has spawned a great deal of research into the potential motivational properties of different compensation schemes (e.g. Stiglitz, (1975), Lazear and Rosen, (1981)), monitoring structures (e.g. Alchien and Demzetz (1972), general labor market conditions (e.g. Shapiro and Stiglitz, (1984)) and other 'strategic human resource management' designs that are developed with the intention of influencing the level of effort offered by workers.[3] The vast majority of the theoretical work in this tradition is rooted in identifying incentives that would appeal to the rational actor, yet personnel issues like worker motivation can be much more complicated and for this reason, its analysis will likely require a *behavioral* perspective. Berg (2006) points out that in a field like labor economics that is so heavily data driven, the incorporation of experiments as a research methodology has been relatively limited.[4]

[3] See Prendergast (1999) for excellent survey on theory and practice of incentive mechanisms and Baron and Kreps (1999) for an introduction to contemporary themes in personnel management.

[4] For a general overview on the use of experiments in labor market experiments see Falk and Fehr (2003). For reviews of findings, see Mellizo (2010) and Charness and Kuhn (2010).

The use of experiments to investigate labor and personnel matters, however, has already contributed much to the field of study. For example, experiments have 'cleanly' revealed that incentive structures that should elicit equivalent effort from rational and selfish agents, influence humans in systematically different ways. For example, Bull et al. (1987) compare the efficacy of tournaments and piece-rates in a chosen-effort lab experiment, finding that mean effort levels under either compensation scheme were roughly equivalent, but further found that a much larger variance was observed within all of their tournament specifications, than under the piece-rate scheme. In a similar vein, van Dijk et al. (2001) use a real-effort experiment finding that individual and team payment schemes induced similar effort levels, while Freeman and Gelber (2010) observe that output for groups under tournament contracts that result in moderate levels of inequality was significantly higher than when payment was either independent of the participants' performance, or when the level of inequality resulting from the tournament prize was high. Of course, by now behavioral findings that are inconsistent with predictions of the rational actor model no longer come as a surprise to economists, but much more importantly, the use of experiments has shown that in many cases both non-rational, and non-selfish behavioral deviations from the standard theoretical

predictions are systematic, common, and replicable (e.g. Rabin, 1998). Moreover, the expanded use of the experimental method has helped in revealing preferences that reflect the norms and values supported by different social environments (e.g. Henrich et al. (2001))

To illustrate the last point as it relates to workplace organization, many firms are structured to encourage competition among their employees, yet it has been reported that competitive institutions might bias performance outcomes for workers. For example, Gneezy, Niederle, and Rustichini (2003) find that one source of systematic variation in performance of solving mazes can be explained by gender, where the performance of women is less responsive to competitive environments than it is for men in their sample. Similarly, Niederle and Vesterlund (2007) find evidence suggestive of gendered attitudes towards competition-- that is, given the choice to enter or not enter a competitive environment, many women would prefer not to, even when it would be in their monetary interest to do so. They additionally find, however, that men often elect to enter into competitive situations far more often than they should. While the experimental economics literature has only begun to explore *why* there are gendered attitudes towards competition, findings from a very interesting field experiment conducted by Gneezy, Leonard, and List (2006) suggest that

processes of socialization play a central role in shaping preferences towards competition. They find that women in matriarchal societies are more likely to demonstrate a preference to compete than men, while the reverse was true in patriarchal societies. To the extent that these findings are externally valid to firm environments in patriarchal societies, they may contribute to an explanation for persistent asymmetries common in labor market outcomes, such as the large discrepancies between the proportion of males and females in competitive professions.[5] Other studies, however, have shown that if subjects are allowed to sort themselves into (or away from) a particular wage-structure that the outcome typically results in higher average levels in performance (e.g. Cadsby et al. (2005); Eriksson et al. (2006); Teyssiar (2008); Dohmen and Falk (2006)).

While it is certainly the case that some workers study the institutional features of the firm prior to entry, we analyze the situation where firms change working conditions for existing employees. Our study extends the literature by using the lab to explore how the distribution of control rights over a firm-level decision determining the compensation scheme for workers influences worker performance. Dal Bo et al. (2007) and Sutter et al. (2006) suggest that democratic processes over the selection of group-level institutions can affect the level of

[5] See Croson and Gneezy (2009) for an excellent review of the experimental economics literature highlighting gendered differences in attitudes and behavior and their potential impacts on labor market outcomes.

cooperation observed in *social dilemmas*, but we believe ours to be the first experimental study to directly examine the impact of different decision making processes on worker performance.

2.3 Experiment Design

In the Fall term of 2008, we recruited members of the University of Massachusetts-Amherst community via table-tents, fliers, and brief announcements given in lecture halls. The participants in our study were not informed about the precise nature of the experiment before arrival. In our recruitment, we promised a $5.00 show-up fee, plus the chance to earn additional money in the experiment. Our recruitment procedure allowed subjects to self-select into a time-slot that was convenient for their own schedule, and we confirmed their participation by email. We conducted 21 sessions over a three-week period. Upon arriving at the experiment, a subject's experience in all treatments followed the following protocol. First all subjects signed a consent form for their participation and were seated at a computer terminal where they found a sheet introducing the study to them and a copy of instructions for Period 1 of the experiment. After all subjects were seated, a lab assistant read both the

introduction of the experiment, as well as the set of instructions for Period 1. All subjects in all sessions received the same instructions for Period 1.

In Period 1 of the experiment, all subjects were paid a flat-wage contract of 75 Experimental Units (30EUs = 1USD) for the task of adding together different sets of five two-digit numbers that appeared on their computer screen. Participants were not allowed to use a calculator, but could use scratch paper and a pencil that were provided to them. After solving a problem, a subject would submit their answer and would be presented with a new problem to solve. The numbers to be added together were randomly generated, but all subjects in a given treatment were presented with the same set of math problems, given in the same order. We chose to use this specific real-effort because (1) we would expect adding sets of numbers together to yield a low intrinsic reward, (2) it requires little skill, (3) a typical college aged subject is familiar with simple arithmetic, and most importantly, (4) previous studies have found that this same real-effort task did not result in biased performance in any systematic manner (Niederle and Vesterlund, (2007)).[6] When the 5 minutes of Period 1 were over, subjects were

[6] The program was written using zTree (Fischbacher 2007). We gratefully acknowledge Muriel Niederle and Lise Vesterlund for sharing their zTree code used in Niederle and Vesterlund (2007) for the production task of adding up sets of 5, 2-digit numbers. The only difference between our instrument and the one used in Niederle and Vesterlund's piece is that in our experiment all subjects receive the same set of randomly generated math problems in the same order, where in the former paper each subject receives a new, randomly generated set of numbers in each math problem. We made this adjustment in response to feedback surveys in our pilot runs where some subjects (unexpectedly) made the claim that they were concerned with the set of numbers that they might have to solve compared with other subjects. That is, some subjects wanted to be sure that they would be solving the same problems

presented with a screen that displayed how many problems they correctly solved as well as a reminder of their earnings in Period 1 (a flat-wage of 75EUs). Subjects were not given any information regarding the performances of any other subjects at this time. At this point, the subjects received a new set of instructions for Period 2 of the experiment.

At the beginning of Period 2 of the experiment, subjects were informed that they had randomly and anonymously been assigned to a group of 3 total subjects. Group members were connected only through the computer network, and for the remainder of the experiment the subjects never knew who was in their group. In the instructions for Period 2, subjects received a message indicating to them that one of two possible compensation schemes would be implemented and that the scheme would affect the payout of all members of their group. Both of the possible compensation schemes were simple incentive contracts where each correct answer from each member of the group was equal to the exogenously determined price of 10EUs. In Period 2, all of the correct answers from all members of each group were summed together and multiplied by 10EU to create the total proceeds to the group. The only distinction between the compensation contracts in Period 2 was the manner by which the total

as everyone else. To eliminate the potential for any noise arise in our data due to this concern, we made this minor adjustment to the original z-tree code used in Niederle and Vesterlund (2007).

proceeds were distributed back to the members within each group. Under the tournament scheme, the person with the highest number of contributions to the group total was to receive 60% of all of the group proceeds, the second highest performer would receive 30% of the proceeds, and the third highest performer would receive 10% of the proceeds. All ties were broken at random. Under the revenue-sharing contract, the total proceeds were simply multiplied by a factor of 1/n so all members within the group would receive an equal share of the group proceeds. The instructions also described *how* the decision to implement either compensation scheme would be made. In each session only one treatment (i.e. one decision-making process) was implemented. Subjects had no prior knowledge of which treatments would be run in a given session. The three decision-treatments are described below.

2.3.1 Decision-Treatments

Voting Treatment: In the Voting Treatment, all members in the group had equal decision-making rights in either implementing the tournament or the revenue-sharing contract. The specific procedure used was a simple voting election where only a simple majority was required to

implement either compensation scheme.

Authority Treatment: In the Authority Treatment, all members in the group were informed that one of the three members would decide whether to implement the tournament or the revenue-sharing contract. Subjects in this treatment were informed that the decision maker would be determined randomly by the computer.

Control Treatment: In the Control Treatment, all members in the group were informed that the implementation of either the tournament or revenue sharing contract would be done at random by the computer program.

Before the 5-minute period of solving math problems began in Period 2, all subjects were informed of the outcome from the decision-making process regarding how they would be compensated for their efforts for the duration of Period 2. At the end of the 5-minutes period, all subjects were presented with a screen indicating to them how many correct answers they provided, the group total of correct answers, their individual payoff, and their relative rank within

their group of three. At this point, all subjects received a set of instructions for Period 3 of the experiment.

Period 3 was identical to Period 1 with the exception of the math problems presented. Like Period 1, subjects in Period 3 were compensated a flat-wage of 75 EUs to solve simple addition problems for 5 minutes. At the end of Period 3, all subjects were again presented with a summary screen indicating how many correct answers they had in Period 3, along with a reminder of their compensation in Period 3.

We collected Period 3 data for two main reasons. First, like Period 1 data, they provide an additional observation of ability and/or the willingness for subjects to provide effort in the absence of extrinsic incentives. We also collect Period 3 data to conduct an auxiliary analysis in section 2.4.3 examining if Period 3 effort could be explained by different experiment conditions.

After Period 3 a brief survey was administered through the computer program that asked about a variety of background characteristics. When all subjects were finished with the survey, each participant was called to the back of the room at random by number, where they received their payment in a sealed envelope.

A total of 270 subjects (133 woman, 137 men) participated in the

experiment. Subjects were predominately undergraduate students. The average overall payoff in the experiment was about $14.00 and the duration of each session lasted for approximately 45 minutes.

2.3.2 Pilot Experiments

It is important to note that the experiment was designed to compare behavior under exogenous and endogenous decision-making processes, and not to compare performances under different compensation schemes. With this in mind, we wanted to create menus of compensation schemes that would be both very simple for subjects to understand, and that would each be reasonably appealing. The contrasting qualities of the basic tournament and the revenue-sharing contract simplify the decision for subjects increasing the possibility that the variance of behavior in the data can be attributed to treatment, rather than to possible confusion over the exact rules of each compensation scheme.

The precise specification of the rank-order tournament used in our experiment (highest performer receives 60% or the total group proceeds, second highest performer receives 30% and lowest performer received 10%) was informed by several pilot experiments conducted with subjects from a separate

subject pool several weeks before the 'live' data collection began. In the pilot experiments we wanted to collect several samples of preferences over various tournament specifications relative to the revenue-sharing contract to increase the probability that each compensation contract would be selected with a reasonable amount of behavioral regularity when 'live' data collection began. Without extensive piloting of the tournament specification, we would have had to rely on theoretical predictions that would require the construction of bias in preferences (i.e. designing a tournament based on selfish preferences or with social preferences). We concluded our pilot experiments when roughly half of our pilot subjects expressed a preference for the 60-30-10 tournament structure and the other half expressed a preference for the revenue sharing contract. During the live data collection process, 57 groups in the Authority treatment (where decision-control rights were held by a single group member) selected the tournament contract and 33 selected the revenue sharing contract (90 total groups). Of the the 93 groups in the Voting treatment (where decision-control rights were held by all group members) the majority voting rule resulted in 57 tournament contracts and 36 revenue sharing contracts implemented by those groups.

We took the further precaution of first collecting data from the treatments

with endogenous choice (Voting and Authority treatments) and then calibrated the randomization procedure in the Control treatment to match the frequency of the compensation scheme selection. Table 2.1 shows that these precautions, in addition to a fair amount of luck, were very successful in generating the same frequency of selected compensation schemes across our treatments.

Table 2.1: Frequency of Compensation Scheme Selected by Treatment

Compensation scheme	Voting	Authority	Control	Total
Revenue Sharing	36	33	33	102
Tournament	57	57	54	168
Total	93	90	87	270

Standard theory would predict that differences in the level of effort between Period 1 (where all subjects paid a flat-wage) and Period 2 (where subjects are paid through an incentive contract), is independent of the *process* by which the incentive contract was implemented. Our study examines the validity of this claim.

2.4 Experiment Results

In the following section we present our results from the experiment.

2.4.1 Summary statistics

The analysis centers on *effort*, which we measure using the number of total attempted questions that subjects provided in each period, as well as their *effective effort,* measured by the total number of correct answers provided in each period. Tables 2.2 and 2.3 provide subgroup summary statistics of total attempted answers correct answers across periods and treatments. Histograms and statistical tests of equivalence of means across treatments are presented in Appendix A.

Table 2.2 Total attempted questions (Effort)

	Overall			**Voting**			**Authority**			**Control**		
	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.
Flat-wage (Period 1)	270	12.75	3.103	93	12.89	3.347	90	13.14	3.357	87	12.19	2.443
Tournament (Period 2)	168	15.78	3.559	57	15.87	3.932	57	16.66	3.700	54	14.75	2.684
Rev Sharing (Period 2)	102	13.98	2.883	36	13.88	3.002	33	14.39	2.946	33	14.39	2.946
Overall (Period 2)	270	15.10	3.429	93	15.11	3.714	90	15.83	3.598	87	14.34	2.735
Flat-wage (Period 3)	270	13.94	4.284	93	13.51	3.846	90	14.95	4.815	87	13.35	3.997

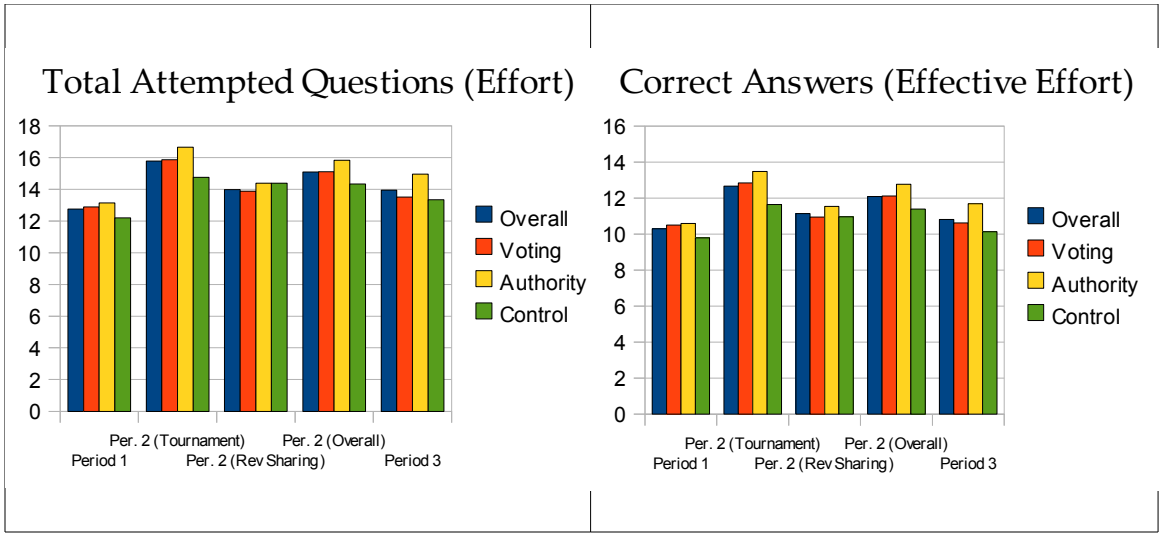
Table 2.3 Correct Answers (Effective Effort)

	Overall			Voting			Authority			Control		
	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.
Flat-wage (Period1)	270	10.30	3.148	93	10.50	3.154	90	10.59	3.613	87	9.79	2.543
Tournament (Period2)	168	12.67	3.865	57	12.84	4.161	57	13.47	4.004	54	11.64	3.169
Rev Sharing (Period2)	102	11.14	2.939	36	10.94	2.562	33	11.54	2.937	33	10.96	3.349
Overall (Period2)	270	12.09	3.615	93	12.11	3.728	90	12.76	3.751	87	11.39	3.236
Flat-wage (Period3)	270	10.81	4.074	93	10.62	3.715	90	11.69	4.585	87	10.13	3.753

The top row in both tables 2.2 and 2.3 report summary performance levels in Period 1 in all treatments (overall), and by treatment. We interpret the level of effort provided in Period 1, where all subjects were compensated via a flat-wage contract, as a reasonable measure of ability and/or a willingness of subjects to provide effort in the absence extrinsic incentives. Where many empirical papers in labor economics rely on the use of proxy measures for unobservable ability levels, a major benefit of our experimental design is that we are able to collect an observation of baseline ability for subjects performing this precise real-effort task, which will become key in subsequent analysis. Standard parametric tests reported in Appendix A (A1 and A2) suggest that there are no differences in mean total attempted questions in Period 1 across treatments ($p = .1097$) or in

correct answers across treatments in Period 1 ($p = .1821$). This indicates that to the extent college-aged subjects had heterogeneous abilities in solving simple addition problems, our randomization procedure successfully distributed these characteristics across treatment groups.[7]

Figure 2.1: Column Graph of Total Effort and Correct Answers in all Treatments and Periods



Before solving math problems in Period 2, subjects were randomly and anonymously assigned to groups of three. The determination of how they would be compensated in Period 2 was done endogenously by vote, endogenously by a single-decision maker in the group, or by a random exogenous process. F-tests evaluating equivalence of mean performance across decision-making treatments is rejected overall among subjects in Period 2 for correct answers ($p = .0400$) and

[7] Random assignment of subjects from our subject pool to treatment occurred prior to the experiment being run.

total attempts ($p = .0150$), as well as in Period 3 for correct answers ($p = 0.0323$) and total attempts ($p = 0.0220$).

2.4.2 Regression analysis

In the following section, we present regression analysis to provide a more thorough examination of whether the process of implementation of the compensation contract at the outset of Period 2 can explain divergence in effort offered in Period 2 performance measures. Our data we meet the Gauss-Markov criteria (with minor heteroscedasticity) therefore justifying the use of OLS with robust standard errors.

Table 2.4: Dependent Variable Total Attempted Questions in Period 2

	(1)	(2)	(3)	(4)	(5)
voting	0.763	0.777	1.864**	1.813**	0.896*
	(0.484)	(0.473)	(0.884)	(0.863)	(0.515)
authority	1.489***	1.466***	1.819***	1.851***	0.931***
	(0.479)	(0.464)	(0.535)	(0.532)	(0.314)
tournament		1.793***	1.509***	1.386***	0.788***
		(0.393)	(0.397)	(0.389)	(0.264)
tourn_preference			-0.708	-0.671	-0.641
			(0.803)	(0.780)	(0.456)
revshar_preference			-1.641**	-1.207	-0.649
			(0.815)	(0.810)	(0.466)
male				1.217***	0.626***
				(0.403)	(0.241)
total1					0.852***
					(0.0456)
Constant	14.34***	13.23***	13.41***	12.79***	3.107***
	(0.293)	(0.381)	(0.382)	(0.454)	(0.614)
Observations	270	270	270	270	270
R-squared	0.031	0.095	0.109	0.138	0.695

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The dependent variable in all regression specifications in Table 2.4 evaluates the number of total answers provided in Period 2, or what we refer to as *effort*. In Regression (1), the reference category is comprised of subjects in the Control treatment, where their compensation scheme in Period 2 was implemented by an exogenous random process. As one can see, subjects in the Authority treatment attempted an average of 1.489 questions more than those in the Control treatment, and this difference is significant at the .01 level. The sign on the coefficient for Voting is positive, but the increase in the level of effort in Voting is not statistically significant relative to the Control. A two tail t-test evaluating whether Voting and Authority coefficients were equal, is rejected (p

= *.1486*).

