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Workplace democracy in the lab

Philip Mellizo, Jeffrey Carpenter and Peter Hans Matthews

ABSTRACT

While intuition suggests that empowering workers to have some say in the control of the firm is likely to have beneficial effects, empirical evidence of such effects 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, worker characteristics such as ability and gender, and possible sorting.

> [H]aving a voice in how they do their work is often as important to people as how much they are paid to do it [George Strauss, 1998].

1 INTRODUCTION

Few scholars of the workplace, we suspect, believe that 'voice' matters this much, but debate about its importance has been part of a broader conversation about decision-making within the firm for decades, if not centuries.[1] The most common model of firm governance remains a centralised one, but participatory alternatives that delegate decision-making responsibilities to workers are regularly observed. Kruse *et al*. (2008) find that up to 40 per cent of employees in the US report having a lot of influence on firm decisions or say that they often participate with other workers in job decisions. More recently, using the latest iteration of the Workplace Employment Relations Study (WERS, 2011), van Wanrooy *et al*. (2013) report that within the British firms studied, 34 per cent of workers report being able to influence important firm decisions, up slightly from 32 per cent in 2004. Historically, the adoption of 'employee empowerment initiatives' and related programs has resulted from concerns about fairness, job satisfaction, workplace trust and, of particular importance to economists, workplace productivity (e.g. Dow, 2003; Freeman *et al*., 2007).

❒ Philip Mellizo is Assistant Professor, Department of Economics, College of Wooster, Jeffrey Carpenter is Jermain Professor of Political Economy, Department of Economics, Middlebury College and Peter Hans Matthews is Johnson Professor of Economics, Department of Economics, Middlebury College. Funding for this research was provided by the National Science Foundation (SBE 0752822). Correspondence should be addressed to Jeffrey Carpenter, Middlebury College, Economics, 601 Warner Hall, Middlebury, VT 05753; email: jpc@middlebury.edu

[1] See Dundon *et al*. (2004) for an excellent taxonomy of the different meanings and purposes of employee voice and Gollan and Lewin (2013) of a survey of recent work on voice in non-union firms.

Despite the longstanding interest in, debate over, and widespread practice of employee participation in decision-making, surprisingly little evidence has been gathered to evaluate their performance. As Dow (2003: 8) puts it, 'A small band of empirical researchers has labored to redress the balance, but abstract modeling has outpaced the evidence.' While part of the problem is the simple dearth of available data, another is the lack of data that allows one to estimate causal effects. Taken in combination, this might explain the considerable variation in published estimates of the effects of firm-governance structures on the motivation and performance of workers (Levine and Tyson, 1990). This is an extremely difficult problem that Bonin *et al*. (1993: 1306) succinctly describe: 'To examine productivity differences between [democratic firms] and [conventional firms], the comparison should be made between firms that are "twins" in all non-organisational respects, e.g. in terms of technology, the product generated and market conditions. However, identifying "twins" is often impossible because the existing data on product type and technology are not sufficiently disaggregated. Firm-level data that applies consistent accounting conventions to both [types of firms] in the same industry are rare. Furthermore, workers are not assigned randomly to the [democratic] or [conventional] firm?'

The general lack of data has meant that researchers have turned to very small sample firm comparisons and single-firm case studies. Indeed the insights that have been gained from this work are qualitatively valuable, and this research has revealed a number of associations in the data. For example, in her comparative study evaluating the quality of home healthcare in a for-profit, a nonprofit and a cooperative firm, Barry (2011) finds that ownership and participatory decision making are positively associated with lower levels of turnover, and higher levels of job satisfaction, greater organisational commitment, and quality of care noting that 'these positive outcomes are more closely associated with the worker cooperative business than with the other forms of organization.' At the other end of the aggregation scale, Erdal's (2001) survey of residents from neighboring towns in Italy that differed in the concentration of worker cooperatives found associations between the concentration of cooperatives and civic participation, social capital and health.

