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ABSTRACT

Workplace Democracy in the Lab*

While intuition suggests that empowering workers to have some say in the control of the firm is likely to have beneficial incentive effects, empirical evidence of such an effect is hard to come by because of numerous confounding factors in the naturally occurring data. We report evidence from a real-effort experiment confirming that worker performance is sensitive to the process used to select the compensation contract. Groups of workers that voted to determine their compensation scheme provided significantly more effort than groups that had no say in how they would be compensated. This effect is robust to controls for the compensation scheme implemented and worker characteristics (i.e., ability and gender).

JEL Classification: C92, J33, J54

Keywords: real-effort experiment, workplace democracy, decision control rights

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1 Introduction

There is a rich history of debate in the economics literature about which members of an organization should make the important decisions. The most common model of firm governance, for example, 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 that 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 et al., 2007, Dow, 2003).

While scores of studies have sought to compare how different firm-governance structures affect the motivation and performance of workers, it is an issue that has proven difficult to resolve (Levin and Tyson, 1990). We speculate that 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 the identification of worker performance. Another hurdle is how to control for innate 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 subjective 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 the mixed findings in this literature.

As suggested by Falk and Fehr (2003) and Berg (2006) we mitigate many of the confounding issues inherent to labor market studies by using a controlled laboratory experiment. Specifically, we investigate whether the performance of workers is sensitive to the provision of decision-control rights over a meaningful decision – the determination of the workers' compensation scheme.

Like van Dijk et al. (2001) and Freeman and Gelber (2010), we collected data from a real-effort experiment; however, we focus on changes in the performance of our participants when the compensation scheme is implemented either endogenously by the workers using a simple majority vote or by a random process completely exogenous to the group. While there are a number of more complicated voting rules that could be implemented, our decision to employ the plurality rule (simple majority) was informed by Frey (1983) who concludes that the best known and most widely used is majority rule.

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. We further strip down the effort task so that it is not reliant on team production to minimize confounds that could arise in social dilemmas (e.g., trust, reciprocity or reputation) 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 2 (decision-control rights regimes) by 2 (incentive contracts) between-subjects design to compare performance under different decision-control rights treatments. All subjects in each session participated in three periods. In the first, practice, period, participants were paid a fixed sum to spend 5 minutes familiarizing themselves with the effort task - solving simple addition problems. In period two, participants were randomly and anonymously assigned to groups of three, and told that they would again solve problems for 5 minutes, but that the method by which they would be compensated for their performance was contingent upon the implementation of one of two possible incentive contracts – either a rank-order tournament or a group revenue-sharing contract. Depending on the treatment, the decision over which compensation scheme would be implemented was made either endogenously by vote where all three group members had equitable decision-control rights (the Voting treatment) or exogenously by the computer (the Control treatment). The third period was identical to the first in that participants were again paid a fixed sum to solve addition problems for another 5 minutes. We included the third period, by which time the participants were surely comfortable with the task, to measure their ability.

We report evidence suggesting that effort 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 this intuition for group level decisions. Further, these effects persist even after controlling for gender, compensation scheme, and ability.

2 Experimental Design

We conducted fifteen 45-minute sessions over a three-week period with 180 participants who earned an average of $14, including a $5 show-up payment. Upon arriving participants signed a consent form and were seated at a computer terminal where they found a sheet introducing the study to them and a copy of instructions for the practice period.[1]

[1] A copy of the instructions appears as in the appendix.

In the practice period participants were paid a fixed wage of 75 Experimental Monetary Units (30EMUs = 1USD) for adding 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. The numbers to be added together were randomly generated, but everyone received the same set of math problems, in the same order. We chose to use this task because we expected that adding yields low intrinsic reward, it requires little skill, especially for a college student, and most importantly, previous work has found that it does not result in biased performance in any systematic manner (Niederle and Vesterlund, 2007).[2] When the 5 minutes of practice were over, participants saw a screen that displayed how many problems they correctly solved as well as a reminder of their earnings for the period.

At the beginning of period two participants were informed that they had randomly and anonymously been assigned to groups of three. The instructions indicated that one of two possible compensation schemes would be implemented and that the scheme would affect the payout of the entire group. The compensation schemes were simple incentive contracts. In both cases the pool of earnings to be distributed among the members of the group was determined by paying 10EMUs for each correct answer generated by the group. The schemes differed only in how the total proceeds were distributed back to the group members. Under the tournament scheme the person with the highest number of correct answers received 60% of the earnings, the second highest performer received 30%, and the lowest performer received the residual 10%.[3] All ties were broken at random. Under the revenue-sharing scheme the total proceeds were simply divided equally among the group members.

The instructions also described how the decision to implement a 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. In the Voting treatment the decision to implement the tournament or the revenue-sharing contract was made by a simple majority vote and in the Control treatment all members in the group were informed that the implementation of either the tournament or the revenue sharing contract would be done randomly by the computer program (similar to what was done in the related prisoner's dilemma study of Dal Bó et al., 2010).