The second regression specification adds an indicator variable of the compensation scheme implemented in Period 2 as a control. This specification assumes that the effect of the compensation scheme is constant across all treatments, and more importantly for the purposes of our investigation, that mean effort differences that arise from our treatments are constant across compensation schemes. Regression 2 shows that tournament contracts, irrespective of the manner they are implemented, result in higher levels of effort than the revenue sharing contract. This is not an unexpected finding, but one that serves as a general robustness check to the experimental literature where tournament typically result in higher efforts in the lab.[8] In Regression 2 we further notice that the coefficients and robust standard errors for both the Authority and Voting treatments are essentially unchanged from Regression 1. That is, subjects that had their compensation scheme implemented by a single group member in the Authority treatment on average attempted more questions than the Control in Period 2, after controlling for the compensation scheme selection.

The specification in Regression 3 examines whether the difference in effort

[8] See Harbring and Irlunbush (2005) for a very useful survey evaluating effort under different tournament specifications in the lab.

demonstrated in the treatments with endogenous selection in Period 2 is explained by their ability to voice a specific preference in the decision-making process. Notice that all participants in the Voting treatment and the randomly selected member in the Authority treatment had the opportunity to express their preferences over how they would like their group to be compensated in Period 2. In Regression 3, we include indicator variables for the expressed preferences of each person with decision-making power in the Authority and Voting treatments. Interestingly, we first notice negative signs on the coefficients suggesting that individuals with decision-making power had lower output than those without decision-making power. Specifically, the regression suggests that a marginal increase in the number of subjects that expressed a preference for the revenue sharing contract was associated with decrease of 1.641 fewer total attempted answers on average in Period 2 and that this is significant at the .05 level. While there are a number of interesting reasons why subjects may express a preference for one compensation scheme over another, (i.e. a desire to free-ride on others, competition averse, inequality averse, a desire to compete, etc) the data collected herein do not allow for a thorough analysis to understand the precise motivations for why subjects expressed the preferences they did.[9]

[9] In Appendix C, we show that the specifications for Regression 3 as well as the Regressions 4 and 5 are not being driven by differences between subjects with decision-making power and without decision-making power.

Nevertheless, in the third regression specification in Table 2.4 we find evidence suggesting that democratic procedures in the Voting treatment are associated with increased effort for all expressed preferences after controlling for the compensation scheme. Similarly, groups that had their compensation scheme decided by a single group member (the Authority treatment) also demonstrate an increase in effort for all expressed preferences and compensation schemes. Under this specification, the increased effect of voting on effort is significant at the .05 level and the increase in effort from groups that had a single decision-maker is significant at the .01 level. Taken together, the results under this specification suggest that the success of a compensation scheme in influencing performance can be different when imposed by a process external to a group, rather than when it is imposed by an endogenous decision-making process.

Because of the well-known literature that has shown gendered attitudes towards competition (e.g. Niederle and Vesterlund (2007)), and performance differences in competitive environments to be sensitive to gender (e.g. Gneezy et al. (2003)), the regression specification in Column 4 of Table 2.4 includes a control variable for gender. We find that on average, men in the second period provided more effort than women, but more importantly for the purposes of our investigation, after controlling for gender, compensation scheme, and expressed

preferences, the treatments with endogenous selection procedures continue to account for significant differences in performance.

Finally, in Regression 5 we include a control variable of subject ability. Our ability measure uses the performance of subjects in Period 1 of the experiment. Perhaps the main advantage of using an experimental design over a tradition empirical design to study worker performance is that in the lab we are able to collect an *observable* measure of ability for a specific production task in the absence of extrinsic incentives. Indeed, a traditional weakness of many labor economics papers examining the effects of different institutional features on performance is not having a measure of performance, and hence having to resort to constructing proxy measures of ability and/or relying on econometric techniques that can be loaded with strong implicit behavioral assumptions to control for unobservable characteristics. Our data set provide a baseline measure of ability for subjects, which we use in Regression 5.

One might expect that individuals with higher demonstrated ability in Period 1 would perform at higher levels in Period 2 after controlling for compensation scheme, decision-making treatment, gender, and expressed preference, and our findings in Regression 5 are consistent with this intuition. Moreover, we find evidence that after including the additional control measure

of ability, the decision-control right arrangement strongly influences the level of performance in Period 2 with members in the Authority treatment displaying an average increase of 0.93 total attempted questions relative to the Control which is significant at the 0.01 level, and subjects in the Voting treatment displaying an average increase in performance of 0.896 total attempted questions relative to the Control, which is statistically significant at the 0.10 level. In sum, Regression 5 shows that endogenous decision processes that result the imposition of the compensation contract result in higher performance than an exogenous contract imposition after controlling for ability, compensation scheme, gender, and the expressed preferences of subjects.

In Table 2.5 (shown below), we conduct the same 5 regression specifications that were run on total effort in Table 3, but we change the dependent variable from *total attempted questions* to *total correct answers* in Period 2 to evaluate *effective effort*. As one can see, our findings reported in Table 2.5 are consistent with the qualitative and quantitative story that arises from Table 2.4 with few exceptions. Most importantly, Table 2.5 again shows that members in the Authority treatment display a higher level of effort in Period 2 across all regression specifications at the .01 level of significance than the reference categories, again suggesting that the process of implementation has an effect on

performance even after controlling for compensation scheme, expressed preferences for those with decision-making power, gender, and our baseline ability measure.

Table 2.5: Dependent Variable Correct Answers in Period 2

	(1)	(2)	(3)	(4)	(5)
voting	0.717	0.729	2.266**	2.221**	1.095*
	(0.519)	(0.513)	(0.936)	(0.920)	(0.588)
authority	1.376***	1.357***	1.860***	1.889***	1.075***
	(0.526)	(0.518)	(0.589)	(0.586)	(0.388)
tournament		1.514***	1.226***	1.117**	0.618*
		(0.415)	(0.443)	(0.439)	(0.346)
tourn_preference			-1.152	-1.120	-1.024**
			(0.836)	(0.815)	(0.505)
revshar_preference			-2.100**	-1.714**	-0.595
			(0.857)	(0.856)	(0.535)
male				1.083**	0.813**
				(0.437)	(0.315)
correct1					0.808***
					(0.0559)
Constant	11.39***	10.45***	10.63***	10.08***	2.629***
	(0.347)	(0.440)	(0.451)	(0.513)	(0.676)
Observations	270	270	270	270	270
R-squared	0.024	0.065	0.084	0.104	0.569

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

2.4.3 Period 3 Data

In both Period 1 and Period 3 subjects were compensated a flat-wage of 75 EUs to solve addition problems for 5 minutes. Notice that subjects do not receive any marginal benefit for each correctly solved math problem and there are no consequences for shirking. Nevertheless subjects engaged in production in both Period 1 and Period 3 attempting an average of 12.75 and 13.94 questions respectively (*effort*). Similarly subjects answering an average of 10.30 and 10.81

questions correctly (*effective effort*) in Period 1 and Period 3 respectively. Our study was designed to test if different implementation processes at the outset of Period 2 affected effort in Period 2, but the additional data gathered in Period 3 are useful for at least 2 reasons. First, they allow for a test of crowding effects which we examine more thoroughly in Appendix E, but more importantly for the present analysis, Period 3 provide an additional observation of ability along with Period 1 data.[10] When we try regression specifications that include Period 3 data, we find that the qualitative results from the previous section do not significantly change.

Specifically, Regression 1 in Table 2.6 and Table 2.7 shown below reproduce Regression 5 in Table 2.4 and Table 2.5. Regression 2 in both tables includes Period 3 data instead of Period 1 data as a measure of ability, and Regression 3 in both tables includes both Period 1 and Period 3 data as control variables.

The results in Tables 2.6 show that the treatment effect of Voting becomes

[10] An alternative interpretation of the data collected in Period 3, however, is that they measure the willingness to provide effort in the absence of any extrinsic incentives-- or their intrinsic motivation for performing the production task. Notice that at the beginning of Period 1, subjects had no prior experience with the specific production task used for the duration of the experiment but by Period 3 the subjects had 10 minutes of experience and they were also told that their compensation would be identical to the compensation received in Period 1 (a flat-wage payment of 75 EUs). The data in Period 3 further allow us an opportunity to study if the experience of the experiment led to any systematic increases or decreases in effort. An econometric exercise shown in Appendix E suggests that the conditions of the experiment did not contribute to any crowding-in or out in Period 3. We do find, however, that that past success in production contributed to more effective effort in Period 3.

even stronger when including the Period 3 data than reported in Tables 2.4. All other co-variates remain largely unchanged with the incorporation of Period 3 data into the regression analysis.

Table 2.6: Dependent Variable Total Attempted Questions in Period 2 (Period 3 Data)

	(1)	(2)	(3)
voting	0.896*	1.752***	1.104**
	(0.515)	(0.587)	(0.443)
authority	0.931***	0.968**	0.756***
	(0.314)	(0.378)	(0.268)
tournament	0.788***	1.010***	0.766***
	(0.264)	(0.334)	(0.253)
tourn_preference	-0.641	-1.078**	-0.839**
	(0.456)	(0.498)	(0.381)
revshar_preference	-0.649	-0.931*	-0.663
	(0.466)	(0.558)	(0.419)
male	0.626***	0.416	0.404*
	(0.241)	(0.292)	(0.218)
total1	0.852***		0.632***
	(0.0456)		(0.0498)
total3		0.550***	0.258***
		(0.0582)	(0.0432)
Constant	3.107***	6.131***	2.490***
	(0.614)	(0.728)	(0.639)
Observations	270	270	270
R-squared	0.695	0.567	0.752

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

It should be noted that Period 1 data and Period 3 data are not statistically equivalent to one another (see Appendix D). Using two-tailed paired t-tests, we reject the hypothesis of equivalence of means between Period 3 and Period 1 total attempted questions *(p = 0.01)* and also correct answers *(p = 0.01)*. Our data unfortunately do not allow us to cleanly infer *why* Period 3 effort was higher than in Period 1. Perhaps Period 3 effort was higher than Period 1 effort because 1)

subjects had more practice with the specific production task, or possibly because 2) subjects did not feel any pressure to perform since they were not going to be paid through an incentive contract.[11]

Turning now to how the inclusion of Period 3 data affects performance measured by effective effort, we again notice that the qualitative story does not change much compared to Table 2.5. In Regression 2 of Table 2.7 we do see a large increase on the Voting coefficient but when we include both Period 3 and Period 1 data in Regression 3, we arrive to the same qualitative conclusions as in Table 2.5. Specifically, after controlling for compensation scheme, ability, expressed preferences, and gender, groups with endogenous rule creation performed at significantly higher levels than the control group with exogenous rule creation.

[11] Cognitive psychologists that suggest that pressure reduces the working memory capacity available for skill execution. For example, an experiment conducted by Beilock and Carr (2005), shows that only individuals high in working memory capacity were harmed by performance pressure when solving math problems, and, furthermore, these skill decrements were limited to math problems with the highest demands on working memory capacity. The authors suggest that that performance pressure harms individuals most qualified to succeed by consuming the working memory capacity that they rely on for their superior performance.

Table 2.7: Dependent Variable Correct Answers in Period 2 (Period 3 Data)

	(1)	(2)	(3)
voting	1.095*	1.936***	1.281**
	(0.588)	(0.692)	(0.530)
authority	1.075***	0.977**	0.822**
	(0.388)	(0.422)	(0.341)
tournament	0.618*	0.536	0.451
	(0.346)	(0.358)	(0.328)
tourn_preference	-1.024**	-1.373**	-1.192***
	(0.505)	(0.626)	(0.457)
revshar_preference	-0.595	-1.545**	-0.843*
	(0.535)	(0.628)	(0.494)
male	0.813**	0.493	0.571**
	(0.315)	(0.333)	(0.289)
correct1	0.808***		0.562***
	(0.0559)		(0.0587)
correct3		0.581***	0.319***
		(0.0540)	(0.0503)
Constant	2.629***	4.891***	2.052***
	(0.676)	(0.656)	(0.644)
Observations	270	270	270
R-squared	0.569	0.502	0.645

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2.4.4 Quantile Regression

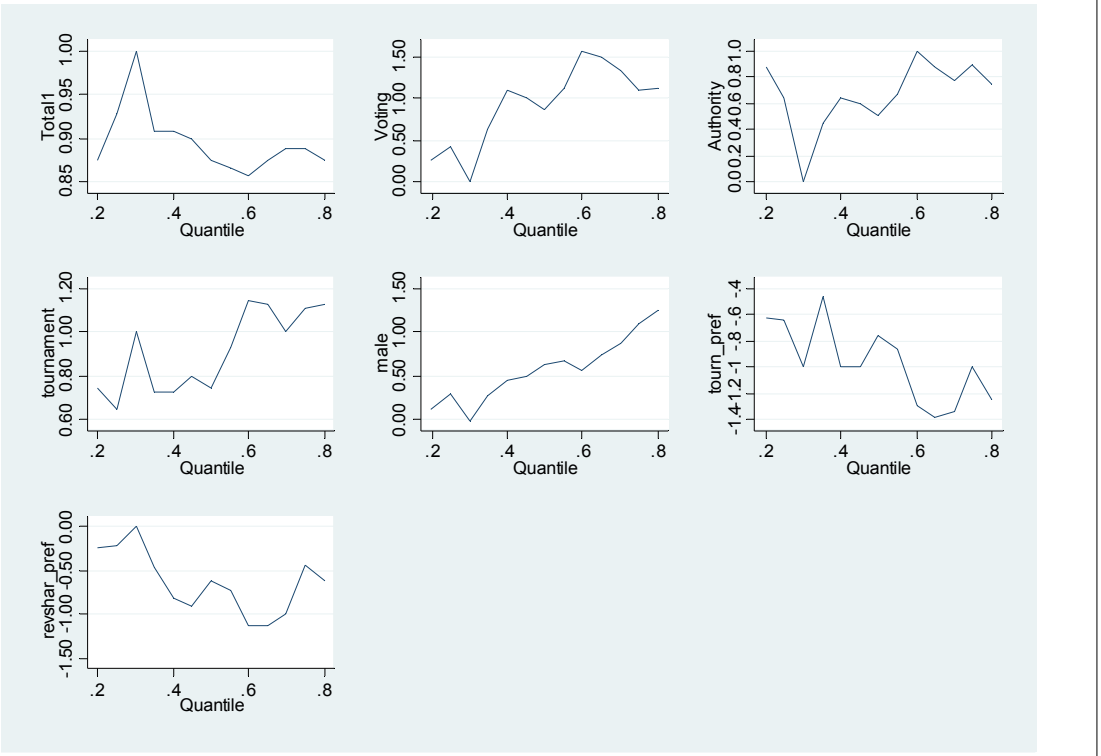
Our analysis up to now has employed conventional least squares regression which evaluate the various effects of our covariates on the conditional mean performance (measured either via total answers, or correct answers) in Period 2. In order to provide a more complete picture of the covariate effects across the performance distribution of subjects we also provide estimates of the conditional quantile regressions shown in Figures 2.2 and 2.3 below to illustrate possible heterogeneous treatment effects. Figure 2.2 shows quantile estimates for

total effort, and Figure 2.3 for correct answers. All graphs are bounded at the 20th and 80th quantiles in both figures because of the relatively small number of observations in those quantiles. In an effort to be consistent with previous regression analyses, we summarize quantile regression on the same seven covariates evaluated in the 5th columns of our linear regression specifications shown in Tables 2.4 and 2.5. At any chosen quantile for a given covariate, the graphs provide point estimates of the impact of a one-unit change of the covariate on performance in Period 2 at each quantile holding the other covariates fixed.

While the quantile regressions do not reveal strong evidence suggesting heterogeneous effects, we do note some potentially interesting and informative qualitative phenomena. Specifically, the second panel in the first row of both figures 2.2 and 2.3 shows the effect of being in the Voting treatments for each performance quantile and this is evidence suggesting that higher performing subjects in the Voting treatment were more affected by the decision-making treatment than lower performing subjects. We observe a similar result when comparing subjects in the tournament with those in the revenue sharing contract (first panel in the second row of both figures). That is, higher performing subjects in the tournament contract performed at a higher level than high

performing subjects in the revenue sharing contract.

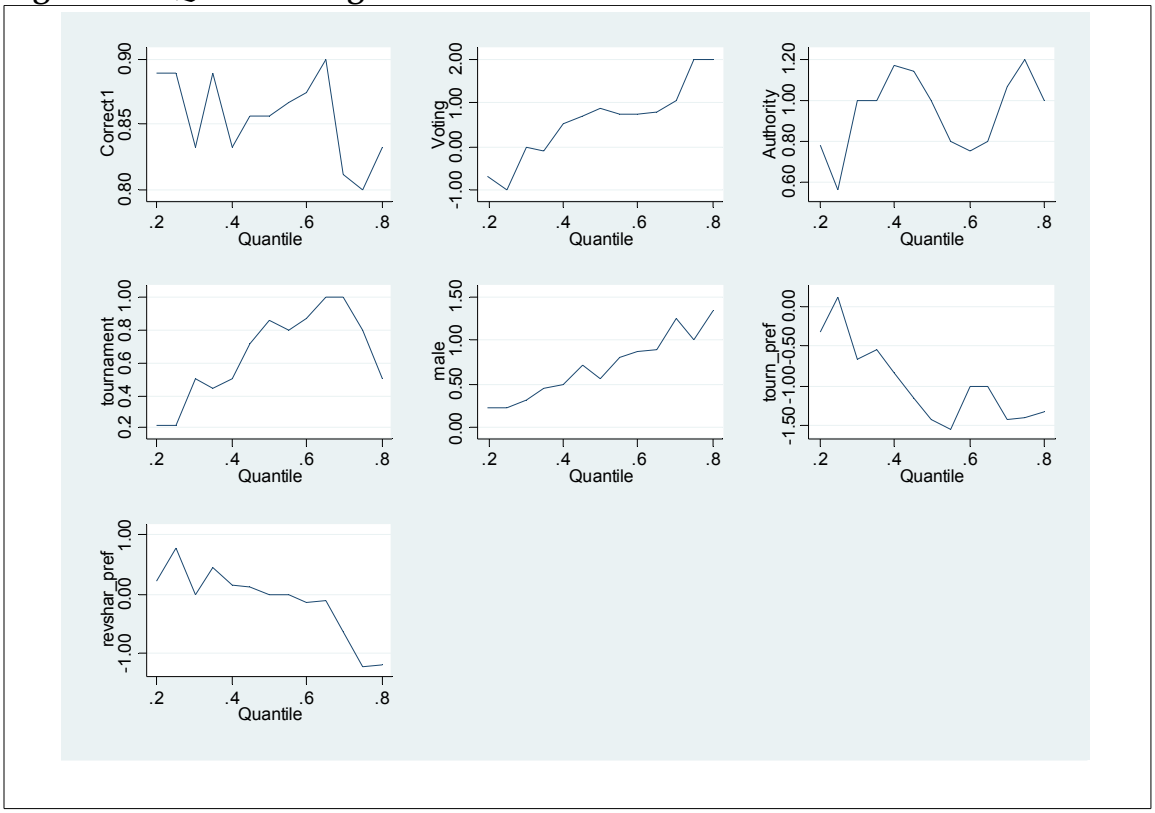
Figure 2.2: Quantile Regression Estimates for Total Answers in Period 2



Effective effort in Figure 2.3 falls off quite sharply for quantiles above .70 but total effort in Figure 2.2 does not. This may indicate that high performing subjects were trying to maintain a high level of performance to win the tournament, but were doing so at the cost of quality performance. Indeed a typical finding in the experimental economics literature shows that the variance of effort under tournament compensation schemes is usually much higher than what is theoretically predicted. This particular pattern has been noted in chosen

effort lab experiments (Bull et al. (1987)), real effort experiments of van Dijk et al. (2001), and the field experiments conducted in Shearer (2004). The heterogeneous effects shown in our analysis do not offer much of an explanation for why there is high variation in tournament schemes, but they do suggest some hypotheses that could be tested with a different design. For example, it may be that high-performers become reckless in tournaments and low performers, knowing that they are relatively weaker are not much affected by tournament schemes relative to say, piece-rates.

Figure 2.3: Quantile Regression Estimates for Correct Answers in Period 2



Finally, when we compare the corresponding performances in Period 2 by

gender, we observe that at all quantiles, men performed at a higher level than women given the specification of the other conditioning variables in both Figure 2.2. and Figure 2.3.