Despite the challenges of collecting and analysing naturally occurring data that either draw from a small sample of firms (e.g. Appelbaum *et al*., 2000; Barry, 2011), workplace surveys (e.g. Bryson *et al*., 2006; Cooke, 1994; Fernie and Metcalf, 1995; Kersley *et al*., 2006), or community level data (e.g. Erdal, 2001), these and several other studies from the industrial relations literature have revealed a number of associations between participation and performance.[2] And while clean data suggesting a causal link is lacking, several hypotheses have been suggested regarding potential mechanisms of decision-participation that might lead to higher firm performance. For example, employee voice and participation may help serve to align the interests of workers with managerial aims (Milgrom and Roberts, 1995), they may offer a way to tap into knowledge acquired by workers that leads to improvements in production, (Brickley *et al*., 1997), or they may, more generally, affect worker motivation leading to higher effort and productivity (Bowles and Gintis, 1993), the hypothesis our experiment is designed to examine.

[2] As noted in Bach's (2005) edited volume, a number of commentaries have been made regarding the methodological shortcomings that accompany large-scale data including an over-reliance on financial output for performance measures, and the inability for much of the data to rule out simultaneity or reverse causality.

Although randomly assigning workers to real enterprises that vary only in firm organisation is ideal for the identification of causal effects, no such natural experiments exist and so properly randomised and controlled lab experiments, especially ones in which the external validity is emphasised by requiring real effort, can prove a valuable resource, especially is situations like this in which little viable data of any sort is available (c.f., Falk and Fehr, 2003). Working with controlled environments can improve our understanding of the underlying assumptions and incentives that drive behavioral responses to firm organisation, and serves as a complement to both the contextually rich case studies and the firm-level surveys that have been used to study participation. The use of experiments has already shone a bright light on several issues that have fundamentally challenged the way personnel and labor economists approach the study of incentives and the behavior of workers (Charness and Kuhn, 2011).

Prompted 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. Our decision to have workers voice their preferences over compensation schemes is partially driven by the fact that previous studies (e.g. Lazear, 2000; Nealey, 1963; Niederle and Vesterlund, 2007) indicate these concerns are salient. However, although not as common as worker input in other domains such as work process, the choice is also representative of a number of real workplaces (e.g. Semco or Skyline Construction), some of which choose compensation schemes using a one-worker/one-vote democratic process (e.g. Mondragón Corporación Cooperativa).

Like van Dijk *et al.* (2001) and Freeman and Gelber (2010), we collected data from a real-effort experiment, but 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 minimise 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 two (decision-control rights regimes) by two (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 familiarising 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, as well as possible selection effects.

2 EXPERIMENTAL DESIGN

We conducted fifteen 45-minute sessions over a three-week period with 180 participants (undergraduate and graduate students from various disciplines at the University of Massachusetts-Amherst) who earned an average of $14, including a $5 show-up payment. The research participants were recruited via table-tents, flyers, and from announcements made in large lecture halls. Individuals that expressed an interest in participating in the study were sent an email allowing them to sign up for one of the sessions. 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.[3]

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).[4] 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

[3] A copy of the instructions appears as in the Appendix.

[4] 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.

with the highest number of correct answers received 60 per cent of the earnings, the second highest performer received 30 per cent, and the lowest performer received the residual 10 per cent.[5] All ties were broken at random. Under the revenue-sharing scheme the total proceeds were simply divided equally among the group members.

We specifically wanted subjects to voice their vote over their preferred compensation scheme rather than allowing individuals to more simply self-select into a compensation scheme. First, we know from public choice theory that an individual's vote over a decision that affects a group might not necessarily map to the same decision taken where the affected party is just the individual. For example, in their seminal piece, Riker and Ordeshook (1968) postulate that rational voters may aim to vote with an instrumental purpose (i.e. to get what they want) and/or an expressive one that fulfills one's civic duty, their responsibility towards others, their support for the democratic process and so on. A second reason we asked subjects to vote was because it allows an interesting comparison between people who successfully voted for a compensation scheme and those who lost the vote and, therefore, might be disgruntled.

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).

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 again paid a flat fee of 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 per cent of the participants reported that math came relatively easy to them and 51 per cent 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?

We hypothesise that voting, as an expression of voice, will increase effort in our experiment. Recent research on the psychology of self, in particular self-determination

[5] 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 randomisation 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.

