**On the Merit of Equal Pay:**

**Influence Activities & Incentive Setting**

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**Abstract**

Influence costs models predict that organizations should limit managerial discretion to deter organizational members from engaging in wasteful politicking activities. We test this conjecture in a controlled, yet realistic, work environment in which we either permit or not permit workers to influence managers’ pay decision. We find that influence activities are pervasive and lead to reduced firm performance. Firm performance is reduced because principals offer lower-powered incentives in the environment in which influence activities are permitted than in the environment in which they are not. Importantly, we show that *equal pay* incentive schemes perform better when influence activities are available than when they are not. Our results thus support the idea that prevalent politicking activities may account for the widespread use of bureaucratic, and apparently inefficient, compensation rules in organizations.

**Keywords**: Influence activities, incentive theory, theory of the firm, organizational economics

**JEL Codes**: C91, D23, D86, M52

“Where different parts of the organization have responsibility for different pieces of information relevant to a decision, we would *expect … some attempts to manipulate information* as a device for manipulating the decision.”

Cyert & March, 1963, p79

**1. INTRODUCTION**

One of the main challenges of economic science is to provide a unifying model of the boundaries of the firm (Gibbons, 2005). To understand firm boundaries one must not only explain why all economic activity is not coordinated through markets but also why “all production is not carried out by one big firm” (Coase, 1937). Following Williamson (1985), one should explain the limits of the “selective intervention” argument under which firms can do at least do as well as markets by having firm executives selectively intervene to coordinate only those activities for which markets fail. To understand why firms do not grow indefinitely one must recognize that firms face internal constraints that increase with size. A widespread view is to consider such costs as emanating from firms internal politicking and influence activities (Milgrom, 1988; Milgrom and Roberts, 1988, 1992; Meyer, Milgrom and Roberts, 1992; Gibbons, 2005; Friebel and Raith, 2010; Powell, 2014). These works put forward that organizational members waste a considerable amount of time trying to bias managers’ decisions in their favor.[[1]](#footnote-1)

The literature on influence activities has thrived by highlighting the widespread costs associated to politicking in organizations. In particular, influence activities have been shown to distort the internal allocation of resources (Scharfstein and Stein, 2000; Inderst, Müller and Wärneryd, 2005; Laux, 2008; Wulf, 2009), the design of compensation contracts and promotion policies (Milgrom, 1988; Chan, 1996; Fairburn and Malcomson, 2001; Corgnet and Rodriguez-Lara, 2013) and the firm’s decision to sell assets (Meyer, Milgrom and Roberts, 1992). Influence activities have also been shown to lead to organizational inertia (Schaefer, 1998).

One important insight of the influence costs literature is to show that, in the presence of influence activities, it will be optimal for organizations to limit managers’ discretion over decisions affecting the distribution of resources. This implies that firms may adopt apparently wasteful bureaucratic rules (Bloom and Van Reenen, 2007) for efficiency concerns (Milgrom, 1988; Milgrom and Roberts, 1988, 1992; Powell, 2014). For example, firms may avoid discretionary bonuses to limit influence activities (Fairburn and Malcomson, 2001). Relatedly, Milgrom and Roberts (1992) suggested that extensive use of equal pay may be justified, despite its negative incentives effects, as a way to limit influence costs:

“*One clear way to limit the competition for rents is to equalize their distribution across potential competitors, or at least limit the possible differentials. (…) The cost of the policy is that informational and incentive roles of rewards are muted by closing differentials*.”

(Milgrom and Roberts, 1992, p 274).

The influence costs literature is central to the theory of the firm as it responds to the call for a better understanding of organizational costs:

“*Understanding the strengths and weaknesses of bureaucracy is very underdeveloped compared with understanding the strengths and weaknesses of markets – mainly because bureaucracy is both a comparatively neglected and a formidably difficult subject.*”

(Tadelis and Williamson, 2013, p181).