2.5 Conclusion

With few exceptions, the economics literature has overlooked how procedural aspects in the determination of rules and institutions affect behavior. We've shown that participants in a real-effort laboratory experiment subjected to an identical set of wage incentives either implemented (1) endogenously by the group to which subjects belong through a simple majority vote, (2) endogenously by only one member of the group who had all decision-control rights, or (3) a random process completely exogenous to the group, systematically performed at different levels even after controlling for gender, the ability to voice a preference for a compensation scheme, and a measure of demonstrated ability. Specifically, we found strong statistical evidence that different levels of effort were provided under endogenous imposition schemes relative to the random exogenous processes. These findings are robust to alternative regression specifications that include two separate measures of ability. Furthermore, quantile regression analysis showed that some of these treatment effects were heterogeneous across

the performance distribution with higher performing subjects in the Voting treatment more affected by 'voting' than low performing subjects in the same treatment, relative to the Control.

By showing that the *process* by which a material incentive was implemented partially explains differences in performance, we add further credence to recent claims in economics (and well established claims in many related social sciences) that procedural aspects cannot be separated from how individuals interpret material incentives (e.g. Frey and Stutzer (2004)). This research raises many more questions than provides definite answers. To what do we attribute the differences in the interpretation of incentives that are materially identical? Would we expect to observe similar results if the decision-maker in the Authority treatment was determined by a non-random process? Does the process of implementation matter more or less if subjects were given a different effort task, or given the opportunity to select out of the effort task? While much is left unresolved, we believe that the issues we touch upon in this paper are ripe for further exploration.

CHAPTER 3

THE RELATIVE EFFECT OF EMPLOYEE VOICE, AUTONOMY, AND WAGES ON JOB SATISFACTION

3.1 Introduction

In the following paper we analyze the relative effects of voice, autonomy, and wages in explaining job satisfaction. While job satisfaction has been identified as an important economic variable because of its association with lower absenteeism, (Wegge et al., (2007)) quits, (e.g. Freeman, 1978), and positive association with work behavior that extends beyond perfunctory standards (Organ and Ryan, (1995)), job satisfaction is still a relatively "new" subject of interest within Economics. By contrast, in both Organizational Behavior and Management Studies job satisfaction is one of the most investigated variables (Cranny et al. 1992, Spector 1997). Daniel Hamermesh (2004) likens the recent uptick in the interest of satisfaction studies in economics as partly a function of the "Mt. Everest phenomenon" or the mountain of extensive, potentially interesting, attitudinal data that should be explored.[12] The increase in behavioral considerations within economics, however, has also contributed to a general

[12] For examples of seminal papers by economists on job satisfaction as an economic variable, See Hamermesh (1977) and Freeman (1978). Frey and Stutzer (2002) provides an excellent review of subjective evaluation studies for questions economists are traditionally study.

expansion of the scope of questions that are studied by economists – including job satisfaction. This new trend has affected the framework of analysis. The rational choice model (RCM) has been central in traditional economic evaluations of personnel and human resources issues in the firm (e.g. incentives, compensation, and job search) but it is increasingly recognized that the RCM may have several shortcomings.

First, within the RCM all agents are assumed to act in the pursuit of their own private interest which has led to the ubiquitous assumption that utility maximizing workers conceptualize 'work' as a private cost and will therefore have a natural tendency to shirk. We know, however, that workers draw upon their employment as a source of utility through various avenues – including the social interaction they have with co-workers, feelings of accomplishment, the associated social status of work, or simply from performing the work task itself (Frey and Stutzer (2002)). Both Lane (1991) and Juster (1991), furthermore, find that most people rate 'satisfying work' as more important to explaining personal happiness than income, material possessions, and most forms of leisure. Other studies show that the psychic costs endured during a of a spell of unemployment typically include anxiety, loss in self-esteem, and depression (e.g. Argyle, 1989).

Second, within the RCM it is assumed that 1) preferences are revealed

through choices, 2) the revealed preferences are those that maximize utility, and 3) preferences are consistent through all points in time (Read, (2007)). Hundreds of studies, however, show that humans often 'reveal choices' that would be inconsistent with utility maximization within the RCM. For example, we know that 'real-world' individuals use cognitive heuristics that can lead to systematic errors when evaluating probabilistic events (e.g. Kahneman and Tversky (1979)), they regularly demonstrate time-inconsistent preferences, (e.g. Thaler, (1981), Laibson, (1997)), and that the hedonic effect of favorable and unfavorable circumstances adjusts through time (e.g. Frederick and Loewenstein (1999)). In consideration of these and other behavioral issues that conflict with the RCM framework, Daniel Kahneman (e.g. Kahneman et al. (1997), Kahneman et al., (1999), Kahneman and Sugden, (2005)), and others have advocated for the use of *experience utility* – or the utility as gained through experiencing a set of circumstances that one lives through, in addition to *decision utility* – or the utility gained when revealing exogenously given preferences, to evaluate outcomes. Consistent with the concept of experience utility, we conceptualize self-reported levels of job satisfaction as a reflection of an *attitude* that a worker has in consideration of their job characteristics *after* having gained some experience with their work. In particular, we investigate the relative impact of job

characteristics that allow workers *autonomy* and *voice* using subjective evaluations of work conditions and satisfaction recorded in the 2004 wave of the Workplace Employment Relations Survey (WERS).

The 2004 WERS includes a variety of questions that depict attitudinal and subjective evaluations by workers of managers, other employees, and third party employee representatives, over a variety of employment relations issues. Some financial data on the firm, including employee wages are also provided, and we use data on wages in addition to employees' subjective evaluation over the ability to control their own work and to influence firm level policy and organization.[13] We also draw upon data that has been made linkable to the 2004 WERS that provides selected characteristics of the local geographic area that the firm is located in, such as the local unemployment rate.

Using survey responses, we find that the wage is positively correlated with job satisfaction; but when measures of worker autonomy and voice are

[13] Subjective evaluation surveys, while are not without 'noise' that can arise from various factors (i.e. mood bias, response biases) offer the most widely used approach for studying satisfaction and are the method that we employ. (See Frey and Stutzer (2002) for a general review subjective evaluation studies in economics. Kahneman and Sugden (2005) do discuss potential alternatives to measuring experience utility that have been used including the experience sampling methodology where each subject is asked to carry a device that beeps at random times during the day, at which time the subject is asked to respond to questions regarding her current situation and affective state. They also describe the day reconstruction method where subjects are asked to think about their previous day, decompose it into short 'episodes' such as 'having dinner' or 'traveling to the gym' and then, for each episode, to note if she was interacting with anyone and to describe how her affective state (for example, 'happy', 'enjoying myself', 'frustrated/annoyed', 'worried/ anxious'). The day reconstruction method is thought to reduce the vulnerability of subjective measurements that might arise due to focusing illusions – or the tendency to exaggerate the importance of the current focus of one's attention, because it does not prompt people to think about particular sources of happiness or unhappiness. Instead, respondents evaluate the overall affective experience of different episodes, whose boundaries they define for themselves.

included in the satisfaction regression, the magnitude of the coefficient on the wage decreases by a factor of 3. The elasticity of satisfaction with respect to autonomy and voice, respectively, is 12 to 15 times larger than the elasticity with respect to the wage. The inclusion of autonomy and voice further causes a spike in explaining the proportion of variability of job satisfaction. For instance, the Adjusted R^2 spikes from 0.093 to 0.476 when including measures of autonomy and voice (F-tests comparing specifications significantly differ at the 0.01 level). Thus, autonomy and voice both appear to trump the wage in determining job satisfaction. We conclude that, conditional upon having a job, the amount of control a worker has is an important determinant of job satisfaction.[14] Our findings suggest that while wages are an important determinant of which job an individual would select into, it is not the most important determinant of job satisfaction (experience utility); particularly when compared to autonomy and employee voice.

3.2 Background

[14] This is cross-section data and occupation fixed effects are used. All identification comes from variation across existing workers in a given occuaption

3.2.1 Employee voice and autonomy

In a general sense, employee voice refers to the idea that workers can express their interests and concerns over firm matters to management in a meaningful manner. The precise meaning of the term, however, and the rationale for its application varies along different economic, moral, and pragmatic dimensions.[15] For example, Hirshman (1970) conceptualizes 'voice' in a firm as employee access to grievance procedures, while others expand on the term for it to refer to the level of influence employees have in development of all firm rules and policies ranging from payment, to work schedules, investment decisions, and production processes (e.g. McCabe and Lewin (1992); Huselid (1995)). Although collective bargaining and/or trade unionism have historically been the primary mechanisms for the representation of worker interests, initiatives designed to increase employee involvement have increasingly featured in the prescriptive management literature for both unionized and nonunion workers over the past 50 years.[16]

Similarly, the literature on worker autonomy contains many definitions

[15] Dundon et al. (2004) provide an excellent taxonomic scheme of how *voice* has been conceptualized in different literatures.

[16] This is particularly true in Europe where nonunion workers can legitimately have formal voice regarding their working conditions supported by the EU Directives on European Works Councils (EWCs) and Employee Information and Consultation. While many petitions and political movements supportive of similar legislation exist in the U.S. such as the Workplace Bill of Right campaign (http://www.workplacefairness.org/workplace-bill-of-rights) similar legislation has yet to be introduced in the U.S. Bowles and Gintis (1996) further comment on the notion that democratic processes within firms are not redundant within a liberal democratic polity.

and interpretations of the precise meaning of the term. While some definitions are used to interpret potential influences on the worker's work practice, others are used to guide research about the organizational features of the firm. For the purposes of the research conducted herein, and in conjunction with the 2004 WERS, we define autonomy as the ability of a worker to use her own knowledge to make decisions that directly affect her work.

3.2.2 Prescriptive Human Resource Management

The prescriptive management and organizational behavior literature is full of superlative hypotheses describing virtuous cascades that follow from providing workers with more autonomy and voice, including higher job satisfaction. The empirical record on these issues is at least as old as scientific management, but most studies have been restricted to case studies on a small number of firms. The quality and extent of large-scale micro attitudinal data on the topic are a recent phenomenon. The bulk of studies on voice and autonomy, moreover, focus on the effects of specific organizational mechanisms (such as joint consultation or works councils) on job satisfaction. While there is surely overlap between employee voice, autonomy, and the firm policies designed to

foster them, we instead use workers' subjective evaluation of the level of voice and autonomy that they have in the firm. This approach allows us to engage with the issues of voice and autonomy on general terms rather than focusing on any specific management or human resources paradigm.[17]

3.2.3 Similar Attitudinal Data

The emergence of large micro data sets linking individuals to particular types of work organization structures is recent. Appelbaum et al. (2000) survey workers in apparel, steel, medical electronics, and imaging industries in the U.S. finding that "high-performance workplace practices" that introduce work teams and rely more on participatory decision-making have many virtuous effects. They report increases in workers' trust in their managers, higher intrinsic motivation, increases in job satisfaction, and also in organizational commitment. They further find that the added responsibility that comes with these organizational structures does not negatively affect workers' stress. In general,

[17] While not entirely intractable, the management literature offers various prescriptive frameworks that overlap significantly with respect to management practices. For example, one can find significant literatures on Strategic Human Resource Management (SHRM), Human Resource Management (HRM), High Commitment Management (HCM), High Performance Management (HPM), High Involvement Management (HIM), High Performance Workplace Practices (HPWP), High Performance Work Systems (WPWS), High Involvement Work Design (HIWD), Total Quality Management (TQM) among (many) others that are different, yet al.l make claims over the importance of advancing employee voice and autonomy.

the positive effects of participation and work teams on job satisfaction are corroborated in the analysis of the U.S. based Workplace Representation and Participation Survey (WRPS) reported in Freeman and Rogers (1999), and Freeman and Kleiner (2000), the European Survey on Working Conditions (ESWC) as reported in Bauer (2004), data collected by the Institute of Work Psychology reported in Stride et al. (2007), and also in many firm level case studies (e.g. Lawler and Hall; (1970); (Bjork et al. 2007). A survey conducted in Godard (2001), however, suggests increasing levels of participatory workplace strategies can weaken and in some cases decrease job satisfaction because of new pressures and responsibilities that accompany participation. Similar findings have been reported in Batt and Applelbaum (1995), Graham (1995), and Lewchuk and Robertson (1997). [18]

Our own study is similar to Wood (2008) because of the use of subjective evaluation data measuring voice and satisfaction taken from the 2004 wave of the Workplace Employee Relations Survey. The emphasis of Wood's piece is in showing the importance of job characteristics for researchers interested in analyzing job satisfaction. Our study, by contrast investigates the relative effects of voice and autonomy, compared to the wage in explaining job satisfaction.

[18] For example, see the edited volume by Freeman, Boxall, and Haynes (2007) for descriptive summaries of various attitudinal surveys that build upon survey work conducted in Freeman and Rogers (1999). Studies from data collected from the WPRS, BWRPS, AWRPS, NZWRPS, NCPP/ESRI/UCD, Canada-U.S. Labor Attitudes Survey, and WERS are presented.

Economists are relatively new to studying job satisfaction, and have largely ignored the importance of job characteristics. To be fair, Lazear (1996) mentions that economists have dealt with the study of job characteristics that operate outside of the price system "quite easily" by transforming nonpecuniary components of the job into their monetary equivalents by equalizing differences or "compensating differentials" (e.g. Rosen 1974). Lazear, however, is quick to point out the "fundamental identification problem" that occurs if workers with exogenous preferences sort according to ability, making it difficult to pick up the trade-off between wages and job characteristics independent of ability. Of course, the theory of compensating differentials is rooted in the RCM, which, as we have illustrated above, may be problematic in and of itself.

3.3 Data

The data come from the worker, management, and geographic files of the 2004 wave of the Workplace Employee Relations Survey (WERS) sponsored and collected by the Department of Trade and Industry, ACAS, the Economic and Social Research Council, and the Policy Studies Studies Institute. The WERS survey is a nationally representative, stratified, random-sample of United

Kingdom workplaces with at least 10 employees. The survey samples no more than 25 employees from a given firm and there are approximately 2300 workplaces and 22,500 employees in the 2004 WERS.

The final subsample of the data set used here contains 15,547 observations. There are two main sources of missing observations. The first source is unusable responses to questions about earnings, where an unusable response is either a non-response or one where the worker did not know her income. The second main source is non-responses to questions about the various aspects of worker autonomy and voice.

3.4 Methodology

Autonomy is a measure of the worker's ability to control her own job, and voice is a measure of the ability of a worker to influence the firm as a whole via communication with management. The questions regarding satisfaction, autonomy, voice, effort, and job security are all coded according to a Likert scale with 1 being ``strongly disagree'' and 5 ``strongly agree.'' Thus, higher numbers mean greater satisfaction, greater autonomy, greater voice, and greater job security, respectively.

Table 3.1: Summary Statistics of Key Variables

Statistic	Satisfaction	Autonomy	Voice	Wage
Mean	24.46	14.97	12.26	10.42
Std. Dev	(4.88)	(3.65)	(3.91)	(5.77)

Source: Authors' calculations based on the 2004 Workplace Employee Relations Survey.
Notes: Index of autonomy is the sum of 5 aspects of workplace autonomy, and varies between 5 and 25. Index of satisfaction is the sum of seven aspects of job satisfaction, and varies between 7 and 35. The wage is calculated by taking usual weekly income and dividing it by usual weekly work hours.

Table 3.1 contains summary statistics of the four key variables: job satisfaction, autonomy, voice, and the wage. Except for the wage, the key variables are all captured by multiple questions in the WERS survey. We combined the respective questions on satisfaction, voice, and autonomy into indexes. These indexes represent the overall level of satisfaction, voice, and autonomy that workers report having in the workplace. The three indexes are constructed through summation of the responses to the relevant questions.[19] There are seven questions on job satisfaction covering satisfaction with achievement, initiative, influence, training, pay, job security, and the work itself. The survey covers five aspects of autonomy: control over the tasks to be completed, the pace tasks are completed, how tasks are to be completed, the

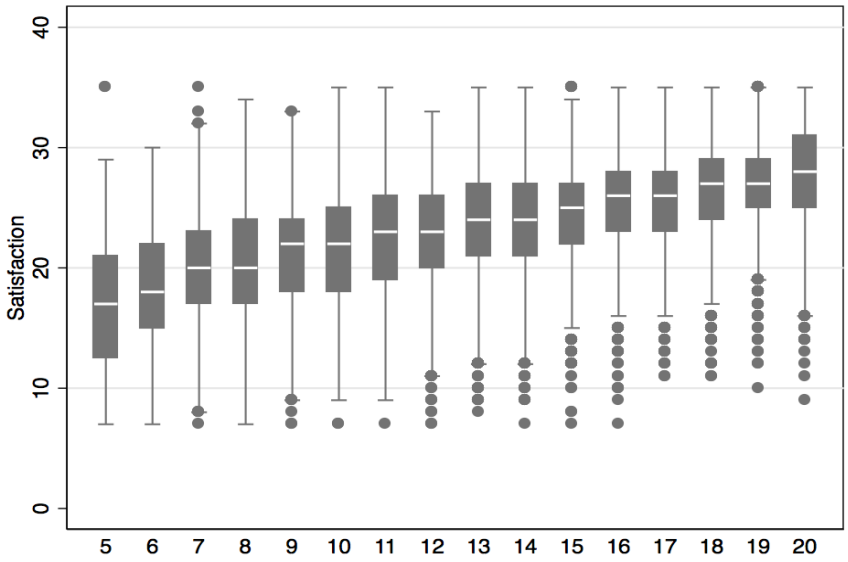
[19] The results of a PCA on the 7 satisfaction questions suggested 6 orthogonal components. We therefore found it reasonable to construct an index using simple summation. In subsequent analysis, we took the logs of all variables for comparison of coefficients without units.

order in which tasks are completed, and daily start and/or stop time. Finally, there are four aspects of voice: the extent to which management solicits views from workers, how responsive management is to the suggestions of employees, how good managers are at letting employees and/or employee representatives influence final decisions, and overall satisfaction with the amount of employee involvement there is in workplace decision making. The precise questions used are listed in Appendix F. The three indexes--job satisfaction, autonomy and voice--vary between 7 and 35, 5 and 25, and 4 and 20, respectively.

The primary justification for using indexes constructed in this manner is simplicity for both presentation and interpretation of the results. There is, however, an important conceptual justification for the indexes, particularly the index of job satisfaction. The questions covering job satisfaction are intended to cover aspects of satisfaction that can vary independently of each other to paint more complete picture of job satisfaction. We find it reasonable to think of the sum of the seven aspects as representing overall satisfaction. Of course, for a given level of overall job satisfaction, this construction obscures important variation across individuals.

Figure 3.1: Plot of Overall Satisfaction over Autonomy

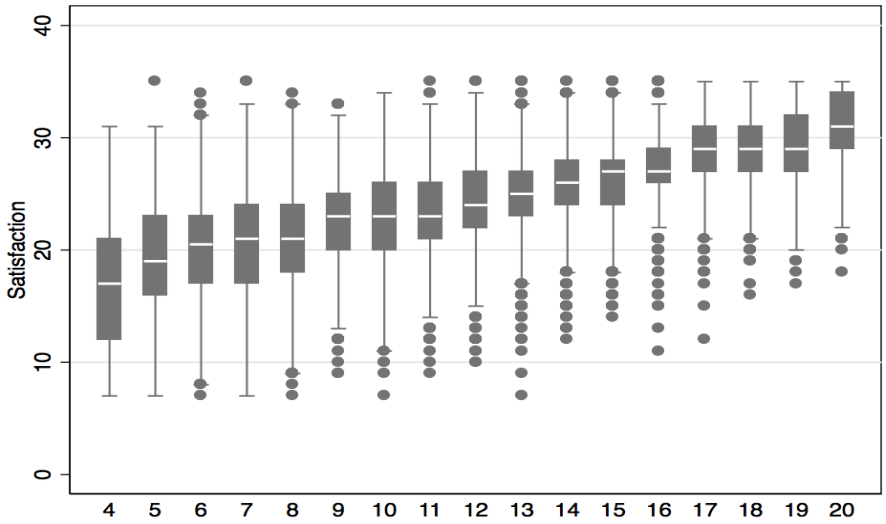
Source: Authors' calculations based on the 2004 Workplace Employee Relations Survey.



Notes: Index of autonomy is the sum of 5 aspects of workplace autonomy, and varies between 5 and 25. Index of satisfaction is the sum of seven aspects of job satisfaction, and varies between 7 and 35.

Figure 3.2: Plot of Overall Satisfaction over Voice

Source: Authors' calculations based on the 2004 Workplace Employee Relations Survey.



Notes: Index of voice is the sum of four aspects of voice, and varies between 4 and 20. Index of satisfaction is the sum of seven aspects of job satisfaction, and varies between 7 and 35.