Table 1: Participant observables by treatment

	Overall ($n = 180$)		Control ($n = 87$)		Voting ($n = 93$)	
	Mean	SD	Mean	SD	Mean	SD
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

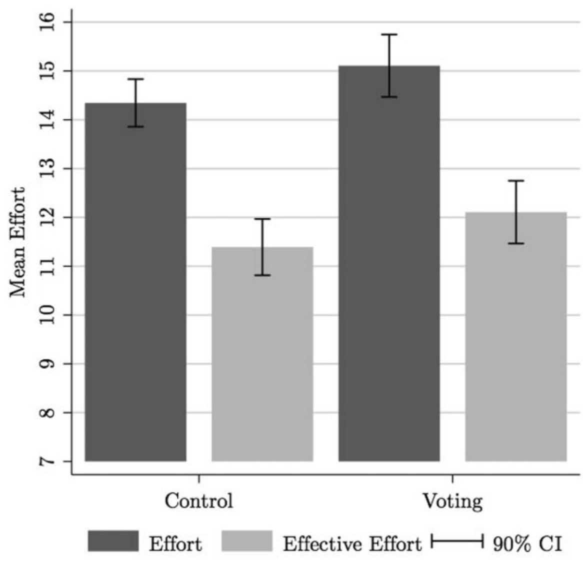

Figure 1: Voting increases effort

theory (Deci and Ryan, 1985; 2000a; 2000b), provides one of several possible motivations. Self-determination theory posits the existence of three basic psychological needs—for autonomy, competence and relatedness—and predicts that the effects of external interventions on well-being turn on the extent to which these needs are satisfied. To the extent that voice meets these needs, there will be intrinsic motivation to produce more.

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. Effort of both sorts increases but, as the diagram also hints, the raw differences are only marginally significant.[6]

An examination of Table 1 suggests at least two explanations for this smaller-than-predicted treatment effect. First, a smaller number of participants in the voting

[6] 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.

Table 2: Effort difference (OLS 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). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

treatment reported themselves comfortable with math. Second, building on Niederle and Vesterlund (2007), who found that men tended to do slightly better at this task, there were more men in the control treatment.

Using multiple regression (ordinary least squares to be specific), Table 2 corrects 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 add controls for gender and compensation scheme and confirm that men provide higher effort ($p < 0.05$) and, consistent with van Dijk *et al.* (2001), find that effort is higher in the tournaments ($p < 0.01$). More important, both our point estimate of the effect of voting and its statistical significance increase. Controlling for the characteristics we gathered, participants in the voting treatment produce 1.074 more summations ($p = 0.03$) than those participants who could not vote. Column (3) adds a measure of ability, the total number attempted in the third period, to the specification, and we see that this soaks up some of the variation previously attributed to voting (i.e. the voting coefficient falls a bit to 0.809), but the estimate remains both substantial and significant at better than the 5 per cent level.[7] Last, in column (4) we switch focus to the number of correct answers in period two and find almost identical results: that is, voting is associated with significant increases in both effort and effective effort.[8]

Our design allows us to dig a bit deeper into our results. Our hypothesis about intrinsic motivation suggests that voice should matter, but it is not clear that this should be true of all the workers who had a vote. On the one hand, it might be the case that just expressing the right to vote will enhance intrinsic motivation for all workers but, on the other hand, vote winners might react differently than vote losers. That is,

[7] The results are similar if we use the number correct in period three as our measure of ability instead.
[8] In unreported regressions, we also explored, but found no, interaction effect between voting and participating in a tournament.

Table 3: Effort difference (negative binomial estimates)

	(1) Effort	(2) Effective effort
Voting treatment (I)	1.054**	1.068*
	(0.025)	(0.037)
Male (I)	1.022	1.014
	(0.024)	(0.036)
Tournament (I)	1.086***	1.086**
	(0.029)	(0.039)
Ability	1.034***	1.042***
	(0.006)	(0.006)
Constant	8.380***	5.924***
	(0.516)	(0.441)
Observations	180	180
Wald χ^2	139.22	116.42

Self-reports of the 'ease of math' included; incidence rate ratios reported; (robust standard errors). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

gains in effort might only arise from those who vote and get their way. We tested these auxiliary hypotheses in an additional regression similar to column (3) of Table 2. However, in this estimation we parse the voters into two groups: those who won the vote and those who lost the vote. Our estimates suggest that, to a certain degree, both hypotheses are correct. Specifically, both types of voters increase their efforts compared to those workers who couldn't vote ($\beta_{won} = 0.83$, $\beta_{lost} = 0.73$), the difference between these point estimates is not significant ($p = 0.86$), but only the vote winners effect is significant ($p = 0.03$).