[2] The experiment was coded in zTree (Fischbacher 2007). We gratefully acknowledge the authors of Niederle and Vesterlund (2007) for sharing their code for the adding production task.

[3] Considerable thought was put into the incentives of the tournament. In fact, we ran pilot experiments to collect participant preferences over the two compensation schemes as we titrated the relative payoffs because we wanted to make sure that both contracts would be chosen with some regularity. We decided on the 60|30|10 distribution because it created a tournament that made the average participant in the pilot nearly indifferent between the two schemes. We took the further precaution of first collecting the voting data and then calibrated the randomization procedure in the control to match the frequency of the compensation scheme selection. Table 1 shows that these precautions, in addition to a fair amount of luck, were very successful in generating the same frequency of compensation schemes across treatments.

Before the 5-minute period of solving math problems began in period two all subjects were informed of the outcome from the decision-making process regarding how they would be compensated. At the end of the 5-minute period, all subjects were presented with a screen indicating 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 three of the experiment which was identical to the practice period in that participants were paid another 75EMUs to solve problems for 5 minutes.

After period three a brief survey was administered that asked for a few characteristics that have proven to be important in this context. As one can see in Table 1, overall 28% of the participants reported that math came relatively easy to them and 51% of our participants were male. When all subjects were finished with the survey, they were individually called to the back of the room by identification number, where they received their payments in sealed envelopes.

3 Does voting increase effort?

In Figure 1 we compare mean levels of effort (i.e., the number of math problems attempted in period two) and effective effort (i.e., the number correct) between the two treatments. As one can see, voting increases both; however, the raw differences are only marginally significant.[4]

If we look at Table 1 we can begin to posit why the raw effect of voting is not as pronounced as one might expect. To begin we see that fewer of the participants in the voting treatment indicated an ease with math. In addition, note that Niederle and Vesterlund (2007) found that men tend to do slightly better in this task and there were more men in the control.

In Table 2 we correct our estimates for these differences. In column (1) we see that after controlling for math ease, participants in the voting treatment increase effort by just shy of one unit ($p<0.05$). In column (2) we confirm that men provide higher effort ($p<0.05$) and, consistent with van Dijk et al. (2001), we also see that effort is higher in the tournaments ($p<0.01$). Most importantly, in column (2) we see that controlling for math ease, gender, and the compensation scheme increases the point estimate on the effect of voting (and its significance). Recall that we ran period three to gather a measure of ability. When we include the total attempted in the third period in column (3), we see that this measure of ability soaks up some of the variation previously attributed to voting (notice the slight

[4]Under the hypothesis that voting should increase effort we find, using simple t-tests, $p=0.06$ for effort and $p=0.09$ for effective effort.

treatment differences seen in Table 1) but the estimate remains substantial and significant at better than the 5% level.[5] Lastly, in column (4) we switch focus to the number of correct answers in period two and find almost identical results – voting significantly increases both effort and effective effort.[6]

4 Concluding remarks

We show that providing workers with "voice" in how they will be compensated increases their effort significantly and the effects are not small. Indeed, there are a number of elements that likely dampen the effects that we have found. Because participants are frequently intrinsically motivated to work hard in real-effort environments (often regardless of the rules), treatment effects tend to be muted. In addition, recall that our participants worked only for a short while. Despite the dampening elements of our design, we still estimate that output increased by approximately one unit which compared to the mean constitutes an increase in effort of 7% and an increase in effective effort of 9%.

By showing that the process by which a material incentive was implemented partially explains differences in performance, we add further credence to recent claims that procedural aspects cannot be separated from how individuals interpret material incentives (e.g. Frey et al., 2004 or Dal Bó et al., 2010). Obviously, this experiment is only a start and there are a number of interesting possible extensions. For example, we plan to next examine what happens when another layer of realism is added back – instead of comparing voting to random assignment, what if the alternative is a regime in which decision control rights are centralized in an "authority" who picks the compensation scheme unilaterally?

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[5] The results are similar if we use the number correct in period three as our measure of ability instead.

[6] In unreported regressions, we also explored, but found no, interaction effect between voting and participating in a tournament.

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6 Tables and Figure

Table 1: Participant Observables by Treatment.

	Overall		Control		Voting	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
Math ease (I)	0.28	0.45	0.34	0.48	0.23	0.42
Male (I)	0.51	0.50	0.57	0.50	0.45	0.50
Tournament (I)	0.62	0.49	0.62	0.49	0.61	0.49
Attempted in piece rate period	13.44	3.91	13.36	4.00	13.52	3.85
Correct in piece rate period	10.38	3.73	10.13	3.75	10.62	3.72