The central hypothesis that job satisfaction increases with the amount of autonomy and voice a worker has over the workplace is illustrated in Figures 3.1 and Figure 3.2. There is a strong positive correlation between satisfaction and both autonomy and voice, respectively. Consistent with Wood (2008), the figures demonstrate the importance of the relationship between these two job attributes and job satisfaction. However, there are likely important correlates of job satisfaction that could reduce or eliminate the correlation between job satisfaction and voice and autonomy, respectively. Most importantly, jobs with more autonomy likely pay more, implying that the strong positive correlation between job satisfaction and autonomy is picking up the effect of the wage as well as autonomy itself. In the following section we investigate this issue further with regression analysis.

3.5 Estimation Results

The regression results can be found in Table 3.2. The key variables of the analysis are the wage, the index of autonomy, and the index of voice. The remaining control variables, common to all regressions include usual weekly work hours, and dummies for begin a supervisor, a union member, female,

married, a permanent employee, and a full-time employee. All regressions also contain categorical variables for age, education, tenure, and race, as well as a set of occupation dummies. All regressions are Log-OLS regressions, where the dependent variable is the index of job satisfaction.[20] Standard errors are clustered on the firm.

[20] Because of how the index is constructed, there is some clustering at the extreme values. The results are robust to estimation with Tobit rather than OLS.

Table 3.2: Regression of Job Satisfaction on the Wage, Voice, and Autonomy

	(1)	(2)	(3)	(4)	(5)	(6)
log wage	0.050***	0.022***	0.036***	0.025***	0.019***	
	(0.007)	(0.005)	(0.006)	(0.006)	(0.006)	
log autonomy		0.229***		0.350***	0.215***	0.232***
		(0.008)		(0.010)	(0.009)	(0.008)
log voice		0.307***	0.356***		0.319***	0.307***
		(0.006)	(0.007)		(0.008)	(0.006)
supervisor	0.068***	0.014***	0.031***	0.034***	-0.075	0.017***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.056)	(0.004)
supervisor x wage					0.008	
					(0.008)	
supervisor x autonomy					0.063***	
					(0.019)	
supervisor x voice					-0.041***	
					(0.012)	
union	-0.050***	-0.018***	-0.025***	-0.034***	-0.017***	-0.017***
	(0.006)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)
female	0.040***	0.021***	0.026***	0.031***	0.022***	0.018***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
hours	0.001***	0.001***	0.002***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
married	0.011**	0.007**	0.012***	0.005	0.007**	0.008**
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
permanent	0.029***	0.030***	0.025***	0.035***	0.030***	0.030***
	(0.009)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
fulltime	-0.057***	-0.046***	-0.044***	-0.057***	-0.045***	-0.042***
	(0.009)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
Age						
22-29	-0.026**	-0.020**	-0.021**	-0.024**	-0.019**	-0.017**
	(0.011)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
30-39	-0.031***	-0.021**	-0.017*	-0.034***	-0.019**	-0.015
	(0.011)	(0.009)	(0.009)	(0.011)	(0.009)	(0.009)
40-49	-0.034***	-0.024**	-0.021**	-0.035***	-0.023**	-0.018*
	(0.011)	(0.009)	(0.009)	(0.011)	(0.009)	(0.009)
50-59	-0.011	-0.007	-0.003	-0.016	-0.006	-0.002
	(0.012)	(0.010)	(0.010)	(0.011)	(0.010)	(0.009)
60-65	0.060***	0.044***	0.057***	0.040***	0.045***	0.048***
	(0.014)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)
> 65	0.140***	0.054***	0.082***	0.086***	0.055***	0.056***
	(0.024)	(0.016)	(0.018)	(0.021)	(0.017)	(0.017)
Tenure						
1-2 years	-0.029***	-0.000	0.004	-0.029***	0.001	-0.000
	(0.009)	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)
2-5 years	-0.045***	-0.006	0.001	-0.046***	-0.005	-0.005
	(0.008)	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)
5-10 years	-0.039***	0.001	0.010	-0.043***	0.003	0.002
	(0.008)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)
>= 10 years	-0.047***	-0.006	0.007	-0.054***	-0.005	-0.004
	(0.008)	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)
Education						
College degree	-0.036***	-0.027***	-0.027***	-0.034***	-0.027***	-0.023***
	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)
Advanced degree	-0.035***	-0.018***	-0.011	-0.040***	-0.020***	-0.012*
	(0.010)	(0.007)	(0.007)	(0.009)	(0.007)	(0.007)
Constant	3.037***	1.729***	2.148***	2.210***	1.740***	1.762***
	(0.037)	(0.036)	(0.034)	(0.041)	(0.040)	(0.036)
N	15650	15650	15650	15650	15650	15650
Adj R^2	0.093	0.476	0.410	0.261	0.478	0.475

Notes: Data from the 2004 W ERS. Sig asterisks: *10%, **5%, & ***1%. All regressions include race and occupation dummies.

Regression 1 in Table 3.2 contains the baseline results and, consistent with standard economic intuition, the wage is strongly positively correlated with job satisfaction. The estimated coefficient implies that a 10% increase in the wage is associated with an 0.5% increase in job satisfaction. For the present purposes, one other coefficient is worth noting: being a supervisor is strongly positively correlated with job satisfaction.

The problem with this baseline specification is that, besides occupation dummies and the wage, it makes no attempt to control employee voice or autonomy. Regression 2 in Table 3.2 reports the results of a regression with the addition of the indexes of autonomy and voice. There are a number of important changes when voice and autonomy are included.

First, the estimated coefficient on the wage decreases by about 50%. Although the coefficient is still positive and significant, it now implies that a 10% increase in the wage results in only a 0.22% increase in job satisfaction. Considerably smaller than the 0.5% increase in Regression 1. Second, the estimated coefficients on autonomy and voice are both positive and significant, and are 12 and 14 times larger than the coefficient on the wage, respectively. A 10% increase in autonomy is associated with a 2.3% increase in satisfaction, while 10% increase in voice is associated with a 3% increase in satisfaction. Notice also

that the Adjusted R^2, measuring the proportion of variability in the data accounted for by the regression model increases from 0.093 in Regression 1 to 0.476 in Regression 2. The results of a Wald test comparing equivalence of models 1 and 2 is rejected at the 0.01 level (Prob > F = 0.0000). This finding is robust to likelihood-ratio tests (Prob > χ^2 = 0.0000). F-tests across coefficients reject the joint hypothesis that autonomy and voice are zero (Prob > F = 0.0000) and the hypothesis that the wage is equal to zero (Prob > F = 0.0001).

Based on the results in Regressions 1 and 2 from Table 3.2 two conclusions can be drawn: the wage, voice, and autonomy are all important correlates of job satisfaction, and, voice and autonomy are an order of magnitude stronger correlates of job satisfaction than the wage. However, as is clear from the preceding discussion, there are a number of estimated coefficients that are not robust to the inclusion of autonomy and voice. This raises important questions about the nature of the correlation between voice and/or autonomy, and the other variables in the regressions.

The estimated coefficients on the wage, supervisor, union, tenure, and education all see changes of at least 50% when voice and autonomy are included. And, in every case, the movement is towards zero. For the present purposes, the most pertinent changes are on the wage and on being a supervisor. We will deal

with each in turn. The changes in the remaining coefficients are certainly interesting, as it suggests the over-arching importance of voice and autonomy in shaping job satisfaction, but are beyond the scope of this paper.

Regressions 3 and 4 help understand what is driving the large change in the coefficients on the wage and the supervisor dummy, respectively. Regression 3 includes only autonomy and the wage, excluding voice. The bulk of the decrease in the coefficient on the wage is due to the inclusion of autonomy alone, reflecting a positive correlation between the wage and autonomy. An auxiliary regression supports this claim, thought the results are not reported here. In a regression of the wage on the same set of controls used in the satisfaction regression, the coefficient on autonomy is statistically significant and positive. The specific reasons for this correlation are not important. What is important, and will be discussed in more detail below, is the fact that autonomy and the wage move together.[21]

Regression 4 includes only the wage and voice, excluding autonomy. Voice also has important effects on the estimated coefficients on the wage and being a supervisor. With only voice, the coefficient on the wage is smaller,

[21] There are two likely candidates as explanations for this correlation. The first is a ``good jobs" effect. Some firms simply offer their workers higher wages and more autonomy, and workers with these types of employments report higher satisfaction. Alternatively, principal-agent models of worker motivation also imply that the wage and autonomy should be positively correlated (e.g. Bowles and Gintis, (1985), Alchian and Demsetz, (1972), Shapiro and Stiglitz (1984), Guy and Skott (2007)).

though not nearly as small as with autonomy. This suggests a small positive correlation between voice and the wage, though in results not reported here it is found that this correlation is not statistically significant.

Voice and autonomy have almost equal impacts on the coefficient for being a supervisor, highlighting a potential pitfall with the estimation. Given that voice and autonomy are, by definition, higher for supervisors, it could be possible that supervisors are driving the results for autonomy and voice in Regression 2. If it were true that workers who put a higher value on voice and autonomy are more likely to be supervisors, we would expect that supervisors should exhibit higher levels of job satisfaction than non-supervisors before controlling for voice and autonomy. Further, when voice and autonomy are controlled for, the value that supervisors place on voice and/or autonomy should be higher than non-supervisors, and the difference in average job satisfaction between supervisors and non-supervisors should be smaller.

Regression 5 of Table 2 reports the results of a regression identical to Regression 2, with the addition of three interaction terms. The results are quite striking. First, with the inclusion of the interactions, the coefficient on the supervisor dummy becomes negative and insignificant. There is no average difference in job satisfaction between supervisors and non-supervisors. Second,

the direct effects of autonomy and voice, respectively, are statistically identical to those reported in Regression 2. Finally, the interaction between autonomy and the supervisor dummy is positive and significant, while the interaction with voice is negative and significant.

The stability of the direct effects of autonomy and voice is important because it demonstrates that the positive correlations between autonomy and satisfaction, and autonomy and voice are not driven by differences between supervisors and non-supervisors.

3.6 Discussion of Results

Before discussing the broader implications of the results, a brief summary is warranted. It is found that, without including measures of autonomy and voice as regressors, job satisfaction has a statistically significant positive correlation with the wage level. However, there are many aspects of firm organization that could shape job satisfaction that are omitted from this regression. Specifically, when indexes of autonomy and voice, respectively, are included in the satisfaction regression, the correlation between the wage and satisfaction decreases by roughly 50%, but remains positive and significant.

Further, the elasticities of satisfaction with respect to voice and autonomy, respectively, are 12 to 15 times large than the elasticity with respect to the wage. Most importantly, the proportion of variation of satisfaction explained by the inclusion of autonomy and voice into the regression specification (Regression 2 vs Regression 1 in Table 2) shows much higher precision. Standard ANOVA F-tests comparing models support this claim. The amount of autonomy and voice that a worker has over the firm is an important omitted variable, biasing the estimated coefficient on the wage upwards. And, conditional upon having a job, voice and autonomy are considerably more important determinants of job satisfaction than the wage. Our results are consistent with similar empirical studies, including Bartling, Fehr, and Schmidt (2009), and complement Dube and Freeman (2008), where it is found that revenue-sharing payment schemes only improve worker productivity when workers also have influence over their workplace.

By showing that autonomy and voice result in such large changes in the proportion of variation of satisfaction, our identification strategy suggests as strong causal relationship. Note, however, that one may argue that there is a problem of reverse causality between tenure and satisfaction. We should be clear that we are not interested in precisely establishing causality here. For us, what

really matters is that the association between satisfaction and tenure is consistent with evidence from the literature. We find this to be the case. For example, Freeman (1978) reports that job tenure has "virtually no effect on job satisfaction," but is associated with much lower quit rates. All reported regression specifications with our voice index are consistent with this finding. It may be then that when people have voice, tenure does not matter, and when people do not have voice, they tenure is negatively associated with satisfaction.

3.7 Conclusion

The results leave open the reasons *why* satisfaction is so influenced by job characteristics that support worker autonomy and voice. The behavioral program of study initiated by Deci (1971), which may contribute to an explanation, evaluates the interaction between intrinsic and extrinsic motivators. In short, self-determination theory (Deci and Ryan, (1985), Deci and Ryan (2000a), and Deci and Ryan (2000b) suggests that the effect of an external intervention can support or thwart satisfaction depending on how the extrinsic institution affects the sense of (1) *autonomy*, or the desire to self-organize own actions and for their actions to be causal, (2) *competence*, or the desire to feel

capable of affecting surroundings, or (3) *relatedness*, or the desire to feel connected to and respected by a social group. Extrinsic incentives can crowd out satisfaction if they are perceived as controlling, but conversely can crowd in satisfaction if perceived as supportive of one's psychological needs. Self-determination theory explains our findings quite well as both increases in worker autonomy and voice support one's sense of autonomy, competence, and relatedness in intuitive ways.

Based on the experienced utility of the employed, job-seekers would do themselves a favor by trying to obtain jobs that will give them more voice and autonomy since they will likely find them satisfying. We've shown that the wage earned is important for job satisfaction, but it should not be given much weight compared to voice and autonomy. A natural extension of our study would investigate how much weight job-seekers give to the wage, voice, and autonomy during their search. If it were true that job-seekers only consider wage characteristics, they run the risk of being unsatisfied with their job. Alternatively, if job-seekers do indeed make good forecast, economists will have to re-think the traditional assumptions that wages drive search behavior commonly made in models of employee search and matching.

CHAPTER 4

INDUSTRIAL RELATIONS IN CONSIDERATION OF BEHAVIORAL AND EXPERIMENTAL ECONOMICS

"[]Just 15 years ago, research on incentives in organizations consisted to a disappointingly large extent of (a) identifying a 'paradoxical' feature of some internal labor market (for example, academic tenure, or pay raises based purely on seniority), then (b) crafting a theoretical model showing how that feature was in fact an efficient response to some contracting problem. Most analyses stopped there."-- Charness and Kuhn (2010)

4.1 Introduction

The identification of facilities and circumstances that motivate labor has been, and remains, of central academic and practical significance. Many "rigorous and analytic" (Lazear, 1996) models of motivation have been developed by economists, but the vast majority are (1) rooted in the rational choice model (RCM) where all agents are assumed to be both rational and solely motivated by their own private material interest, and (2) entirely without context, thereby eliminating the potential role of complementary institutions supportive of a motivational framework. When, do these caveats hold? Or more to the point, how well do theories of motivation rooted in the RCM predict employee performance?

To investigate these issues, one would ideally compare firms selected at

random that only differ in one salient, structural dimension. The 'real-world,' unfortunately, does not produce these data. Further, when using firm-level data, it is impossible to fully control for all firm characteristics and processes when trying to isolate causal relationships of interest. The appeal of using controlled experimental techniques is precisely because of the ability to mitigate many of these issues and better focus on the precise question of interest. Although still a relatively new research methodology, experiments in economics have helped influence many aspects within the broader economics discipline.[22] Some of the major contributions might be crudely summarized in the following three ways.[23]

First, experiments in economics have illustrated that the RCM predicts real-world behavior in some settings-- such as those in competitive experimental

[22] One example that supports this claim can be taken from the recently 2009 ISI Journal Citation Reports where the The journal *Experimental Economics* was rated as having an the 10th largest impact factor (measuring the average number of citations to articles published in science and social science journals) out of 245 economics journals evaluated.

[23] It is well known, however, that subjects that participate in an economics experiment might misrepresent their own wants and desires due to any number of biases (i.e. trying to impress the experimenter or other subjects) that threaten both the internal and external validity of the experiment, thereby making inferences made from the data unreliable. Some threats to the internal validity of a project include a decision task for subjects that is not salient and non-random sampling of subjects. As an ideal robustness check, the results and evidence found from a specific experimental protocol should be repeated under the same set of controls more than once. Falk and Fehr (2003) suggests that external validity for an experiment may hinge upon whether the relevant conditions in the experiment parallel those in the real-world, and second, whether or not behavioral regularities observed in the lab persist in new situations when the relevant underlying conditions remain substantially unchanged. That is, if an experiment implements certain conditions that generate robust and replicable regularities, Falk and Fehr (2003) argue that we can inductively reason that the same behavioral regularities are likely to occur under similar conditions that would put an individual in the same cognitive state. Indeed, a major application of experiments has been precisely to 'testbed' mechanisms and policies that have either not been implemented in the real-world, or the data needed to test such policies are difficult to obtain – including many incentive schemes and governance facilities thought to influence the level of worker productivity as will become apparent below.

markets or in the presence of arbitrage -- and miserably in others -- such as in ultimatum or trust games. That is, rationality and self-interest are now commonly interpreted as contextually dependent behaviors. For example, Smith (1991), Chu and Chu (1990) and later Cherry et al. (2003), Brown et al. (2004), Henrich et al. (2001), Carpenter (2005), and Hoffman et al. (1994) all show that in the context of markets and market-like institutions, we typically observe behaviors that are very consistent with the assumptions in the RCM framework. A common point of emphasis among these studies is that *complete* market institutions support a very specific and well defined set of behaviors that appear to be self-serving and rational.

Second, experiments have illuminated a number of cases where deviations from the RCM are systematic, common, and replicable. Robust behavioral tendencies in controlled settings have already led to the emergence of many influential theories in Economics. For example, scores of findings from experiments conducted by Daniel Kahneman and Amos Tversky throughout the 1970's and 1980's led to the development of Prospect Theory. More recently, the lab experiments have repeatedly helped show that many individuals have strong preferences for the material well-being of others leading to the development of utilitarian models of other-regarding and social preferences (e.g. Fehr and

Schmidt (1999), Bolton and Ockenfels (2000), or Charness and Rabin (2002)).

Third, experimental economics has produced specific examples of how cultural norms influence individual behavior. Where economists are trained to only consider the role of relative prices in guiding behavior, experiments have helped give estimates over how much social instruments such as communication (e.g. Sally (1995)), social sanction (e.g. Ostrom et al.(1994), Masclet et al. (2003), and social reward Carpenter and Seki (2005)) influence individual behavior.

In the following chapter we review (mostly) experimental economic work that has evaluated personnel models of motivation. In Section 1 we review the behavioral effect of different incentive contracts; in Section 2, we review studies that evaluate the role of general labor market conditions on worker behavior; in Section 3 we examine some experimental work on the role of loyalty, job satisfaction, and intrinsic motivation, and in Section 4, we review work that examines the motivational effect of different firm governance structures. In the final section, we conclude.

4.2. Incentive contracts

Mitchell, Lewin, and Lawler (1990) suggest that incentive contracts can be defined as payment schemes that link pay to either individual or group output.

In the following section, we present a summary of reported behaviors from controlled experiments of different compensation schemes that have been evaluated in the lab.

4.2.1 Piece-rates

Piece-rates are incentive contracts that guarantee a pre-determined level of payment for each unit of output produced. For all practical purposes, piece-rate contracts are non-strategic, utility-maximization problems. The benefits of production are well defined and the costs are private and unique to each employee. In theory, piece-rates shift all risks, costs, and benefits of labor onto employees after the commission rate is chosen by the employers. In a traditional game-theoretic framework, the price-rate is found by solving for the optimal rate, given the best-response of the employee. When we assume a linear production function, full-information in production, zero uncertainty, and know the risk-preferences of workers, finding the optimal piece-rate that would motivate labor is a straight-forward exercise. These conditions, however, never hold in practice.

Laboratory experiments designed to test the efficacy of piece-rate contracts reveal that when subjects are given their private cost function and also

the production function of a homogeneous good, behavior conforms to the income-maximizing value of effort (Bull et al., 1987). Similar findings hold in a real-effort experiment (where the cost function is endogenous and unique to each worker) conducted by van Dijk et al., (2001). They further find, however, that piece-rates result in lower mean outputs compared to compensation schemes based on relative performance. Shearer (2004) finds evidence from a field experiment conducted on a tree-farm that piece-rate schemes increase the productivity of workers by roughly 20% when compared to fixed-wage payments. Shi (2007) uses two field-experiments to again find productivity increases when compensation contracts switch from fixed wage to a piece-rate scheme. In addition to the costs associated with finding the optimal piece-rate in practice, piece-rates cannot be implemented in most occupations since they require perfectly measurable output. Relative performance based payment, therefore, may offer a viable alternative which we turn to below.