A recurrent concern, however, with any theory of the firm and with the influence costs theory in particular is the lack of direct evidence. As is argued by Powell (2014), it is inherently difficult to provide a direct test of most theories of the firm because of the impossibility to observe key model variables. This is even more so for influence activities that are typically concealed because of strategic or ethical concerns. We believe, however, that a direct test of the crucial elements of the various theories of the firm is a crucial step in the search of a unifying framework. This is where we see the experimental methodology as providing added value to the literature in organizational economics. Laboratory experiments allow for the control of otherwise unobservable key variables thus permitting to confirm or refute causal relationships (Falk and Fehr, 2003; Charness and Kuhn, 2011; Camerer and Weber, 2013). In that regard, our work is close in spirit to the analysis of Fehr, Hart and Zehnder (2011) who use the lab to provide a direct test of the Hart and Moore (2008) model by assessing whether contracts act as reference points. Our work also relates to the lab study of Grosse, Putterman and Rockenbach (2011) who test the conjecture of Alchian and Demsetz (1972) that monitoring in teams should be assigned to one central monitor.

Building on a stylized model that incorporates crucial ingredients of previous influence costs models we derive key testable conjectures. In particular, we expect workers to engage in time-consuming influence activities that will ultimately destroy firm value.We also expect influence activities to lead managers to set low-powered incentives making lesser use of possibly manipulated performance measures. Finally, we expect that limiting managerial discretion by using an equal pay policy will deter influence activities and lead to higher firm production.

To test these conjectures, we develop a computerized environment that incorporates several features of real-world organizations such as real-effort tasks (e.g Dickinson, 1999; van Dijk, Sonnemans and van Winden, 2001), real-leisure activities (internet browsing) and real-time supervision. Our virtual organization setting (Corgnet, Hernan-Gonzalez and Schniter, 2014) was developed as a response to the call of Camerer and Weber (2013) for designing more “realistic” lab organizations. In this environment, we study organizations with three employees and a boss. Employees can either complete a work task that generates value to the firm or shirk.

Depending on the treatment, employees could also engage in influence activities to inflate their task performance as viewed by the boss. Influence activities did not involve a direct monetary cost but they were time consuming, and as is argued by Milgrom (1988):

“*That time of course is valuable; if it were not wasted in influence activities, it could be used for directly productive activities or simply consumed as leisure*.”

Depending on the treatment, the allocation of firm profits across employees was either fixed to 20% each or chosen by the boss at the end of the period. Regardless of the treatment, the boss always received 40% of firms’ profits. We implemented a 2×2 factorial design such that influence activities were either available or not, and firm profits allocation was either fixed (*fixed pay* treatments) or chosen by the boss (*discretionary pay* treatments).

In line with previous models, we report that influence activities were pervasive and led to substantial costs for the organization. In the treatments with *discretionary pay*, firm production was 12.7% lower when influence activities were available than when they were not. This first result echoes the findings of Carpenter, Matthews and Schirm (2010) who studied sabotage in tournaments with peer evaluations. In a real-effort experiment where workers had to prepare letters and envelopes, the authors compared workers’ production in a tournament setting in which performance was assessed by a supervisor with a tournament in which performance was partly determined by peers. In their setting, politicking activities followed from peers underreporting others’ performance to increase their chance of winning the tournament prize. This led to a lower level of performance in the tournament with peer evaluations than in the tournament setting with supervisor evaluation.

Unexpectedly, we also found in our setting that workers engaged in influence activities when rewarded *equal pay*. In line with the work of Charness, Masclet and Villeval (2014), people who value status per say may engage in influence activities to improve their relative standing (as perceived by the boss) in the organization. Despite this unexpected behavioral effect, we find that, consistently with previous theoretical literature, influence activities led managers to set low-powered incentives by overlooking observed performance when deciding upon workers’ pay. Furthermore, *equal pay* led to higher firm performance than *discretionary pay* when influence activities were available whereas the opposite was true when influence activities were not available.

Our results support the idea that prevalent influence costs and politicking activities may account for the widespread use of bureaucratic, and apparently inefficient, compensation rules in organizations. Our findings confirm the intuition of Gibbons (2005; 2013) that influence costs may serve as a building block to develop a behavioral theory of the internal organization of firms (Powell, 2014).