We offer two robustness checks of our results. The first concerns whether we are correctly accounting for how the data is generated. One could argue the outcome is better treated as a count variable. To that end, we report in Table 3 the results of negative binomial regressions for both effort and effective effort. A cursory glance suggests that the results are identical to Table 2. As for the point estimates, we now find that voting leads to an effort rate that is 1.054 times greater than that of participants in the no-voice control group ($p < 0.05$) and an effective effort rate that is 1.068 times greater ($p < 0.10$).

Our second robustness check has important implications for the interpretation of our results. While enhanced intrinsic motivation is our favored interpretation of the 'effort bump' seen in the voting treatment, it is possible, of course, that the difference is a consequence of sorting. If, for example, competitive people tend to prefer tournaments, then some of the increased effort in the voting treatment could reflect their disproportionate presence.[9] The tournament indicator would then be endogenous (i.e. at least partially the result of worker choices), with consequences for our estimates of both the effect of the compensation scheme and, more important for our purposes,

[9] As an illustration of such a selection process, Lazear (2000) found that although switching to piece rates from hourly wages is associated with a 44 per cent increase in productivity at the Safelite Glass Corporation, up to half of the effect is due to the sorting of high and low ability workers.

voting. In particular, one might worry that our estimate of the effect of being in a tournament might be incorrectly measured because more competitive types, for example, might sort into tournaments and if it is biased in this way, it might also bias the coefficient we really care about, the one estimating the effect of voting.

Some readers will find it odd that we implemented a design in which one of the treatment variables could be endogenous in this way. It would be no less odd, however, and certainly more contrived, if participants could cast votes that mattered without sorting. In our case, participants would either have to cast irrelevant (that is, unrelated to work conditions) votes, or to cast them under a clever but perhaps convoluted set of rules. In both cases, the external validity of our results would be compromised. Faced with this dilemma, one that Dal Bó *et al*. (2010) also confronted, we understood that our choice would attract the attention of the 'identification police,' but were also confident, for reasons we discuss below, that if there was a crime, it was a minor, and more important, that the related experimental literature provided a solution or, to extend the metaphor, the promise of immunity.

On the extent of the potential problem, recall that few participants were even afforded the opportunity to sort: the half in the control group could not sort because the compensation scheme was randomly assigned. Furthermore, because the voting treatment used majority rule, sorting required more than just a preference for one compensation scheme over another: in terms of our earlier example, competitive people with a predilection for tournaments will not work under tournament rules unless at least one other person in their three person group had similar preferences. Clearly, not all of them did.

None of this rules out the possibility of *some* sorting in the voting treatment, of course. There is, however, a standard econometric solution (i.e. an instrument) for the troublesome tournament indicator. Our task, it should be recalled, comes from the literature on gender differences in willingness to compete, in which the stylised fact is that men are much more likely to select into a tournament (c.f., Niederle and Vesterlund, 2007). This fact characterises our data too. Men were 40 per cent more likely to vote for a tournament ($p < 0.01$). The critical implication for our purposes is that someone who is randomly assigned to a group with two other men is much more likely to work under tournament rules, regardless of their own preferences or gender.

Random group assignment therefore provides an exogenous source of variation in selection into tournaments in the voting treatment. With this in mind, we construct an instrument for the tournament indicator using the gender composition of groups. Table 4 reports our estimates when we instrument for the tournament indicator. We first note, based on the relevant first stage F-statistic, that this is not a weak instrument. Second, we find that the tournament point estimates rise slightly but not significantly ($p > 0.30$ in both cases), which suggests that any sorting bias is minimal. Third, and most important, our estimate of the voting coefficient is almost unchanged, from which we infer that the effects of tournaments and voting are mostly independent of each other. In short, not only do we conclude that endogeneity is, in the end, a minor concern, we find little need to parse the effect of the voting treatment into voice and sorting—it is all voice (that is, intrinsic motivation).