4.2.2 Rank-order tournaments

The model in Lazear and Rosen (1981), shows that when the cost of monitoring effort is prohibitively high, compensation based upon one's relative

performance in the firm can be as efficient, and under certain conditions, superior to piece-rate incentive schemes. They consider the incentive properties of a rank-order payment scheme that awards predetermined prizes that are distributed based on the relative performance of the participants. By assuming that all workers are identical, and their behavior is consistent with the RCM, they show that (1) the effort of all workers will increase with an increase in the prize spread, (2) workers' effort is independent of the fallback (loser's) wage, and (3) workers can provide the same level of effort and end up being paid different amounts. Furthermore, Lazear and Rosen (1984) suggest that by paying workers on the basis of their relative performance, employers save on measurement costs and the nature of the risk borne by workers when compared to piece-rate incentive schemes. Williamson et al. (1975) suggest that the possibility of vertical promotion could act as a substitute to excessive monitoring by rewarding workers that exhibit 'consummate' behavior over time. In Lazear and Rosen (1989), however, the authors speculate that the behavior of workers in tournaments could be affected by the perceived fairness of the ranking.

The experimental research has revealed behavior consistent with theory insofar that tournaments and piece-rates typically result in comparable levels of effort, but experiments also reveal that the variance of effort under tournament

compensation schemes is usually much higher than what is theoretically predicted. This particular pattern has been noted in chosen effort lab experiments (Bull et al. (1987)), real effort experiments of van Dijk et al. (2001), and the field experiments conducted in Shearer (2004). As Drago and Heywood (1989) suggest, high variance of effort could lead to "costly and unexpected inventory buildups, shortages, and production bottlenecks." Another potential drawback of tournaments highlighted in an experiment by Carpenter et al. (2007) is that tournaments elicit a much higher tendency of sabotage. They find that the positive incentive effects of a tournament are effectively canceled out due to sabotage when compared to a piece-rate compensation scheme.

Frank and Cook (1995) further worry that tournament compensation schemes might be *too* attractive to workers. Because of the strong behavioral tendency for individuals to be overconfident, they fear that workers would select too frequently into a tournament contract, even when it may not be in their best material interest to do so. In an experiment aimed partly to test the validity of this claim, Vandegrift et al. (2007) find only "modest overcrowding"of tournaments across various conditions. Niederle and Vesterlund (2007), however, find evidence from the lab that preferences for operating in competitive environments is quite gendered with men selecting into tournament

compensation schemes over twice as women of equal ability.

Although some guidelines of tournament design can be found throughout the personnel literature that might aid in selecting the optimal size of the prize, or what the prize spread should be (e.g. Lazear and Rosen (1981)), all of these models are constructed with a preference bias. For example, it may be that individuals may have social preferences but love to compete, or they are only concerned with their private payoff, but do not want to compete etc. Rather than assuming a specific set of preferences, Freeman and Gelber (2010) use experiments to investigate if and how different variations in parameters of the tournament design such as the number of competitors, number of prizes, and the prize spread, affect behavior under tournament incentives. They find that effort in a real-effort experiment (solving-mazes) is lowest when payments are independent of performance; higher when a single, large prize is given; and highest when multiple, differentiated prizes are given. Although most tournaments are designed to pit workers against one another, many other compensation schemes have been based on cooperative rewards, which we turn to below.

4.2.3. Group rewards: Profit Sharing and Gainsharing

Gainsharing and profit sharing incentives respectively link wages of employees to the performance of group or firm accomplishments. In general, profit sharing takes the form of end-of year cash bonuses and/or the deposit of bonuses into retirement funds linked to the profitability of the firm, whereas gainsharing plans split the financial value of gains to the firm when workers exceed performance targets to be paid to employees as they occur. Because the payoff to any single individual is tied to the performance of the group as a whole, the success of group reward schemes is contingent upon the capacity to sustain high performance from all employees despite the presence of free-riding incentives that arise when private contributions are both (1) difficult to verify, and (2) the extra private payoff associated with the marginal contribution of any worker is diluted by a factor of 1/n. Within the RCM the 'free-rider problem' would prevent group incentive schemes from being effective yet the evidence from the empirical record is suggestive that this need not necessarily be true. In their meta-analysis on a series of projects investigating profit sharing incentive plans in real-world firms, Weizman and Kruse (1990) conclude that profit sharing has at least a weak-positive correlation with firm productivity. This finding

rightfully raises questions regarding if, how, and when free-riding affects groups and whether there are lessons to be learned from successful cases that can inform the development of general strategies to sustain cooperative behavior in similar social dilemmas.

In a world without context and comprised entirely of rational agents, the folk theorem of noncooperative game theory gives plausible stories of how group incentive schemes can overcome free-riding in repeated interactions when the discount rate of agents is sufficiently small so that future outcomes are valued similarly to present outcomes (Fudenberg and Maskin, 1986). Under these conditions, cooperation can be rationally preferred and sustained through time by egoists in infinitely repeated games when all, or some fraction of agents (depending on the parameters defined by theory) share the exogenous traits for specific, and credible 'tit-for-tat' or 'grim reaper' strategies.

Furthermore, reputation has been a phenomena studied extensively in understanding the strategic behavior of economic actors in a variety of settings. The Kreps and Wilson (1982) 'reputation effect' model shows that incomplete information regarding the preferences of other players (egoist or altruist) is sufficient for an egoist player to play a cooperative strategy because of the unverifiable belief that the population may have some actors that will play a

cooperative strategy. When evaluated in the lab, Andreoni and Miller (1993) find evidence from a series of experiments in repeated PD games that subjects were much more cooperative than predicted by Kreps and Wilson, suggesting that a non-trivial portion of their subjects were demonstrating altruism beyond the purpose of building an otherwise 'false' reputation. Cooper et al. (1996) test and compare theories of reputation building with theories of altruism that explain cooperation in both one-shot and finitely repeated PD games, but are unable to find strong evidence in support of either theory, instead concluding that the context of the interaction strongly drove the behavior of their subjects.

A major tool used in experimental economics, voluntary contribution mechanism games (VCMs) have been used to study free-rider issues by creating an environment where the option to free-ride is perfectly salient. This is done intentionally so that when subjects decide not to free-ride, we can confidently conclude that they did not want to free-ride. Specifically, a standard VCM requires each subject to decide if, and how much they want to contribute to a collective project and the payoffs are calibrated so that full contribution to the collective project from all group members would result in a Pareto optimal outcome. Each subject, however, has a private, material incentive to free-ride for all strategies played by the other subjects in their same group. Over the course of

20-30 years, data collected in the lab through hundreds of versions of VCM games provide a handful of general lessons and behavioral regularities that provide insight into fundamental questions regarding (1) whether the incentives to free-ride lead to free-riding behavior, and (2) to the extent that incentives to free-ride do lead to free-riding behavior, how free-riding behavior can be successfully mitigated in the lab. The goal is to use VCMs to study how free-riding behavior might be overcome in an environment that strongly supports free-riding. It is dangerous to generalize but the experimental record of VCM games has produced some lessons for the management of the free-rider problem that are widely recognized across the social sciences.

4.2.3.1. Lesson 1

In the absence of institutional controls, free-riding increases

Although the idea that strong material incentives to free-ride will lead to free-riding behavior seems intuitively obvious, experiments routinely find the tendency for subjects to act in manners that defy the predictions of theory. In the baseline linear VCM public goods game, mean contributions from subjects hover near 50% of their endowment in the first few rounds, with a steady decline towards the Nash equilibrium of mutual defection after only a few rounds of

play. The significance of this result adds credence to the fear that group incentive payment schemes might not work by themselves since free-riding likely occurs in the absence of institutions that counteract this tendency.

4.2.3.2 Lesson 2

Inter-group competition induces high intra-group effort.

Nalbantian and Schotter (1997) test how well alternative types of group incentive systems explain behavior in the lab. They investigate the behavior of groups under (1) simple revenue sharing partnerships (modeled and tested as a linear VCM), (2) target-based systems such as profit sharing (modeled and tested as a linear VCM with a provision point target mechanism), (3) gainsharing (modeled and tested as a linear VCM with provision point target that generated endogenously by the previous output of workers), (4) a group tournament (where payoffs to the group were contingent upon relative, rather than absolute, performance of the group against the performance of other groups, and (5) an individual wage-cum-supervision mechanism (where the firm offers a wage to workers provided that they provide an effort e* when on the job with a probability p of being monitored each period; if the worker is caught working at an effort lower than e*, she is fired). The novel finding reported in their paper is

that group competition mechanisms led to both the highest mean output with relatively low variance. They also re-validate previous findings from PG experiments that only high levels of monitoring are effective in mitigating free-riding. Their data indicate that only profit sharing and revenue sharing contracts had lower variance but they were Pareto dominated since most of the data showed subjects playing the Nash strategy. These results are consistent with the findings from Erev et al. (1993), and Gunnthorsdottir and Rapoport (2006) that show how intergroup competition markedly reduces free riding -- hinting at the importance of team identity in overcoming social dilemmas in team production. We return to this in section 3 below.

4.2.3.3 Lesson 3:

Forcing contracts can work/higher provision points induce higher contributions.

As mentioned above, Nalbantian and Schotter evaluated group behavior VCM's with exogenous and endogenously established targets meant to serve as proxies for profit sharing and gainsharing incentives schemes respectively. They note that the design of these contracts are a test of the forcing contract solution to the free-rider problem proposed by Holmstrom (1982), where workers would share in any revenues generated that exceed the pre-established target and

would receive a significantly lower penalty wage if they were to fail in exceeding the target. Theoretically, forcing contract VCM games can have multiple equilibria similar to an assurance game (if contributions are not returned) some of which can include positive contributions from rational individuals. Various forcing contract VCM games in the lab have been evaluated, and results show that subjects typically do not settle on the (socially optimal) cooperative equilibrium (e.g. Isaac et al. (1989)) In contrast to these studies, however, Spraggon (2002) identifies two forcing contract instruments that either tax (punish) or subsidize (reward) subjects depending upon the relative distance of the group total to the optimal level of contributions. Both instruments result in socially optimal contributions. Spraggon's study provides evidence that exogenous targeting instruments can be designed to mitigate free-riding in groups.

4.2.3.4 Lesson 4

Punishment works.

Anderson and Stafford (2003) find that the increased probability of receiving a punishment has a smaller effect in inducing high contributions than does the severity of a punishment. Dickinson (2001) shows that voluntary

contributions increase by 10-28% in groups when both punishment (sticks) and rewards (carrots) are made available to players. Andreoni et al. (2003) also find that the institutional arrangement where rewards and punishment are present induces high levels of cooperation in proposer-responder games. Their findings suggest that rewards and punishments by themselves are not as effective as a combination of the two in inducing cooperation.

As Masclet et al. (2003) show, punishment need not be monetary. They conduct an experiment to evaluate the effectiveness of monetary versus non-monetary punishments. Their results suggest that monetary punishment sustains higher contributions than does non-monetary sanctions--but not by much. Non-monetary sanctions such as expressed disappointment and disapproval induce higher contributions to the public good. They also find, perhaps unsurprisingly, that non-monetary sanctions are more effective in partner treatments, than in stranger treatments. Carpenter and Seki (2005) also present results from a field experiment showing that disapproval in the form of a picture of an unhappy face can induce higher voluntary contributions to the public good.

4.2.3.5 Lesson 5

Communication works

It may be reasonable to assume that communication among individuals is a vital instrument in situations where socially desirable outcomes require coordination. Ledyard (1995) singles out communication and the marginal per capita return to be the two most important variables in obtaining cooperative solutions in public goods games. Sally (1995) conducts a meta-analysis of social dilemma games over a 35-year period and finds that when communication is allowed, cooperation increases circa 40% over the baseline "institution-less" environments. The primary forms of communication that are commonly introduced as treatment variables in the experimental literature are cheap-talk, and face-to-face communication.

Cooper et al. (1992) provide evidence of how pre-play "cheap-talk"—or costless, non-binding, non-verifiable communication that does not affect the actual payoffs -- can help achieve coordination in two-player coordination games with multiple Nash equilibria with a Pareto-dominant equilibrium. They note that the addition of a cheap-talk treatment does not eliminate any equilibrium outcomes of the original game, concluding that at least theoretically, cheap-talk should be a transparent variable action that can (should) simply be ignored.

They find that cheap-talk can help coordination, but the rules or institutions that govern the interaction also matter. That is, different cheap-talk regimes may have different effects on efficient play.

The role of cheap-talk has been evaluated extensively to unpack its robustness as an efficiency and/or fairness-enhancing mechanism under a slew of different conditions, including its impact in ultimatum bargaining outcomes (Croson et al. 2003) and even in its role in identifying possible scenarios in which R&D cooperation can be obtained (Suetens 2005). Duffy and Feltovich (2002) compare the effectiveness of cheap-talk with the effectiveness of the subject's access to the past observations of the other subjects. They find that effectiveness of cheap-talk is contingent on the type of coordination game being played. Specifically, they find cheap-talk to be more effective than access to the decision history of other players in stage hunt games, but the results do not generalize to the Chicken and PD games where past observations are more effective in influencing coordination among the players.

Unrestricted face-to-face communication in experimental settings require subjects to be in the same location so they can identify all of the other players. Subjects are then allowed to talk about whatever they want among themselves. In their 1994 book, Ostrom, Gardner, and Walker report in a series of

experiments that continuous face-to-face communication more than tripled cooperation rates in a repeated CPR games; similarly, Frohlich and Oppenheimer (1998) found that cooperation tripled in VCM public goods games. Cardenas (2003) conducts a field experiment in a rural village with subjects who routinely depend on a common pool resource and finds that face-to-face communication does not have the effects observed in experiments in Ostrom, Gardner, and Walker. Instead, Cardenas finds that individuals who do not depend on the resource as much as the other parties-- because their financial status allows them allows them to 'go to market'—are not trusted by the other subjects, despite the many (false) promises made when face-to-face communication is allowed.

4.3. Labor-Market Conditions

Several theories investigate the relationship between job rents, unemployment, and productivity. The main theoretical models in this tradition share the common intuition that both the unemployment rate and wages are positively correlated with worker effort.

The intuition of the Shapiro and Stiglitz (1984) model suggests that wages that exceed the competitive market rate would reduce shirking, but if every firm

were to raise their wages workers would again begin to shirk since they are no longer earning a rent. It follows, however, that because all wages rise the demand for labor would decrease and create a positive level of unemployment, and with positive unemployment, workers would therefore not shirk since they would not immediately be able to obtain another job if they were fired. In short, the model predicts that the level of effort by workers is determined by both the unemployment rate and the level of wages.

The Akerlof 1982 model is based on the notion that worker effort depends on the work norms of the relevant reference group. He states that the firm can raise group work norms and hence mean group effort by paying workers a "gift" of wages in excess of the minimum required to keep workers, and would in return receive effort higher then the minimum required to stay employed. Akerlof and Yellen (1990), build upon this framework and put forth the slightly different *"fair-wage hypothesis"* which states that workers form a notion of a 'fair-wage' and will either give more or less effort if the wage they are being paid is higher or lower than the fair-wage level. We now turn to the empirical evidence aimed to test the predictions of these models.

4.3.1 Testing labor market conditions in the lab

The incentive to work hard in an efficiency wage model is a function of the *rent* received by a worker. The job rent can be created in two ways-- either by paying a higher wage than the reservation wage of a worker, or by reducing fallback position of the worker. The common interpretation of the fallback position throughout much of the efficiency-wage literature, is that it is a function of the unemployment rate, or more more broadly as the opportunity cost of employment, or "the availability of other jobs, unemployment insurance, and the like." Bowles (1985)). The basic intuition that drives the connection between effort and labor market conditions is such that when employment opportunities are in low supply (i.e. high unemployment), employees would be more apt to give full effort since they cannot easily find another job, whereas when employment opportunities abound, the opposite is true.

In non-experimental empirical studies, this relationship has been used to explain the rates of productivity growth (Weisskopf et al. (1983), the incidence of strikes in the U.S. (Schor and Bowles (1987)), and the rise in work intensity in the U.K during the 1980's (Schor, 1987). International comparisons vary considerably across countries and the effects of unemployment on productivity appear

strongest where industrial relations are most 'confrontational' (Weisskopf, (1987)). Green and Weisskopf (1990) find evidence that show how the worker discipline effect of unemployment affects different industries with different intensities.

Turning to the lab, Falk et al. (2005) offer experimental evidence that contradicts standard labor market models that assume minimum wages do not affect the labor supply schedule. They find that minimum wages have significant and lasting effects on subjects' reservation wages which persist even after the minimum wage has been removed. They further suggest that because of this, profit-maximizing firms may find it optimal to increase employment after the introduction of a binding minimum wage since fewer offers would be rejected by prospective employees. The 2-stage experimental framework in Brandts and Charness (2004) is similar, but workers can offer discretionary levels of effort (instead of simply accepting an offer). They use gift-exchange games to study the effect of two dimensions of a labor market conditions. They consider both 1) the impact of a competitive imbalance in an experimental labor market by creating an excess supply of firms or an excess supply of workers in the market, and also 2) the effects of a minimum wage in the market with excess supply of workers. They find that competition does not have a significant impact on either

wage offers or efforts relative to standard findings in gift-exchange and only limited evidence that workers offer less effort when a minimum wage is imposed.

Brown, Falk and Fehr (2008), find that tight labor market conditions do not necessarily lead to decreases in productivity when relational contracts-- or contracts that are based upon a relationship of trust between parties-- are established through a gift-exchange with contingent-renewal. They do find, however, that high-wage, high-effort long-term employment relationships are not as frequent in the slack labor market treatments they conducted in Brown, Falk, and Fehr (2004). These papers demonstrate that the rate of unemployment does not have a large effect on effort in incomplete labor contracts. When markets *are* complete, however, they show that the presence of competition on one side of the market leads to indifference over the identity of the trading partner and a concentration of rents to the short-side of the market. This is also a common finding in competitive bargaining games. For example, Roth et al. (1991) conduct an ultimatum game with one-to-one matching and a 'market game' in which a single responder can agree to an offer from nine agents on the other side of the market. The competition among proposers drives offers down and acceptance rates up.

4.3.1.1 Threats

Where Shapiro and Stiglitz (1984) show how a worker would avoid shirking provided that the equilibrium unemployment rate were sufficiently large, Bowles and Gintis (1985) further stress *how* employers might exploit their 'short-side power' resulting from positive unemployment-- specifically by using threats and sanction of an employee. Although studies that evaluate the effects of the threat of expulsion in experimental labor markets are rather limited, Cinyabuguma et al. (2005) show that when members of a group had the opportunity to vote to expel fellow group members, contributions rose to nearly 100% of endowments in a linear VCM game. Croson et al. (2003) find evidence that incredible threats of future actions can influence outcomes in Ultimatum Games, both in the short- and long-term, while Knez and Camerer (1995) conduct ultimatum games where players received a known outside option if the initial offer is rejected. They find that the rate of rejected offers was higher than what is observed in typical UG environments.

4.3.2 Gift exchange game

The gift-exchange game is usually set up so that the principal makes the initial wage offer to a worker and workers have the opportunity of accepting them (usually without the opportunity of making counter-offers) followed by the choice of workers of a privately costly effort level. The payoff framework for a gift-exchange game is therefore similar in many respects to a sequential prisoner's dilemma since the payoffs are structured so that higher wages yield lower monetary payoffs for firms and higher ones for workers (holding effort constant) and higher effort levels have the reverse effect on respective payoffs for firms and workers (holding wages constant), but the combination of high effort and high wages is the Pareto optimal outcome. Classical game theory predicts that workers would follow their material self-interest, and subsequently choose the lowest possible effort level irrespective of the wage offer. In anticipation of this, fully rational firms will only make the lowest possible wage offer.

In a series of experiments aimed at examining the behavior of people under variants of the gift-exchange game, economists have found a strong tendency for people to reciprocate high offers (framed both as wages and prices) with high responses (framed as quality or effort). Fehr et al. (1998), Fehr et al.

(1993), Gachter and Falk (2001), and Charness and Haruvy (2002), Maximiano et al. (2007) are a subset of an extensive experimental literature that show a regular behavioral pattern that supports the basic gift-exchange hypothesis in labor market settings. In contrast to these studies, however, Rigdon (2002) found that a double-blind gift-exchange with a higher cost of effort resulted in much higher levels of free-riding. Other studies note the importance of group effects in gift-exchange (Hannan et al. (2002)), and how asymmetric, or incomplete information of the surplus can further affect reciprocal behavior (Sadreih et al. (2003)).

Gneezy and List (2006) note that the process of implementation of the gift wage is large factor in the levels of reciprocation. Specifically, they find that when subjects are surprised with a higher wage than they expected, they work harder than workers earning the same wage without being surprised. These findings are in line with a more general notion of reciprocal behavior where individuals tend to respond positively to friendly actions, and negatively to hostile ones (e.g. Berg et al. (1995), Fehr and Gachter (2000), Andreoni et al. (2003)) and Falk (2007)).