**2. DESIGN**

*2.1. Virtual Workplace with Real Effort and Real Leisure*

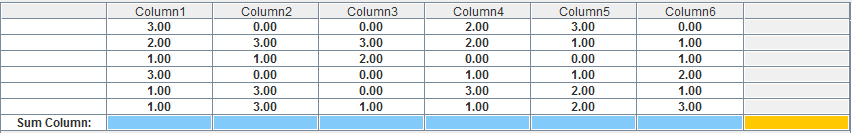
We use a framework in which participants can undertake a real-effort task while having access to Internet at any point in time during the experiment (Corgnet, Hernan-Gonzalez and Schniter, 2014). The experiment consisted of 5 periods of 10 minutes each. The experimental environment is described in detail below, and the complete set of instructions is available as supplementary material.[[2]](#footnote-2)

*2.1.1 Organizational Roles*

We consider organizations with two types of participants referred to as B (agent) and C (principal). At the beginning of the experiment, participants were randomly assigned to one of these two roles. Each principal was then matched with three agents. Participants kept the same role and the same partners for the whole duration of the experiment. During a period, and regardless of the treatment, agents and principals could dedicate their time to either completing the *work task* or *browsing the web*. In addition, principals could monitor agents’ production in real time.

*2.1.2 The Work Task*

We consider a real-effort task that is particularly long, laborious and effortful compared to previous real-effort experiments that have reported the use of counting tasks (e.g. Dohmen and Falk, 2011; Eriksson, Poulsen and Villeval 2009; Niederle and Vesterlund, 2007). In particular, participants were asked to sum up matrices of 36 numbers comprised between 0 and 3 for five periods of ten minutes each. The numbers in each table were generated randomly. Participants were not allowed to use a pen, scratch paper or calculator. This rule amplified the level of effort participants had to exert in order to complete tables correctly. Our work taskwas designed to reduce as much as possible the intrinsic motivation derived from the task itself. An example of the work task is shown in Figure 1.



**FIGURE 1.-** Example of table summation for the work task.

Each table completed correctly generated a 60-cent of individual production while a penalty of 30 cents was subtracted from individual production for each incorrect answer. Penalties did not apply when individual production was equal to zero so that individual production could not be negative.[[3]](#footnote-3)

*2.1.3 Internet Browsing*

At any point during the experiment, agents and principals could switch from the work taskto the leisure activity that consisted of browsing the Internet. Each activity was undertaken separately, in a different screen so that participants could not complete tables while being on the Internet. Participants were informed that their use of the Internet was strictly confidential. Participants were free to consult their email or visit any web page. The Internet browser was embedded in the software (see Figure 2) so that the experimenter could keep record of the exact amount of time participants spent on each activity.

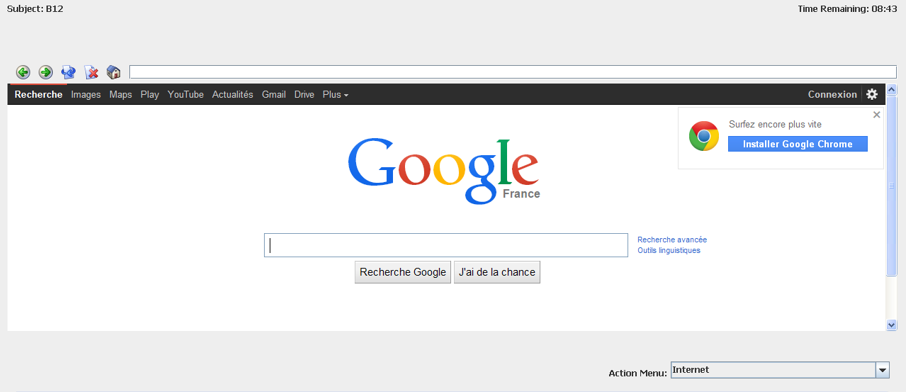


FIGURE 2.- Embedded Internet screen.

The introduction of Internet in our virtual workplace is motivated by the widespread use of Internet at work. According to a 2005 study by *American Online* and *Salary.com*, employees spend about 26% of their time on activities unrelated to their work (Malachowski, 2005). Almost half of this time actually corresponds to Internet usage. An appealing feature of Internet as an alternative to the work task is the wide range of activities that can be completed online.