4 CONCLUDING REMARKS

As Dougcouliagos (1995) summarises, the theoretical work on the effects of participation on performance can be divided into two camps. Proponents of increased

Table 4: Effort difference (instrumental variable estimates)

	(1) Effort	(2) Effective effort
Voting treatment (I)	0.809**	0.809*
	(0.359)	(0.423)
Male (I)	0.416	0.272
	(0.358)	(0.419)
Tournament (I)	1.363***	1.242**
	(0.487)	(0.536)
Ability	0.515***	0.503***
	(0.085)	(0.078)
Constant	6.188***	3.373***
	(0.993)	(0.946)
Observations	180	180
Weak instruments F	17.25	

Self-reports of the 'ease of math' included; (robust standard errors). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

employee participation theorise that the effect is likely positive—perhaps through increases in more output per worker, and/or the need for fewer firm inputs (i.e. a reduced need for specialised managerial labor and monitoring inputs) and critics of the idea believe it to be negative since they may lead to inefficient and slow management (Kremer, 1997), wasting talent and resources (Williamson, 1980), and/or by creating free-rider problems (Alchian and Demsetz, 1972). He further notes that such questions ultimately require empirical evaluation—a task that has proven difficult for several reasons. For example, Ledford and Lawler (1994) note that organisations tend to introduce participatory processes along with other interventions, such as goal setting, reward incentives, and performance feedback, which thus make it difficult to isolate the impact of participation on performance. Locke and Schweiger (1979) further comment that the effects of participation on performance are likely mediated by contextual factors that are prohibitively difficult to measure at the industry, organisational, and individual levels. It should not be surprising, then, that the empirical work attempting to measure the effect of participation on performance is mixed, both in terms of conclusions and quality.

While there is some value in conducting meta-analyses based on this ambiguous empirical record (e.g. Dougcouliagos, 1995; Levine and Tyson, 1990), these studies can only reveal whether there is, on average, an association between participation and performance (which appears to be the case). This obviously does not answer the question of whether there is a systematic, or a causal link. Indeed, the experimental method is far from perfect in this regard, but we, like Pencavel (2012), believe it be 'a propitious direction' to study 'repeated claims about [participatory firms] that have not been thoroughly examined' because it offers a way to isolate and properly identify any effects of participation on behavior. It also offers a way to systematically study the effects of some of the mediating factors (e.g. ownership rights) that have been theorised to influence the efficacy of participation in decision-making on performance.

We show, using a controlled lab experiment, that providing workers with a 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 per cent and an increase in effective effort of 9 per cent.

An important question that can't be answered by our experiment is whether the productivity gains we see are permanent. Can worker coops, for example, sustain the productivity gains afforded by voice or do they fade over time? The influential study of Craig and Pencavel (1995) documents productivity gains similar to those we find for worker coops in the plywood industry (between 6 per cent and 14 per cent) and argues that these gains have likely persisted for quite a while and explain the coexistence of slightly more productive coops and slightly less productive traditional firms that have better access to capital.

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 next plan to 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 centralised in an 'authority' who picks the compensation scheme unilaterally and may have incentives to choose one scheme over another?

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APPENDIX—EXPERIMENT INSTRUCTIONS

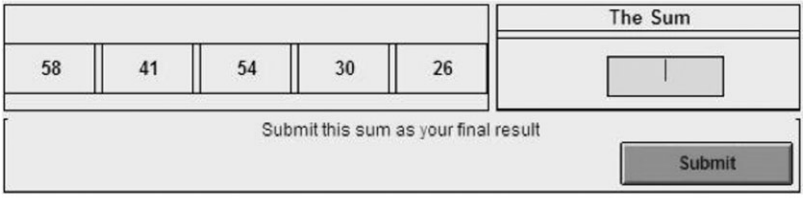
Introduction (Common for All Treatments)

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 × 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 × (0.10) = 30EMUs (5 Correct), Subject 2 would receive: 300EMUs × (0.30) = 90EMUs (10 Correct) and Subject 3 would receive: 300EMUs × (0.60) = 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 answers4 + 7 + 7 = 18 total correct answers
18 correct answers × 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 × (0. 10) = 18 EMUs. Subject 2 and Subject 3 could either receive: 180EMUs × (0.60) = 108 EMUs (Depending on tie-break outcome) or 180EMUs × (0.30) = 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 answers5 + 10 + 15 = 30 total correct answers
30 correct answers × 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.