4.4. Non-pecuniary motives

"If one could enhance a common interest in nonshirking in the guise of a team loyalty or team spirit, the team would be more efficient... Obviously the team is better, with team spirit and loyalty, because of the reduced shirking.... [Loyalty] can be preached with an aura of moral code of conduct-a morality with literally the same basis as the ten commandments- to restrict our conduct toward what we would choose if we bore our full costs."
-- Alchian and Demsetz (1972)

Through the years it has become less obvious that remuneration is always the most effective way to increase worker motivation and satisfaction. For example, a study conducted by Brulin and Nilsson (1991) evaluated 1500 randomly selected projects intended to reduce job stress and report that these projects led to significant improvements in employee satisfaction and motivation. They also found that these projects led to a reduction in production errors and better delivery times. Terra (1995), moreover, found that the implementation of self-regulating work-teams was strongly associated with a decrease in employee absenteeism related to sickness in the canning industry, and Dale-Olsen (2006) found that fringe benefits reduced turnover more than equivalent wage increases.

Personnel economists usually focus on the influence of monetary variables and have not made the same inroads into understanding psychological and sociological dimensions in explaining motivation. This is partly due to the modeling restrictions that accompany the RCM, but also because of an absence of

strong empirical findings to build off of. We posit that experimental methods offer great potential in bridging what has been studied in related disciplines with Economics. In this section we review non-pecuniary motivations and behavior that have their inspiration in industrial and social psychology with support from the experimental economic record.

4.4.1 Identity

Akerlof and Kranton (2005) define *identity* as a person's self image — both as an individual and as part of a group. Building upon insights from Psychology on the effects of in-group favoritism and out-group discrimination, they model two ways of incorporating identity into the general discussion regarding work and work incentives. The first model highlights the idea of forming an identity with the goals of the firm. They claim that economists have overlooked the significance of "the identification of the officeholder with the office" and its motivational consequences. In this model, workers experience a loss in utility when they do not follow the rules of their superiors or act in the interests of the firm. They additionally model a second case where workers identify at the group level within the firm. In this model, the authors claim that there is an

organizational policy trade-off between the introduction of a monitor-manager and policies that foster the identification of workers with firm goals. They describe "corporate culture," similarly to Hodgson (1996) where 'culture' is interpreted as internalized preferences.

When testing the claim that group identification can suppress self-interest in favor of collective interest, the experiments conducted by Eckel and Grossman (2005) find that in a repeated VCM overt means of identification of a team do not generate greater cooperation than with random, anonymous team assignments. However, when subjects were asked to work together on an unrelated and unpaid project before playing a repeated VCM, cooperative behavior was much higher than the baseline. These findings are consistent with in Cox et al. (1991) where greater cooperative play has been observed in ethnically homogeneous groups, and Charness and Jackson (2007) also find that salient group membership affects behavior in a strategic environment.

When one's group identity is threatened in an economic interactions by an out-group, McLeish and Oxoby (2007) find that in-group individuals cooperate more with fellow in-group members. Furthermore, the authors observe greater negative reciprocity among in-group individuals when the in-group's norm of behavior are violated.

4.4.2 Trust and Trustworthiness

Although there is widespread recognition of the important roles that trust and trustworthiness play in facilitating behavior when contracts are incomplete, economists have struggled with how best to conceptualize "trust." Berg, Dickhaut and McCabe's (1995) (BDM) trust game, however, has standardized the economist's interpretation of trust and trustworthiness. There are three basic stages in the trust game. First, a proposer must decide what portion (if any) of their endowment will be sent to a responder. Whatever the proposer decides to send to the responder is multiplied by a factor greater than 1 (usually 3) and then given to the responder. The responder must then decide how much (if any) of her new sum should be sent back to the proposer. Hundreds of studies that marginally differ from the BDM baseline game aim to illuminate how different experimental conditions could affect the willingness to trust (send over the endowment), or to be trustworthy (send part of the new sum back) to better understand how to mitigate moral hazard when contracts are incomplete.

There are disagreements in the literature over the why trust or trustworthiness are observed. For example, it may be that trust is not any different from risk. Bohnet and Zeckhauser (2004), however, compare the

decision to trust a stranger in a one-shot interaction equivalent to taking a risky bet finding that first movers require a higher probability in order to trust than in situations where nature determines the outcome. This suggests that the decision to trust entails an additional risk premium to balance the costs of trust betrayal. Ashraf et al. (2006) finds that while expectations of trustworthiness explain most of the variance in trust in their study, unconditional kindness also plays a significant role. Conversely, they find that unconditional kindness accounts for most of the variance in trustworthiness, while reciprocity plays a comparatively smaller role. The findings reported by Gneezy et al. (2000) suggest that inequality aversion is not required to observe trusting behavior using a variant of the BDM game. These results indicate that we are getting closer to distinguishing between Machiavellian and pure intention of that explain observable trusting behavior, but in the meantime, we have already begun to learn what institutional factors contribute more or less trust in a relationship. Below we summarize some findings.

4.4.2.1 Past relationships

The formation of a trusting relationship is formed among a pair of people does not imply that a trusting person in one relationship would be trusting in another. Gueth et al. (1998) find support in favor of this claim, but Engle-Warnick and Slonim (2004) find strong evidence that the length of a past trusting relationship with a person greatly influences trust and trustworthiness in new relationships; shorter-lasting relationships have an immediate negative impact on trusting behavior, while longer-lasting relationships have the opposite effect.

4.4.2.2 Type of relationships

Burnham, et al. (2000) find that language matters for the outcomes in a trust game. In different variants they use the terms "counterpart," “partner” and opponent in referring to the person that an individual is matched with in a trust game. These changes in wording lead to significant differences in both trust and trustworthiness; trustworthiness more than doubles when "partner" is used instead of "opponent." Danielson and Holm (2007) report the results from an experiment in which all subjects were members of a church. When comparing

the trusting behavior (of the first mover) to that of typical student samples, they detect no differences in their findings. They do find, however, that churchgoers returned a significantly higher proportion than do students. Engle-Warnick and Slonim (2006) find that the level of trust is lower in relationships that will end at a definite time than when the relationships end at an indefinite time.

4.4.2.3 Demographic and cross-cultural differences in trust and trustworthiness:

There are several studies that essentially regress demographic variables on trust to investigate if the socio-economic characteristics of a population can explain trust. For example, Gachter et al. (2004) relate answers from a survey to experimental evidence on trust from non-student and student participants in Russia, while Fehr et al. (2002) employ a similar methodology in Germany. The results offer an interesting set of lessons regarding heterogeneous trusting behavior among the respective populations. The lesson may not be generalizable but could certainly be used as an additional consideration in the development of group-targeted policy initiatives.

More generally, Glaeser et al. (2000) find that when individuals are closer

socially, trust and trustworthiness rise and that trustworthiness declines when partners are of different races or nationalities. Similarly, Cassar et al. (2007) present evidence from field experiments in South Africa and Armenia. Subjects participate in trust and microfinance games and the study suggests that personal trust between group members and social homogeneity are more important to group loan repayment than general societal trust or acquaintanceship between members. In the lab, however, Anderson et al. (2006) find that induced heterogeneity by varying the show-up payments given to subjects did not consistently affect a willingness to trust, or to be trustworthy. Futhermore, Bouckaert, and Dhaene (2004) find that male small businessman in Belgium of either Turkish or Belgian ethnic origin exhibit levels of trust and reciprocity that are independent of their own ethnic origin, and that of the opposite party, while Willinger (2003) again finds no evidence of discrimination in an inter-cultural trust game between French and German participants. Fershtman and Gneezy (2001), however, find a systematic mistrust in Israeli Jewish society towards men of Eastern origin. Interestingly, the observed ethnic discrimination in their study was entirely a male phenomenon. Eckel (2007) finds men less trusting and trustworthy than women; yet Chaudhuri and Gangadharan (2007) in the other hand find that men are more trusting than women and that there are no

significant gender differences in reciprocal behavior. Sutter and Kocher (2007) find that trust within one's own age group increases almost linearly from early childhood to early adulthood, but stays constant within different adult age groups, while trustworthiness is constant across age groups. Colleti et al. (2005) find that sanctioning and monitoring systems aimed to improve cooperation can also increase trust among collaborators and Masclet and Penard (2007) find that reputation management mechanisms in the form of evaluations and ratings often found in online marketplaces can build trust and increase market efficiency. They compare different evaluation systems -- simultaneous evaluation, sequential evaluation, evaluation with a waiting option and find that the existence of a reputation system increases the level of trust and cooperation.

Additionally, Fehr and List (2004) conduct a field experiment to find that CEO's tend to exhibit behavior that is both more trusting and trustworthy than common student subjects suggesting that the difference is perhaps due to their recognition that trust and efficiency are often positively correlated with one another in business practices.

4.4.3 Intrinsic motivation

Akerlof and Kranton (2005) suggest that strong identification with the firm might act as a substitute for remuneration to motivate employees, and that firm identity should be understood as a "new type" of capital, since it could contribute to the profitability of the firm.[24] If one were to use Walrasian economics as a prescriptive guide for increasing the affective attachment of individuals, they would likely introduce better price (wage) instruments. Decades of work in experimental psychology, and more recently in experimental economics, has illuminated that the relationship between intrinsic and extrinsic motivators is quite complex. For example, an experiment testing the effects of different payment incentives for solicitors seeking collecting charitable donations, Gneezy and Rustichini (2000a) find who subjects that received small payments visited fewer houses than participants who were not paid anything for their effort. Gneezy and Rustichini (2000b) also found that the fines that were

[24] Beyond economics, there are few topics in industrial and organizational psychology have been studied as has the relation between one's job attitude and performance in the firm (e.g., Brief and Weiss, (2002), Judge et al. (2001), Meyer and Allen, (1997), Mowday et al. (1982), Staw et al. (1994)). Numerous meta-analyses have demonstrated that positive job attitudes, such as commitment and satisfaction, are accompanied by better work outcomes (e.g. Cooper-Hakim and Viswesvaran (2005), Harrison (2006), Meyer et al. (2002), Riketta, (2002)) giving credence to the classic managerial claim that "happy workers make better workers." Iaffeldano and Muchinshky (1985) find evidence in support of the claim that job satisfaction is positively correlated with performance, while employees who are experiencing low satisfaction in their employment are typically less creative and innovative, and more dis-attached from the goals of the firm (Amabile 1996).

implemented on parents that were late for picking up their children from daycare resulted in a significant increase in late-coming parents. Other widely cited experimental studies have shown that payment for blood donation decreased blood donation (Titmuss, (1971)), fines increased common pool resource exploitation relative to no-fine groups (Cardenas et al. (2000)), fines deceased effort in gift-exchange environments (Fehr, et al. (1998)), and the introduction of weak sanctions by the trustor or Nature lowered 'trustworthiness' by responders in trust-game (Houser et al. (2010)).

Many theories from social psychology (e.g. Deci (1971), Deci, and Ryan (1985), Deci and Ryan (2000) and also from economics (e.g. Frey and Jegen (1999), Frey and Jegen (2001), Rob and Zemsky (2001), Huck, Kübler, and Weibull (2003)), and Bowles and Huang (2008)) have all been developed to try to show how intrinsic and extrinsic motives are related. For example, it has been thought that the introduction of extrinsic motives can change either their 1) desire to organize their own actions, 2) their sense of competence 3) their social standing, 4) their moral obligation, or 5) their identity, all of which are thought to influence how individuals feel about performing a particular task. While the precise mechanisms underlying the impact of extrinsic motivators on intrinsic motivation are not well understood empirically, most theorists believe that

intrinsic motivation can be increased or decreased if it is respectively perceived as supportive or thwarting of any of the above factors.

4.5. Firm governance

Bowles and Gintis (1988) define the political structure of the economy as "the ensemble of rules governing investment, production, and distribution in economic institutions." The political structure of the firm, although embedded within the rules and regulations within a larger economic and political system, is largely unrestricted in *how* it makes firm-wide and personnel decisions. Any individual or group associated with the firm, moreover, can be assigned with the responsibility of making such decisions. While it is usually the case that a manager or a group of managers is used, ultimate control rights are said to be held by the entity within the firm that holds the manager accountable. For instance, in many large corporations, the manager is held accountable by the board of directors, which in turn is held accountable by the shareholders of the firm. The composition of the shareholders varies widely from firm to firm with equity shares held by the public, employees, a single owner, or the state. Although the ownership structure generally influences the distribution decision-

control rights, this need not be the case. Many firms assign decision-control rights to non-equity owners, including employees, and still others assign rights to a co-governance board where equity owners and non-equity owners share decision-control rights (Dow, 2003). One would expect, however, that the precise location and composition of members with ultimate control can be extremely important since different actors have both a different economic stake and/or affective attachment to the firm, and this in turn can influence the normative goals pursued by the firm. Control structures could influence firm policies in determining the monitoring structure (mutual monitoring vs bosses), compensation structure (relative payment vs fixed payment), and strategies for market resiliency (layoffs vs wage-cuts).

While this is a relatively understudied topic in the experimental economics literature, this section presents a summary of experiments that inform how the governance structure of the firm might affect the motivation of workers.

4.5.1 Employee Participation in Firm Governance

When evaluating the effects of different governance structures, researchers would ideally want to know (1) to what extent employee participation is consultative, suggestive, or final, (2) the scope of the types of decisions made.

The meta-analysis from Levine and Tyson (1990) over an extensive literature that have examined various forms of industrial participation suggests that "participation usually has a positive, often small, effect on productivity, sometimes a zero or statistically insignificant effect, and almost never a negative effect." They further conclude from their analysis that substantive participation (rather than consultative participation) over decisions that directly affect conditions of workers on the shop-floor is most likely to produce significant, long-lasting increases in productivity. The authors of refrain from speculating over *why* participation affects productivity, though many explanations tend to be repeated in organizational behavior and industrial psychology. Below, wherever possible, we provide evidence testing these claims.

4.5.1.1 Participation increases the intrinsic reward of employment

Evidence from psychology suggests that individuals like to control decisions that affect themselves. Drawing on this insight, Frey et al. (2004) argue that authoritative decision-making structures could lead to lower job satisfaction, which in turn could lead to lower levels of motivation. A survey from Frey and Benz (2002) reports evidence showing that self-employed workers report higher

job satisfaction than non-self-employed workers. They further find that workers in large firm hierarchies report lower job satisfaction than people working in small organizations.

4.5.1.2 Procedural fairness

According to Frey et al. (2004), the evaluation of final outcomes is a function of the process that produced the outcome. In particular, outcomes that result from 'fair' processes are seen in a much better light than outcomes that result from 'unfair' processes. In a similar vein, Lind et al. (1993) find that litigants who perceive the arbitration process as being 'fair' are much more likely to accept court's decision than litigants who do not, after controlling for the outcomes. Greenberg (1990) shows that employees' reactions to pay cuts are less averse if management thoroughly and sensitively explains the basis for the pay cuts. Hewlett Packard's emphasis on transparent and participatory procedures have been credited with giving the firm more flexibility than the norm. For example, they have, on occasion, been able to temporarily lower wages by 20 percent with high support from employees (Weibel and Rota (2001)).

4.5.1.3. Participation leads to increases in cooperation and rule compliance

In the literature on Industrial or Workplace Democracy, it is often assumed that an equitable distribution of control rights would lead to a firm culture supportive of higher levels of worker cooperation and trust, thereby providing low-cost solutions to coordination failures through mutual monitoring and work norm enforcement (Bowles and Gintis (1998)). Two recent experimental studies have found evidence for a "democracy participation rights premium," as it relates to increased cooperation in simple public good environments. Dal Bo et al. (2007) find that when subjects can vote on either playing a Prisoner's Dilemma or a Coordination Game, higher cooperative behavior is elicited regardless of the outcome. They attribute at least some of this behavioral tendency to the intrinsic motivation of having been part of a democratic process (controlling for selection effects). Similarly, Sutter et al. (2006) show that in groups allowed to elect rules of enforcement for a PG game (punishment or reward), contributions exceed those of groups governed by the same (non-elected) rules. In contrast to these studies, however, Kroll et al. (2007), find that introducing a voting mechanism has only an impact on contributions to a public good game when a costly punishment mechanism is added. In Chapter 2 of this dissertation, I report

results from a real-effort experiment comparing work teams with different decision-control arrangements. I find that work teams that were involved in the decision over how all group members would be compensated for their effort in the experiment performed at higher levels than teams excluded from the decision-making process.

4.5.2 Authoritative control

The experiment conducted by Pech (2008) compares team production under various treatments where firm decisions over the division of the residual claim are made by either a "boss" that is either 1) a part of the work team (productive boss), 2) a boss that is not a part of the work team (unproductive boss), and 3) no boss. He finds that work teams with a productive boss contribute more to production than members in the 'no-boss' treatment after controlling for the division of the residual. He further shows that when the "unproductive boss" is sufficiently generous in the division of the residual, workers still reciprocate the generosity of the boss and contribute more than groups in the 'no boss' treatment.

4.6 Conclusion

A firm's viability is contingent, at least in part, upon the productivity of its workers. Workers draw upon their employment as a source of utility through various avenues including the social interaction with other workers and managers, the feeling of accomplishment, dignity, social status, and also through the intrinsic reward associated with the work task itself. The firm is partly a kind of marketplace -- with institutions that support specific contractual exchange relations -- and partly a place where people interact expressively as whole personalities, yet economists have primarily focused their study on monetary variables as and neglected psychological and social variables. We speculate that this is due to the use of the rational choice model since its assumptions define a specific psychology and social orientation. There is no doubt that the RCM can be an excellent first-approximation for human behavior, but as Gintis (2009) suggests, "the well has run dry" on what we can study by sticking exclusively to the RCM.

In this chapter we have given many examples of a growing body of experimental data that is contributing to the study of personnel in the workplace. While most of the studies reviewed examine the predictions of utilitarian-based

models of motivation, others are more exploratory in nature. As we have reported, some findings are consistent with predictions of the RCM, and others are not. This, however, is not important. What is important is that a clear path is now in place to move Economics into the realm of real-world behavior where psychological, cultural, ideological, and economic influences can be reasonably evaluated with observation rather than by assumption. The hope is for more personnel theorists and practitioners to accept the results from experimental studies as necessary and useful.

As we have shown, experiments offer the opportunity to test-bed institutional innovations in the compensation structure, the labor market conditions, the organization of the firm, the ownership structure, and the governance structure. The main strength of using experiments is the added control over conditions that better allow for isolating the relationship being investigated. Experiments, moreover, can be easily replicated and expanded to include theoretical or counterfactual ideas that may not exist in the 'real-world.' The results from experimental data will not always map perfectly to the population or phenomenon of interest and should therefore be thought of as a tool that is complementary to other empirical methods.

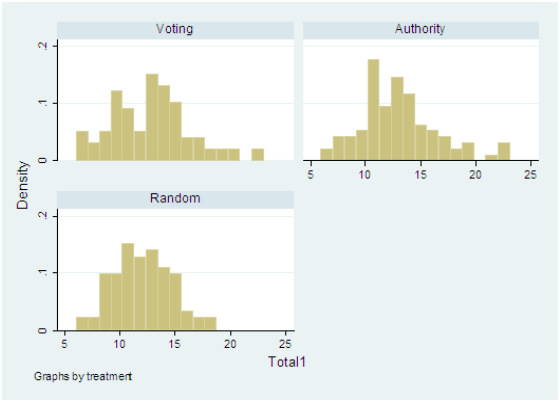
Although experiments have been used to expand on these analyses over

the past two decades, our review points to some issues that remain largely understudied. Specifically, there remains a large gap between the work done in industrial psychology and the work in economics investigating the motivational effects of different workplace governance practices and asset ownership distributions. Most existing theories in industrial psychology are modeled in non-utilitarian frameworks (possibly contributing to their neglect by economists), but experiments offer both a method to test many of the behavioral claims made in this work and a means of facilitating their incorporation into economic practice. Chapter 2 of this dissertation provides an example for this type of work.