*2.1.4 Monitoring*

The principal could observe the value of individual production (in cents) of each of the three agents at any time during a period by accessing a separate window with a monitoring screen. Principals could not monitor agents’ activities, however. At the end of each period, the principal received a monitoring summary which indicated the observed individual production of each of the agents the last time the principal accessed the monitoring screen. As a result, principals could easily obtain the maximum amount of information regarding agents’ period production by accessing the monitoring screen in the very last seconds of each period.

*2.1.5 Payment Scheme*

At the end of each of the five periods, agents and principals were rewarded a share of the production of the organization defined as the individual production of the principal and the three agents. Regardless of the treatment, the principal was always rewarded 40% of firm production. In the *equal pay* treatments, each of the three agents was rewarded an equal share of 20% of firm production. In the *discretionary pay* treatments, the principal chose how to allocate the remaining 60% of firm production to the three agents.

*2.1.6 Influence Activities*

In the *Influence* treatments, agents had access to an additional activity which was referred to as *boost* in the experiment. This activity allowed agents to exaggerate their level of production as *observed* by the principal in the monitoring screen. To do so, agents had to choose the amount (in cents) by which they wanted to increase their apparent production. After clicking on a confirmation button, the screen of the agent was frozen for 30 seconds. This aimed at representing the cost associated with time-consuming influence activities that detract agents from the work task (Milgrom, 1988). This 30-second freeze represents 5% of the time available in a given period and about half the time a person needs to complete one table correctly in the work task (see the following study using the same task: Corgnet, Gómez-Miñambres and Hernan-Gonzalez, 2014). Agents could easily keep track of their influence activities in a given period as the total amount by which they exaggerated their production was recorded in the history panel at the bottom of their screen.

Even though influence activities allowed agents to exaggerate their individual production in a given period, the principal knew the *actual* production of the firm at the end of each period. In case agents engaged in influence activities, the *actual* production of the organization was at odd with the sum of the *observed* individual productions in the principal’s monitoring summary.

In the *no influence* treatments, influence activities were not available to agents as the *boost* option was disabled.

*2.2. Treatments and procedures*

We conducted four treatments as part of a 2×2 factorial design (see Table 1).

**TABLE 1.** Factorial design.

|  |  |  |  |
| --- | --- | --- | --- |
| Number of  participants (firms) [sessions] | | **Payment Scheme** | |
| *Equal Pay* | *Discretionary Pay* |
| **Influence Activities** | *No Influence* | 60 (15) [5] | 60 (15) [5] |
| *Influence* | 64 (16) [5] | 64 (16) [5] |

Our participant pool consisted of students from a major US University. The experiments took place in March and April 2014. In total, 252 participants completed the experiment (52% females), divided in 20 sessions. We conducted five sessions for each of the treatment. In each session we had a total of either 12 or 16 participants, which corresponds to either 3 or 4 firms of four individuals (one principal and three agents).

The experiment was computerized and all of the interaction was anonymous. The instructions were displayed on participants’ computer screens. Participants had exactly 20 minutes to read the instructions. A 20-minute timer was shown on the laboratory screen. Three minutes before the end of the instructions period, a monitor announced the time remaining and handed out a printed copy of the summary of the instructions. None of the participants asked for extra time to read the instructions. At the end of the 20-minute instruction round, the instructions file was closed, and the experiment started. The interaction between the experimenter and the participants was negligible.

Upon arrival at the lab and before receiving instructions for the corresponding treatment, participants were asked to sum as many five one-digit numbers as they could during two minutes. Each correct answer was rewarded 10 cents. The number of correct answers is what we refer to as “summation skills”.

Participants were paid their earnings in cash. Individual earnings at the end of the experiment were computed as the sum of the earnings in the 5 periods. Participants earned on average $25.10, including a show-up fee of $7.00. Experimental sessions lasted on average an hour and a half.

**3. CONCEPTUAL FRAMEWORK**

We consider firms which are composed of one principaland *n* agents interacting for periods. We assume both the principal and the agents to be risk neutral, abstracting away from risk-sharing issues, and focusing our attention on the effect of influence activities on contract design and organizational performance (Milgrom and Roberts, 1988; Gibbons, 2005; Powell, 2014).