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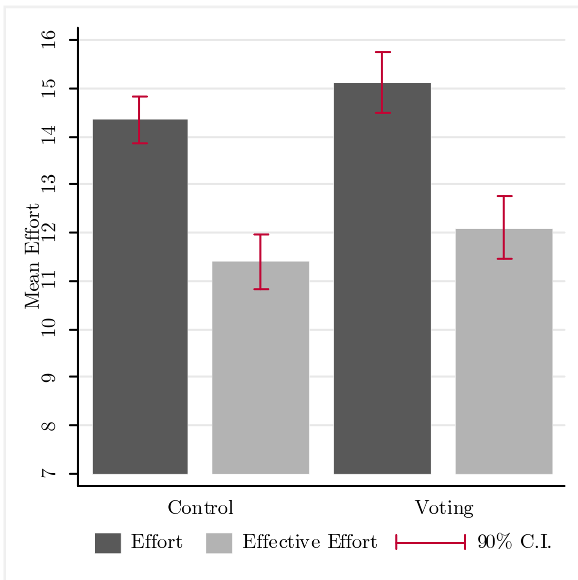


Figure 1: Voting increases effort.

Table 2: Effort Difference Estimates.

	(1) Effort	(2) Effort	(3) Effort	(4) Effective Effort
Voting Treatment (I)	0.946**	1.074**	0.809**	0.810*
	(0.477)	(0.477)	(0.365)	(0.430)
Male (I)		1.071**	0.427	0.291
		(0.469)	(0.362)	(0.426)
Tournament (I)		1.426***	1.173***	0.925**
		(0.451)	(0.388)	(0.424)
Ability			0.517***	0.505***
			(0.087)	(0.080)
Constant	13.813***	12.357***	6.277***	3.521***
	(0.323)	(0.529)	(1.021)	(0.980)
Observations	180	180	180	180
R^2	0.058	0.133	0.483	0.419

Self-reports of the "ease of math" included; (robust standard errors).

* p<0.10, ** p<0.05, *** p<0.01.

7 Appendix - Experiment instructions

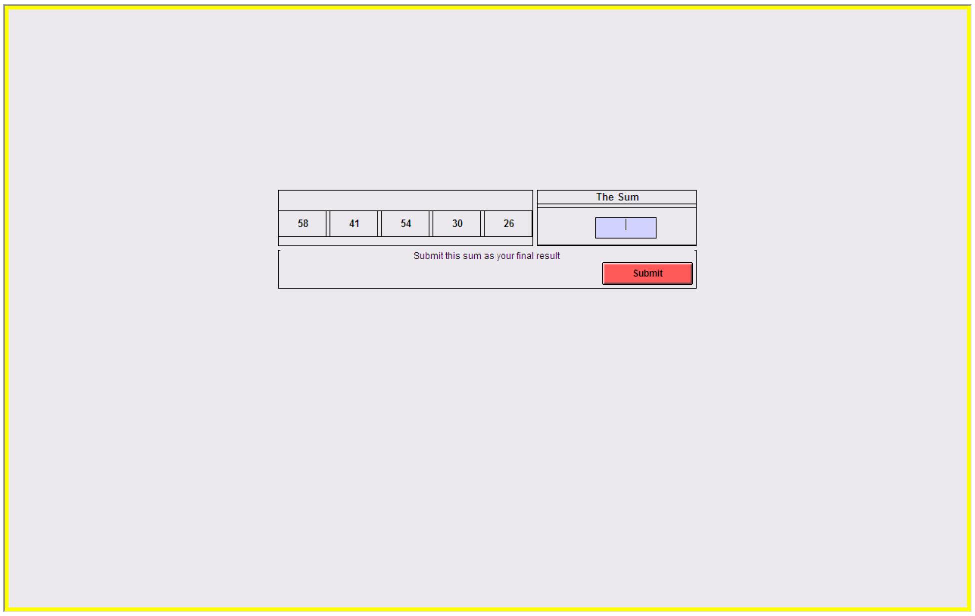
Introduction (Common for all Treatments)

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 EMUs. 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 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: Control)

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 10EMUs. *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 $\times$ 10EMUs = 300EMUs (Total Proceeds)

In this example, the payments for each subject in the group under CS1 are as follows: Subject 1 would receive: 300EMUs $\times$ (.10) = 30EMUs (5 Correct), Subject 2 would receive: 300EMUs $\times$ (.30) = 90EMUs (10 Correct) and Subject 3 would receive: 300EMUs $\times$ (.60) = 180EMUs (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 $\times$ 10EMUs = 180EMUs (Total Proceeds)

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: 180EMUs $\times$ (.10) = 18 EMUs. Subject 2 and Subject 3 could either receive: 180EMUs $\times$ (.60) = 108 EMUs (Depending on tie-break outcome) or 180EMUs $\times$ (.30) = 54 EMUs.

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 10EMUs. *Under CS2, every subject in the group will receive the same share 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 $\times$ 10EMUs = 300EMUs (Total Proceeds)

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

Payoffs in this example (CS2): Subject 1 receives 100 EMUs (5 Correct answers), Subject 2 receives 100 EMUs (10 Correct answers) and Subject 3 receives 100 EMUs (15 Correct answers).

Period 3 Instructions (Common for all Treatments)

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 EMU.