APPENDIX A

HISTOGRAMS

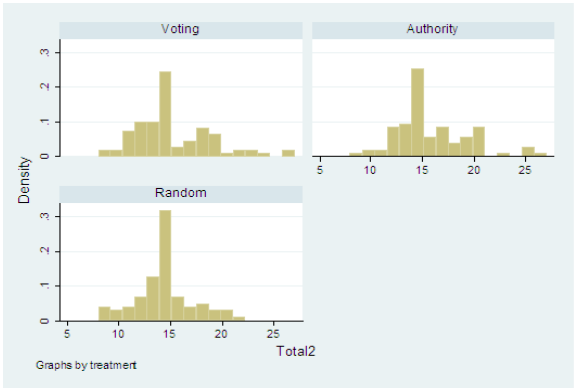
Figure A1: Total Attempts by Period and Treatment



Period 1 Total Attempted

	Obs	Mean	Std. Dev.
Voting	93	12.89247	3.34749
Authority	90	13.14444	3.357236
Random	87	12.1954	2.443973

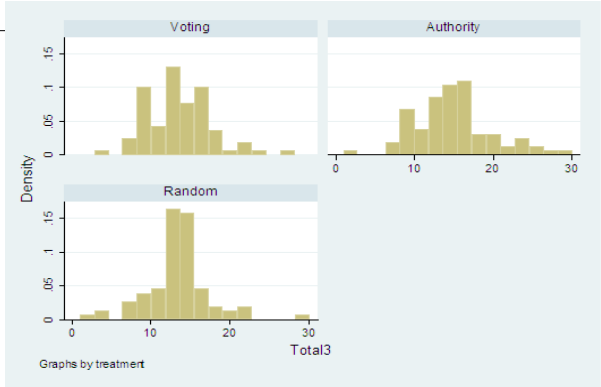
Kruskal-Wallis test probability = 0.2170
F-test p value = 0.1090



Period 2 Total Attempted

	Obs	Mean	Std. Dev.
Voting	93	15.10753	3.713847
Authority	90	15.83333	3.598533
Random	87	14.34483	2.735756

Kruskal-Wallis test probability = 0.0347
F-test p value = 0.0150

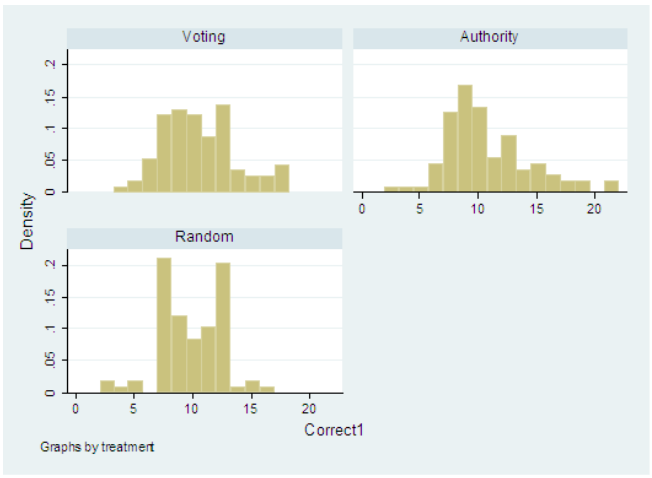


Period 3 Total Attempted

	Obs	Mean	Std. Dev.
Voting	93	13.51613	3.846548
Authority	90	14.95556	4.815497
Random	87	13.35632	3.997025

Kruskal-Wallis test probability = 0.0371
F-test p value = 0.0220

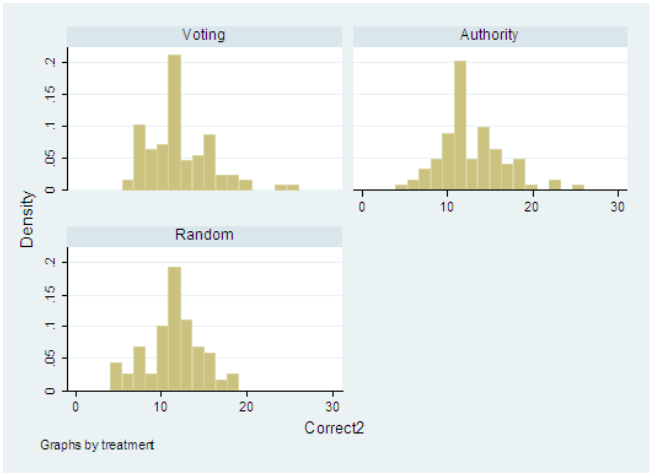
Figure A2: Histograms of Correct Answers by Period and Treatment



Correct Period 1

	Obs	Mean	Std. Dev.
Voting	93	10.50538	3.154099
Authority	90	10.58889	3.613005
Random	87	9.793103	2.54329

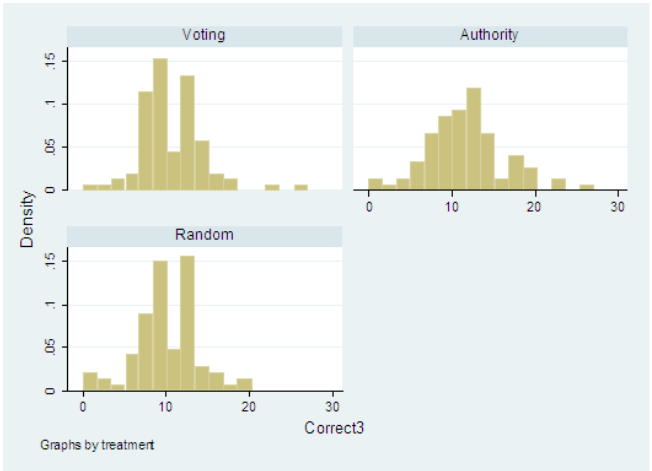
Kruskal-Wallis test probability = 0.5163
F-test p value = 0.1823



Correct Period 2

	Obs	Mean	Std. Dev.
Voting	93	12.10753	3.728452
Authority	90	12.76667	3.750805
Random	87	11.3908	3.236264

Kruskal-Wallis test probability = 0.1282
F-test p value = 0.0400



Correct Period 3

	Obs	Mean	Std. Dev.
Voting	93	10.62366	3.715169
Authority	90	11.68889	4.585381
Random	87	10.12644	3.753462

Kruskal-Wallis test probability = 0.0576
F-test p value = 0.0323

APPENDIX B

EXPERIMENT INSTRUCTIONS

Introduction (Common for all Treatments)

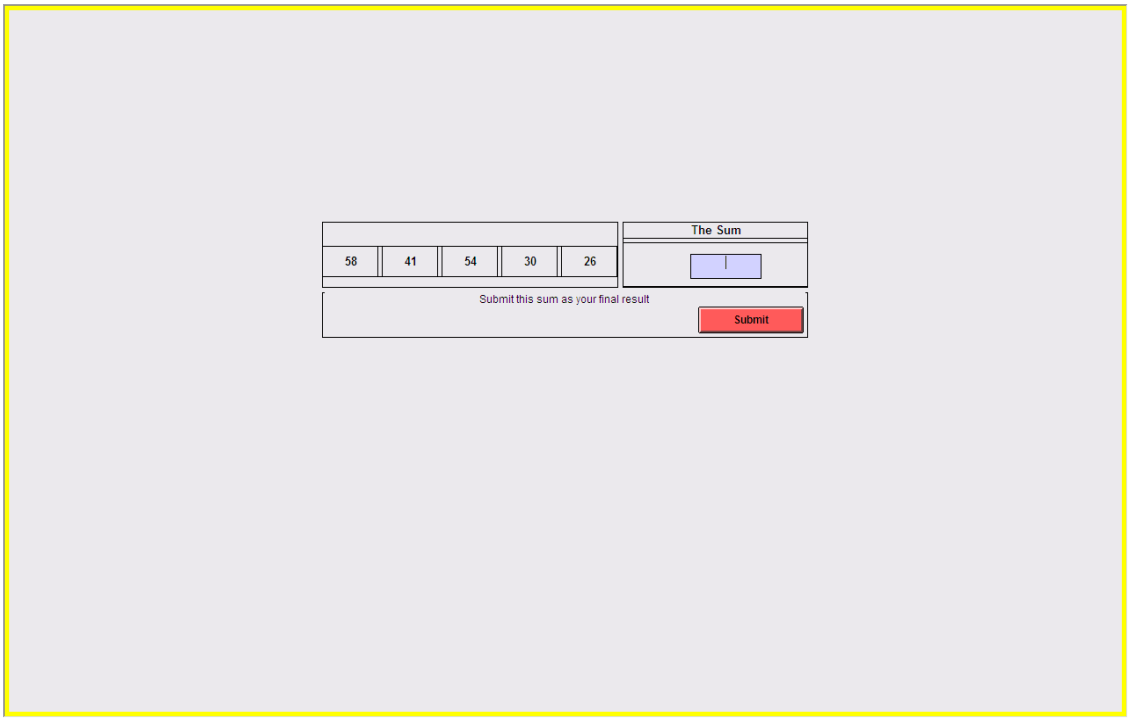
Thank you for participating in our study today. You will earn $5 just for showing up on time and during the experiment, you will have the opportunity to earn more money. The amount of money that you will get paid depends on your actions, as well as the actions of others in this experiment session. The monetary unit that is used throughout the duration of this experiment is an 'experimental monetary unit,' (EMU). At the conclusion of the experiment, all EMUs that you have accumulated will be converted into dollars at the rate of 30 EMUs = $1.00. You will be paid in cash today, at the end of the experiment. The money to conduct this study has been provided by the National Science Foundation. Please note that any and all actions and decisions that you make in the exercises or responses you provide are strictly confidential and anonymous. We intend to use the data collected from our study for academic work as it relates to group organization. To assure your responses are confidential, we ask you to not speak to each other until the entire study is completed.

A lab assistant will read all subsequent instructions aloud to you. Please read along with the lab assistant as s/he read them to you. If you have any

questions while these instructions are being read, please raise your hand and we will attempt to answer them. *You are not allowed to communicate with other participants during the experiment, even to clarify instructions*. Again, if you have any questions, please raise your hand and a lab assistant will assist you. This experiment will have 4 different parts; Period 1, Period 2, Period 3, and a brief survey. At the end of the experiment session, we will call you individually by number to give you your earnings in cash.

Instructions for Period 1 (Common for all Treatments)

In this experiment you will be completing a production task that consists of adding up sets of five 2-digit numbers. The use of a calculator is prohibited, but you may use scratch paper and pencil provided to you on your desk. The numbers that you will be adding together are randomly drawn and each problem is presented in the following way:



After you submit an answer on the computer, you will be given a new problem to solve. The production task of solving addition problems in Period 1 will last for 5 minutes. At the end of 5 minutes you will be presented with a summary of how many problems you correctly solved as well as your payment for Period 1.

Your compensation for solving problems in Period 1 will be a fixed payment of = 75 EUs.

At the end of Period 1, we will hand out a new set of instructions for Period 2.

Period 2 Instructions (Opening Paragraph for Treatment 1: Voting)

In Period 2 of the experiment, you will be randomly put into a group with 2 other people (3 total). Group members are connected through the computer network in this room and your identities will remain anonymous throughout the remainder of the experiment. At the beginning of Period 2 you will receive a message that indicates that you and the other two group members will democratically decide how all group members will be compensated for correctly adding up different sets of 2-digit numbers. The democratic process by which your group will reach a decision is through a simple voting election. You will each vote for one of the following two compensation schemes (CSs) which will affect the way all three persons in the group are compensated.

The compensation scheme that receives a majority of votes will be implemented.

Period 2 Instructions (Opening paragraph for Treatment 2: One-decision maker)

In Period 2 of this experiment, you have been randomly put into a group with 2 other people (3 total). You are connected through the computer network in this room and your respective identities will remain anonymous throughout Period 2. Before we begin Period 2, you will receive a message that indicates whether or not you have been randomly designated to be a 'decision-maker,' or or not.

If you ARE the decision-maker: You are fully responsible for choosing the compensation scheme that will be used for all 3 members of your group (yourself, and two others) for correctly adding up different sets of 2-digit numbers. The other members of the group do not have any say in say in this choice. The decision-maker will choose between one either compensation scheme 1, or compensation scheme 2, described in detail in the following.

If you ARE NOT the decision-maker: You do not have decision-making power over the choice of the compensation scheme. You will simply wait for the decision-maker to implement your group's compensation scheme.

The choice regarding how all group members are compensated (either through CS1 or CS2) is completely up to the decision-maker.

Period 2 Instructions (Opening paragraph for Treatment 3: Computer)

In Period 2 of the experiment, you will be randomly put into a group with 2 other people (3 total). Group members are connected through the computer network in this room and your identities will remain anonymous throughout the remainder of the experiment. Once you are in a group, a message will be sent to all three members that indicates how the members of your group will be compensated

for correctly adding up different sets of 2-digit numbers. The computer will randomly choose between 1 of the following 2 compensation schemes which will affect the way all three persons in the group are compensated.

The computer will randomly assign either compensation scheme 1 or compensation scheme 2.

[The following are common instructions for all treatments]

Compensation scheme 1 (CS1): If CS1 is chosen, then all of the correct answers from all members in the group are summed together. Each correct answer from the group is worth 10EUs. ***Under CS1, the person who has the highest number of contributions to the group total will receive 60% of all of the proceeds, the second highest performer will receive 30% of the proceeds, and the third highest performer will receive 10% of the proceeds.***

For example: Let us assume that Subject 1 solves 5 addition problems correctly, Subject 2 solves 10 correctly, and Subject 3 solves 15 correctly.

Subject 1: 5 correct answers

Subject 2: 10 correct answers

Subject 3: 15 correct answers

5 + 10 + 15 = 30 total correct answers

30 correct answers X 10EUs = 300EUs (Total Proceeds)

In this example, the payments for each subject in the group under CS1 are as follows:

Subject 1 would receive: 300EUs X **(.10)** = 30EUs (5 Correct)

Subject 2 would receive: 300EUs X **(.30)** = 90EUs (10 Correct)

Subject 3 would receive: 300EUs X **(.60)** = 180EUs (15 Correct-Highest performer)

Tiebreaker rule: It is possible that that 2 or more subjects have solved the exact same number of addition problems correctly. Regardless of whether there is a 2-way, or 3-way tie, ALL TIES ARE BROKEN AT RANDOM BY THE COMPUTER PROGRAM.

An example of a tie between highest and second highest contributions: Let us assume that Subject 1 solves 4 problems, both Subjects 2 and 3 solve 7 problems

each.

Subject 1: 4 correct answers

Subject 2: 7 correct answers

Subject 3: 7 correct answers

4 + 7 + 7 = 18 total correct answers

18 correct answers X 10EUs = 180EUs

In this example, Subject's 2 and 3 have each produced the same total of correct answers (each with 7 correct). If there is a tie under CS1, the tie is broken randomly by the computer program.

In this example, under CS1:

Subject 1 would receive with certainty: 180EUs X **(.10)** = 18 EUs

Subject 2 and Subject 3 could either receive: 180EUs X **(.60)** = 108 EUs

(Depending on tie-break outcome) or

180EUs X **(.30)** = 54 EUs

Compensation scheme 2 (CS2): If CS2 is chosen, all of the correct answers from all members in the group are summed together. Each correct answer from the group is worth 10EUs. ***Under CS2, every subject in the group will receive the same share of the of the total earned by the group.***

For example: Again, let us assume that Subject 1 solves 5 addition problems correctly, Subject 2 solves 10 correctly, and Subject 3 solves 15 correctly.

Subject 1: 5 correct answers

Subject 2: 10 correct answers

Subject 3: 15 correct answers

5 + 10 + 15 = 30 total correct answers

30 correct answers X 10EUs = 300EUs (Total Proceeds)

Under CS2, all subjects receive a the same share of the group total. In this example, the group total is 300EUs, therefore the payoffs to each member is 300EUs/3 group members = 100EUs per subject.

Payoffs in this example (CS2)

Subject 1 receives 100 EUs (5 Correct answers)

Subject 2 receives 100 EUs (10 Correct answers)

Subject 3 receives 100 EUs (15 Correct answers)

Period 3 Instructions

In Period 3, you will again be presented with the same production task that consists of adding up sets of five 2-digit numbers. The use of a calculator is prohibited, but may use scratch paper and pencil provided to you on your desk.

After you submit an answer on the computer, you will be given a new problem to solve. The production task of solving addition problems in Period 3 will last for 5 minutes. At the end of 5 minutes you will be presented with a summary of how many problems you correctly solved as well as your payment for Period 3.

Your compensation for solving problems in Period 3 will be a fixed payment of 75 EU.

APPENDIX C:

STRATIFIED REGRESSIONS BY DECISION-MAKING POWER

C.1 Introduction

Throughout the paper we report results of regression analysis that uses the expressed preferences of subjects in the experiment that had decision-control rights as a co-variate. Of the 270 subject that participated in the experiment, 123 had decision-control rights at the outset of Period 2 (all 93 subjects in the Voting treatment and 30 subjects in the Authority treatment).

The interpretations of the analysis put forward in the paper hinge on the understanding that mean differences in performance explained by the treatment occur because the subject is a part of an entire decision-making institution (i.e. everyone in the Authority treatment is affected the same way by being in the Authority treatment etc). There is, however, an important issue not explicitly considered in the body of the text. Namely, the compensation scheme implemented in the Voting and Authority treatments is a product of the expressed preferences of decision-makers and as we mention in the body of the paper, there are many motivations that may drive a subject to use their decision-control right one way or another (strong egalitarian ethic, the desire to challenge or be challenged in a competitive environment, lack of confidence of winning a

tournament, overconfidence, etc). Unfortunately, our data do not allow for clean test to distinguish one motive over another.

Our experimental design collects preferences for some subjects (those with decision-control rights), but not all subjects. We cannot, therefore, definitively claim that the differences we observe in behavior are fully due to the experience of being on one institution or another since it could be that the distribution of preferences may not have been randomized to treatment. In short, we simply do not know since we did not have preferences from all subjects. The worry is that the performance of some subjects without decision-making power could be driven by having a compensation scheme that they wanted to have implemented, had some influence in determining, and also knew that they would perform better under that particular compensation scheme. One way around this issue would be to design an alternative experimental protocol that would collect preferences from all subjects prior to revealing what the decision-making process (the treatments) would be for each subject. This alternative design would produce data that could directly address the critique that the results we report are being driven by an uneven distribution of preferences for the compensation scheme across treatments. One can further imagine a design where not only the preferences for the compensation scheme are collected, but also a measure of the

intensity of preferences using a 5-point likert scale or where subjects must provide a relative ranking of the compensation schemes. After having collected preferences from all subjects, the alternative experimental protocol would randomize subjects in a session to treatment and the experimental procedure would thereafter continue in the same way as the present protocol.

An alternative analytical strategy using the data collected under the experimental protocol reported in this paper would stratify the pool of subjects by those with decision-making power and those without decision-making power, or alternatively further split those cell into groups along various dimensions. For example, the sample could be divided among

- 1) Groups that have decision-making power (N = 123, Voting = 93, Authority = 30, Control = 0)
- 2) Groups that did not have decision-making power (N = 147, Voting = 0, Authority = 60, Control = 87)
- 3) Groups that have decision-making power and expressed a preference for a tournament (Overall N = 74; Voting N = 55; Authority = 19; Control = 0)
- 4) Groups that have decision-making power and expressed a preference for the revenue-sharing contract (N = 49; Voting = 38; Authority = 11;

Control = 0)

- 5) Groups that have decision-making power, expressed a preference for the tournament, and had the tournament contract implemented (Overall N = 65, Voting = 46, Authority = 19, Control = 0)
- 6) Groups that have decision-making power, expressed a preferences for the revenue-sharing contract and had the revenue-sharing contract implemented (Overall N = 38, Voting = 27, Authority = 11; Control = 0)
- 7) Groups that have decision-making power, expressed a preference for the tournament, but had a revenue-sharing contract implemented (Overall N = 9, Voting = 9, Authority = 0; Control = 0)
- 8) Groups that have decision-making power, expressed a preferences for the revenue-sharing contract, but had a tournament contract implemented (Overall N = 11, Voting = 11; Authority = 0; Control = 0)

among many other possibilities.

C.2 Analysis

By splitting the sample into subgroups, however, we lose observations and also the ability to make reliable comparisons in the data-- especially along further

stratification that comes by including categorical variables in regression analysis. For this reason, we therefore find it reasonable to stratify the data into two groups: those with decision-control rights, and those without decision-control rights. We will first estimating the same model for both groups groups of data assuming that the econometric model predicts something about the behavior within the group based on the subjects either having power, or not having power.

Below we test whether the coefficients estimated over the group of subjects that had decision-making power are equal to the coefficients estimated over subjects that had no decision-making power. For linear regression, the appropriate procedure is to use a Chow or Wald test.[25]

First, in Table C.1 below, 6 regression specifications are reported. Regressions 1-3 correspond to the group of subject with decision-control rights (N = 123), and 4-6 to the groups without decision-control rights (N = 147).

[25] When the estimated variance-covariance matrix of the estimators is used the Wald test is the Chow test and vice versa.