*Agents*

In each period, agent can either exert effort on the work task () or shirk . Working on the task implies a cost . Exerting effort will generate a positive output for the organization with probability and no output otherwise. We assume that exerting high effort is efficient so that: . Agents can also engage in influence activities or not do so ). Influence activities are costly because agents exerting effort on the task () will have a lower probability of generating a positive output when choosing . This cost represents the fact that engaging in influence activities reduces the amount of time an agent has to complete the work task ultimately jeopardizing task success as in Milgrom (1988). Nonetheless, agents may gain from influence activities as they may affect the principal’s perception of the output of their effort . In particular, whenever agent engages in influence activities the principal will perceive his or her output as = regardless of the actual value of agent ’s output . Influence activities thus allow employees to manipulate the principal’s signal over the agent’s performance in the spirit of signal-jamming models (Fudenberg and Tirole, 1986). These signal-jamming models have been used in previous models of influence activities (e.g. Gibbons 2005; Corgnet and Rodriguez-Lara, 2013; Powell, 2014).

*Principals*

In each period, the principal can either exert effort on the work task () or not . Similarly to agents, the principal incurs a cost for working on the task. Exerting effort will generate a positive output for the organization with probability and no output otherwise . At the end of each period, the principal can observe neither agents’ effort levels , nor influence activities and actual output levels . The principal can only observe the *perceived* output of each employee (). In the absence of influence activities: . The principal’s perception of agents’ levels of output is subject to distorting influence activities. The principal can, however, observe the organization’s total output: . Based on the previous information ( and *Y*), the principal chooses a payment to each agent. The pay of the principal and the agents are subject to the following budget-balanced restriction:

=

Following our experimental design, we also impose that the principal is paid a fixed proportion of the organization’s total output where the remaining share is either allocated equally across the *n* agents (*equal pay*) or is allocated at the discretion of the principal (*discretionary pay*).

*Efficient equilibrium in the absence of influence activities*

This case corresponds to the treatment with endogenous pay allocation without influence activities. Given that , an efficient equilibrium is such that and for all . The principal’s payment scheme consists in paying nothing to agents with a zero output and in paying the following to each of the remaining workers . The efficient equilibrium holds as long as each agent is not willing to shirk:

[1]

The manager will also exert effort as long as: [2]. As a result of conditions [1] and [2], the maximum sustainable pay for the manager is .[[4]](#footnote-4) We assume that [1] and [2] are automatically satisfied so that the efficient equilibrium can be obtained in the absence of influence activities. We now study the efficient equilibrium when influence activities are available. This case corresponds to the *influence* treatment with *discretionary pay*. We consider both the cases in which the manager prevents influence activities in equilibrium (influence-free contracts: ) or not (influence contracts: ).

*Influence-free contracts*

The principal can use two types of contracts to induce agents to work while preventing them from engaging in influence activities: performance pay or equal pay. Under performance pay, the principal’s payment scheme consists in paying nothing to any agent for which and in paying to each of the remaining workers for which . Under equal pay, all agents will be paid the same amount regardless of . Evidently, the principal can also implement a mixed contract using equal pay with probability and performance pay otherwise.

We first show that the type of influence-free contract that implements the efficient equilibrium for the largest set of parameters is equal pay (see Appendix). Under equal pay, influence activities are never appealing so that the only condition necessary to sustain an efficient equilibrium is that high effort is preferred to low effort:

[3]

This condition is more stringent than [1]. As a result of condition [3], the maximum sustainable pay for the principal is where . In other words, there is a smaller set of contracts for which the principal will be exerting effort when influence activities are available than when they are not. This suggests that the principal’s effort is likely to be lower in treatments that allow for influence activities than in treatments that do not.