Table C.1 Dependent Variable Total Attempted Questions in Period 2 By Power

	Power == 1			Power ==0		
	(1)	(2)	(3)	(4)	(5)	(6)
voting	0.0181	-0.0973	-0.0606	0	0	0
	(0.707)	(0.677)	(0.401)	(0)	(0)	(0)
authority	0	0	0	1.820***	1.838***	0.925***
	(0)	(0)	(0)	(0.534)	(0.532)	(0.323)
cs1	2.147***	1.551***	0.528	1.494***	1.412***	0.950***
	(0.619)	(0.570)	(0.398)	(0.500)	(0.494)	(0.282)
male		2.000***	1.006**		0.691	0.357
		(0.640)	(0.441)		(0.485)	(0.281)
total1			0.851***			0.850***
			(0.0745)			(0.0496)
Constant	13.77***	13.35***	3.423***	13.42***	13.07***	3.187***
	(0.688)	(0.669)	(0.974)	(0.426)	(0.509)	(0.622)
Observations	123	123	123	147	147	147
R-squared	0.082	0.150	0.684	0.128	0.140	0.71

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

We want to test whether the coefficients estimated for subjects with decision-making power are equal to the coefficients estimated over subjects that had no decision-making power. To do this we follow the sequential Wald-test procedure from Gould (2007), of pooling the data, estimating the fully interacted model, and then testing the second group (those with decision-making power) coefficients against 0.

In Table C.2 we report the results of the 'pooled' regressions. Regression (1) pools Regression (1) and (4) from Table C.1. Similarly Regression (2) pools (2) and (5), and Regression (3) pools (3) and (6).

Table C.2 Dependent Variable Total Attempted Questions in Period 2 (Pooled)

	(1)	(2)	(3)
voting	0	0	0
	(0)	(0)	(0)
authority	1.820***	1.838***	0.925***
	(0.535)	(0.533)	(0.324)
cs1	1.494***	1.412***	0.950***
	(0.501)	(0.494)	(0.282)
male		0.691	0.357
		(0.486)	(0.282)
total1			0.850***
			(0.0496)
power1	0.374	0.183	0
	(0.632)	(0.685)	(0)
power1voting	0	0	0.175
	(0)	(0)	(1.172)
power1authority	-1.838**	-1.740**	-0.689
	(0.885)	(0.860)	(1.238)
power1cs1	0.653	0.139	-0.422
	(0.795)	(0.754)	(0.488)
power1male		1.308	0.650
		(0.803)	(0.523)
power1total1			0.000916
			(0.0894)
Constant	13.42***	13.07***	3.187***
	(0.427)	(0.510)	(0.623)
Observations	270	270	270
R-squared	0.105	0.145	0.698

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

For all three 'Pooled' regressions, we fail to reject the hypothesis simultaneously testing all linear restrictions with Prob > F = 0.1568 for Pooled Specification (1), Prob > F = 0.1254 for Pooled Specification (2), and Prob > F = 0.3692 for Pooled Specification (3).

We conclude that there are no differences in the coefficients estimated for

subjects with decision-making power are equal to the coefficients estimated over subjects that had no decision-making power. We can now confidently combine the group of subjects with decision-making power with those without decision-making power since there are no statistical differences between the groups.

Tables C.3 and C.4 take the same steps with effective effort as the dependent variable. Table C.3 splits the sample between group 1 (those with decision-making power) and group 2 (those without decision-making power.

Table C.3 Dependent Variable Correct Answers in Period 2
By Power

	Power == 1			Power ==0		
	(1)	(2)	(3)	(4)	(5)	(6)
voting	0.380	0.288	0.0475	0	0	0
	(0.728)	(0.706)	(0.438)	(0)	(0)	(0)
authority	0	0	0	1.861***	1.881***	1.061***
	(0)	(0)	(0)	(0.588)	(0.586)	(0.391)
cs1	1.939***	1.460**	0.502	1.155**	1.066*	0.517
	(0.612)	(0.584)	(0.504)	(0.559)	(0.560)	(0.395)
male		1.609**	0.751		0.757	0.797**
		(0.664)	(0.524)		(0.555)	(0.382)
correct1			0.782***			0.822***
			(0.0889)			(0.0659)
Constant	10.54***	10.20***	3.197***	10.67***	10.29***	2.561***
	(0.722)	(0.736)	(0.943)	(0.501)	(0.573)	(0.754)
Observations	123	123	123	147	147	147
R-squared	0.068	0.111	0.536	0.092	0.103	0.596

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

We now follow the Wald-test procedure from Gould (2007) of pooling the data, estimating the fully interacted model, and then testing the second group (those with decision-making power) coefficients against 0.

Table C.4. Dependent Variable Correct Answers in Period 2 (Pooled)

	(1)	(2)	(3)
voting	0	0	1.109*
	(0)	(0)	(0.587)
authority	1.861***	1.881***	1.061***
	(0.588)	(0.587)	(0.392)
cs1	1.155**	1.066*	0.517
	(0.560)	(0.560)	(0.395)
male		0.757	0.797**
		(0.555)	(0.383)
correct1			0.822***
			(0.0660)
power1	0.245	0.192	-0.426
	(0.651)	(0.706)	(1.225)
power1voting	0	0	0
	(0)	(0)	(0)
power1authority	-2.242**	-2.169**	0
	(0.935)	(0.917)	(0)
power1cs1	0.784	0.394	-0.0148
	(0.828)	(0.809)	(0.640)
power1male		0.852	-0.0462
		(0.865)	(0.648)
power1correct1			-0.0399
			(0.111)
Constant	10.67***	10.29***	2.561***
	(0.501)	(0.574)	(0.755)
Observations	270	270	270
R-squared	0.081	0.107	0.568

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

For all three 'Pooled' regressions, we fail to reject the hypothesis simultaneously testing all linear restrictions with Prob > F = 0.1287 for Pooled Specification (1), Prob > F = 0.1502 for Pooled Specification (2), and Prob > F = 0.4172 for Pooled Specification (3).

C.3 Winners and Losers

The last part of this exercise investigates if there was a difference among individuals that had decision-control rights and whether their expressed preference resulted in the compensation scheme implemented. As mentioned above, we would have liked to have collected preferences from all subjects in our experiment, in part to examine this issue. We do, however, have 93 observations from the Voting treatment were we have all of the preferences from subjects which is not a small sample compared with other experimental studies. All subjects expressed a preference for a compensation scheme, in order to have that compensation scheme implemented, at least one other person in their group of three (all interactions were anonymous) needed to express the same preference.

Below we examine if there was a difference between subjects that had their preference implemented and those that did not have their preference implemented. Considering only the subsample of people in the Voting treatment, in the reference category in Regression 1 (in Table C.5 and C.6) are subjects that had the revenue sharing contract implemented in Period 2. On average, subjects that had the tournament compensation scheme implemented attempted 1.988 more questions, answering 1.898 more questions on average than subjects under the revenue-sharing contract. Both differences are significant

at the .01 level. The reference category in Regression 2 are subjects that expressed a preference for the revenue-sharing contract. They attempted an average of 1.695 fewer questions answering 1.650 fewer questions than subjects that voted for the tournament contract. These differences are also significant at the .01 level.

In Regression 3 in from Table C.5 and C.6 we find that after controlling for the preference expressed, subjects attempted 1.502 more questions and answered 1.407 more questions than the reference category – or those subjects under the revenue sharing contract and also expressed a preference for the revenue sharing contract.

In Regressions 4-6 we include the interaction of subjects that expressed a preference for the tournament with the implementation of the tournament. In Regression 4, the reference category are subjects that expressed a preference for the revenue-sharing contract and also had the revenue-sharing contract implemented. From Table C.5, those subjects, on attempted 13.85 questions. Subjects that expressed a preference for the revenue-sharing contract attempted, but 'lost' since the tournament contract was implemented answered an average of 0.875 more questions than the reference category. This difference, however, is not statistically significant. Similarly, subjects that expressed a preference for the

tournament but had a revenue-sharing contract implemented answered 0.148 more questions than the reference category (Table C.5). This is also not significantly different from zero. Subjects that expressed a preference for the tournament and had the tournament implemented attempted 0.148+0.875+1.277 = 2.30 more questions than the reference category. T-tests examining if there is a difference between coefficients are all rejected.

(1) Ho: tournament = pref_tourn =tournament*pref_tourn

Prob > F = 0.7345

(2) Ho: tournament = pref_tourn

Prob > F = 0.6030

(3) Ho: tournament = tournament*pref_tourn

Prob > F = 0.8538

Similar analysis, not reported here holds for Regressions 5 and 6 and also for Table C.6 analysis shows that there was not a difference in behavior among subjects that 'won' or subjects that 'lost' the election of a compensation scheme using data from the Voting treatment.

Table C.5 Dependent Variable Total Attempted Questions in Period 2: Winners and Losers

	(1)	(2)	(3)	(4)	(5)	(6)
cs1	1.988***		1.502**	0.875	0.950	0.628
	(0.722)		(0.691)	(0.973)	(0.952)	(0.540)
votecs1		1.695**	0.873	0.148	-0.261	0.199
		(0.731)	(0.697)	(0.966)	(0.928)	(0.950)
cs1votecs1				1.277	0.820	-0.289
				(1.369)	(1.326)	(1.203)
male					1.839**	1.034*
					(0.840)	(0.523)
total1						0.843***
						(0.0925)
Constant	13.89***	14.11***	13.67***	13.85***	13.44***	3.411***
	(0.499)	(0.493)	(0.563)	(0.626)	(0.588)	(1.158)
Observations	93	93	93	93	93	93
R-squared	0.069	0.051	0.078	0.083	0.134	0.669

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Voting treatment only

Table C.6 Dependent Variable Correct Answers in Period 2: Winners and Losers

	(1)	(2)	(3)	(4)	(5)	(6)
cs1	1.898***		1.407*	0.838	0.895	0.896
	(0.697)		(0.745)	(1.022)	(1.014)	(0.802)
votecs1		1.650**	0.881	0.222	-0.0885	-0.466
		(0.715)	(0.762)	(0.991)	(0.993)	(1.163)
cs1votecs1				1.159	0.812	-0.105
				(1.484)	(1.483)	(1.461)
male					1.398	0.925
					(0.866)	(0.685)
correct1						0.766***
						(0.122)
Constant	10.94***	11.13***	10.72***	10.89***	10.58***	3.419***
	(0.426)	(0.437)	(0.470)	(0.497)	(0.475)	(1.191)
Observations	93	93	93	93	93	93
R-squared	0.062	0.048	0.072	0.076	0.105	0.484

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Voting treatment only

APPENDIX D

TESTING STATSISTICAL EQUIVALENCE OF PERIOD 3 AND PERIOD 1

Table D.1 Effort (Testing Statistical Equivalence of Period 3 and Period 1)

Overall

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
total3	270	13.94444	.2607503	4.284565	13.43107	14.45782
total1	270	12.75185	.1888527	3.103167	12.38003	13.12367
diff	270	1.192593	.1995446	3.278853	.7997248	1.58546

mean(diff) = mean(total3 - total1) t = 5.9766

Ho: mean(diff) = 0 degrees of freedom = 269

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 1.0000 Pr(|T| > |t|) = 0.0000 Pr(T > t) = 0.0000

-> Voting treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
total3	93	13.51613	.3988685	3.846548	12.72394	14.30832
total1	93	12.89247	.3471188	3.347492	12.20307	13.58188
diff	93	.6236559	.2785431	2.686172	.0704453	1.176867

mean(diff) = mean(total3 - total1) t = 2.2390

Ho: mean(diff) = 0 degrees of freedom = 92

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.9862 Pr(|T| > |t|) = 0.0276 Pr(T > t) = 0.0138

-> Authority treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
total3	90	14.95556	.507598	4.815497	13.94697	15.96414
total1	90	13.14444	.3538838	3.357236	12.44128	13.8476
diff	90	1.811111	.3661366	3.473477	1.083605	2.538617

mean(diff) = mean(total3 - total1) t = 4.9465

Ho: mean(diff) = 0 degrees of freedom = 89

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 1.0000 Pr(|T| > |t|) = 0.0000 Pr(T > t) = 0.0000

> Control treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
total3	87	13.35632	.4285261	3.997025	12.50444	14.2082
total1	87	12.1954	.2620214	2.443973	11.67452	12.71628
diff	87	1.16092	.3816401	3.559702	.4022441	1.919595

mean(diff) = mean(total3 - total1) t = 3.0419

Ho: mean(diff) = 0 degrees of freedom = 86

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.9984 Pr(|T| > |t|) = 0.0031 Pr(T > t) = 0.0016

Table D.2 Effective Effort(Testing Statistical Equivalence of Period 3 and Period 1)

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
correct3	270	10.81852	.2479742	4.074632	10.3303	11.30674
correct1	270	10.3037	.191629	3.148786	9.92642	10.68099
diff	270	.5148148	.1988346	3.267185	.1233449	.9062847

mean(diff) = mean(correct3 - correct1) t = 2.5892

Ho: mean(diff) = 0 degrees of freedom = 269

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.9949 Pr(|T| > |t|) = 0.0101 Pr(T > t) = 0.0051

-> Voting treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
correct3	93	10.62366	.385245	3.715169	9.858526	11.38879
correct1	93	10.50538	.3270648	3.154099	9.855797	11.15496
diff	93	.1182796	.3072482	2.962994	-.4919419	.728501

mean(diff) = mean(correct3 - correct1) t = 0.3850

Ho: mean(diff) = 0 degrees of freedom = 92

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.6494 Pr(|T| > |t|) = 0.7012 Pr(T > t) = 0.3506

-> Authority treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
correct3	90	11.68889	.4833416	4.585381	10.7285	12.64928
correct1	90	10.58889	.3808442	3.613005	9.83216	11.34562
diff	90	1.100000	.3893238	3.69345	.3264219	1.873578

mean(diff) = mean(correct3 - correct1) t = 2.8254

Ho: mean(diff) = 0 degrees of freedom = 89

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.9971 Pr(|T| > |t|) = 0.0058 Pr(T > t) = 0.0029

-> Control treatment

Paired t test

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
correct3	87	10.12644	.4024134	3.753462	9.326465	10.92641
correct1	87	9.793103	.2726693	2.54329	9.251055	10.33515
diff	87	.3333333	.3272636	3.052512	-.3172452	.9839119

mean(diff) = mean(correct3 - correct1) t = 1.0185

Ho: mean(diff) = 0 degrees of freedom = 86

Ha: mean(diff) < 0 Ha: mean(diff) != 0 Ha: mean(diff) > 0

Pr(T < t) = 0.8444 Pr(|T| > |t|) = 0.3113 Pr(T > t) = 0.1556

APPENDIX E:

MOTIVATIONAL CROWDING

An alternative interpretation of the data collected in Period 3, however, is that they measure the willingness to provide effort in the absence of any extrinsic incentives-- or their *intrinsic motivation* for performing the production task. Notice that at the beginning of Period 1, subjects had no prior experience with the specific production task used for the duration of the experiment but by Period 3 the subjects had 10 minutes of experience and they were also told that their compensation would be identical to the compensation received in Period 1 (a flat-wage payment of 75 EUs). The data in Period 3 further allow us an opportunity to study if the experience of the experiment led to any systematic increases or decreases in effort in Period 3.

Table E.1 Dependent Variable Total Attempted Questions in Period 3

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
voting	0.160	0.170	0.172	0.111	-0.808	-1.528*	-1.457*
	(0.585)	(0.589)	(1.181)	(1.160)	(0.902)	(0.786)	(0.784)
authority	1.599**	1.582**	1.566**	1.604**	0.683	-0.0694	0.00757
	(0.664)	(0.658)	(0.744)	(0.744)	(0.575)	(0.496)	(0.514)
tournament		1.353***	0.832	0.684	0.0844	-0.569	-0.487
		(0.512)	(0.557)	(0.567)	(0.455)	(0.459)	(0.468)
tourn_preference			0.697	0.741	0.771	1.347*	1.236*
			(1.107)	(1.072)	(0.818)	(0.699)	(0.712)
revshar_preference			-1.022	-0.502	0.0573	0.589	0.528
			(1.057)	(1.085)	(0.831)	(0.747)	(0.737)
male				1.456***	0.864**	0.356	0.410
				(0.533)	(0.416)	(0.397)	(0.396)
total1					0.854***		0.236
					(0.0691)		(0.161)
total2						0.904***	0.725***
						(0.0521)	(0.138)
Constant	13.36***	12.52***	12.84***	12.09***	2.397**	0.534	0.144
	(0.428)	(0.591)	(0.615)	(0.636)	(1.052)	(0.875)	(0.928)
Observations	270	270	270	270	270	270	270
R-squared	0.028	0.052	0.066	0.092	0.451	0.544	0.553

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In Table E.1, we report seven regression specifications examining Period 3 Effort. Regression 7 in the table regresses effort on the treatments, compensation scheme implemented, expressed preferences, gender, and the total number of questions attempted in Period 1 and Period 2 respectively. Controlling for all other co-variates, subjects in the Voting treatment attempted 1.457 questions less than the reference group which is significant at the 0.10 level, and subjects that expressed a preference for the tournament prior to Period 2 attempted 1.236 more questions than the reference group. Regression 7 also shows that an additional question attempted in Period 2 resulted in 0.725 questions attempted

in Period 3 on average. This was significant at the 0.01 level of significance. Regression 5 shows an additional question attempted in Period 1 resulted in 0.854 additional questions attempted in Period 3 which was significant at the 0.05 level, but after including Period 2 effort into the regression specification, Period 1 effort is still positive, but no longer statistically significant.

Table E.2: Dependent Variable Correct Answers in Period 3

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
voting	0.497	0.508	0.534	0.491	-0.584	-1.204	-1.194
	(0.557)	(0.553)	(1.111)	(1.094)	(0.905)	(0.813)	(0.815)
authority	1.562**	1.544**	1.543**	1.569**	0.792	0.128	0.194
	(0.629)	(0.620)	(0.721)	(0.728)	(0.579)	(0.529)	(0.520)
tournament		1.424***	1.103**	1.000*	0.524	0.148	0.180
		(0.472)	(0.502)	(0.509)	(0.394)	(0.417)	(0.385)
tourn_preference			0.405	0.436	0.527	1.290*	1.097
			(1.037)	(1.012)	(0.872)	(0.778)	(0.800)
revshar_preference			-0.654	-0.291	0.777	1.017	1.108
			(0.987)	(0.991)	(0.772)	(0.724)	(0.712)
male				1.016**	0.758*	0.189	0.305
				(0.499)	(0.393)	(0.380)	(0.370)
correct1					0.772***		0.322***
					(0.0744)		(0.103)
correct2						0.763***	0.556***
						(0.0568)	(0.0871)
Constant	10.13***	9.242***	9.442***	8.922***	1.810**	1.233*	0.347
	(0.402)	(0.509)	(0.524)	(0.586)	(0.854)	(0.729)	(0.791)
Observations	270	270	270	270	270	270	270
R-squared	0.025	0.054	0.060	0.074	0.408	0.485	0.513

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Similar to Table E.1, in Table E.2 we report the same seven regression specifications examining Effective Effort in Period 3. After controlling for treatment, compensation scheme implemented, expressed preferences, gender, and the number of correct answers Regression 7 in Table 8 shows that the inclusion of the number of correct answers in Periods 1 and 2 are positively and

significantly correlated with the number of correct answers in Period 3. The respective additional effect for a correct answer in Period 1 and Period 2 was 0.322 and 0.556 additional correct answers in Period 3 controlling for all other co-variates in the specification. No other co-variates are significant in the full specification.

The findings reported in Tables E.1 and E.2 suggest that the conditions of the experiment did not contribute to any crowding-in or out in Period 3 per se, but they do suggest that past success in production contributed to more effective effort in Period 3.

APPENDIX F

SURVEY QUESTIONS

Subjective reports of satisfaction

How **satisfied** are you with...

1) The sense of achievement that you get from your job?
2) The scope for using your own initiative?
3) The amount of influence you have over your own work?
4) The training that you receive?
5) The amount of pay that you receive?
6) Your job security?
7) The work itself?

Employee Voice

Overall, how good would you say managers at this workplace are at...

1) Seeking the views of employees or employee representatives?
2) Responding to suggestions from employees or employee representatives?
3) Allowing employees or employee representatives to influence final decisions?
4) Overall, how satisfied are you with the amount of involvement you have in decision-making at this workplace?

Autonomy

In general, how much influence do you have...

1) Over the tasks you do in your work?
2) The pace at which you work?
3) How you do your work?
4) The order in which you carry out your tasks?
5) The time you start or finish your working day?

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