*Influence contracts*

In this case, influence activities may be observed in an efficient equilibrium (as in Corgnet and Rodriguez-Lara, 2013). We show that the type of influence contracts that implements the efficient equilibrium for the largest set of parameters is performance pay (see Appendix). A condition for this equilibrium to exist is that agents will not choose to shirk:

[4]

Where [4] is more stringent than [3] so that influence contracts implement the efficient equilibrium for a smaller set of parameters than influence-free contracts. The other condition for the performance pay influence contract to hold is that employees do not avoid engaging in influence activities in equilibrium. This will be the case as long as:

[5]

Although we have solved our model considering a single period, our analysis extends to the case of periods. In our setting, relational contracts are not sustainable as the willingness for managers to punish influence activities in the last period will not be credible. Furthermore, the principal cannot observe influence activities and can thus not identified and punish a specific agent. Even in the case in which the principal knew whether some influence activities had been undertaken during a period, she could not punish agents because the contract stipulates that the share of total output assigned to agents must be exactly equal to . This implies that the principal cannot punish the group of agents after observing influence activities. Evidently, one could elaborate a model in which relational contracts arise in equilibrium. One could think of an interesting extension of our model in which agents could observe each other influence activities and develop disciplining devices that limit influence activities and help sustain a relational contract. Even though these extensions are interesting avenues for future research, our present focus is on settings in which influence activities can potentially be observed in equilibrium.

Our framework extensively builds upon previous influence activities models (Milgrom, 1988; Milgrom and Roberts, 1988; Gibbons, 2005; Corgnet and Rodriguez-Lara, 2013, and Powell, 2014) with the aim of capturing a number of essential predictions of the literature. Following our finding that influence contracts can implement the efficient equilibrium, our first hypothesis sates that influence activities may be observed in equilibrium under *discretionary pay*. In this context, influence activities will affect agents’ pay as principals will rely on the manipulated *observed* output for payment. In the spirit of Milgrom and Roberts (1988), principals will be using inaccurate information to make their payment allocation.

**Hypothesis 1** *(Influence activities)*

*i) Under discretionary pay, we expect agents to engage in time-consuming influence activities spending less time working on the task.*

*ii) Under discretionary pay, we expect influence activities to affect agents’ pay.*

*iii) Under equal pay, we expect agents not to engage in time-consuming influence activities.*

In our model, when influence activities are available, equal pay implements the efficient equilibrium for a larger set of parameter values than performance pay whereas the opposite is true when influence activities are not available. In other words, performance pay is expected to be less pervasive when influence activities are available than when they are not. Moreover, the relationship between an employee’s output and pay will be further weakened when influence activities are available. This is the case because performance pay will more likely rely on manipulated measures of output than on the actual value of output . Hypothesis 2 captures these conjectures.

**Hypothesis 2** *(Low-powered incentives)*

*i) We expect principals to rely less on observed performance for agents’ payment when influence activities are available than when they are not.*

*ii) Because of influence activities and low-powered incentives, we expect the relationship between agents’ output and pay* *to be significantly weaker when influence activities are available than when they are not.*

We have also shown that, when influence activities are available, the set of possible contracts implementing the efficient equilibrium is smaller. This implies that agents as well as principals are less likely to exert effort in an environment in which influence activities are available. We summarize these conjectures in Hypothesis 3.

**Hypothesis 3** *(Influence costs)*

*i) Under discretionary pay, we expect firm production to be lower when influence activities are available than when they are not.*

*ii) Under discretionary pay, we expect both agents and principals to produce less when influence activities are available than when they are not.*

It also follows from our model that principals minimize influence costs when using equal pay. That is, principals who refrain from using discretionary pay, even when given the right to do so, will maximize firm production. In particular, we have shown that, when influence activities are available *discretionary pay* cannot lead to higher firm production than *equal pay*. The opposite is true when influence activities are not available as *equal pay* cannot lead to higher firm production than *discretionary pay*. This result echoes an important finding in the influence costs literature according to which limiting managerial discretion is an effective response to influence activities (Milgrom, 1988; Milgrom and Roberts, 1988, 1992; Fairburn and Malcomson, 2001; Bloom and Van Reenen, 2007; Powell, 2014).

**Hypothesis 4** *(On the merit of equal pay)*

*We expect firm production to be at least as high under equal pay as under discretionary pay when influence activities are available. We expect firm production to be at least as high under discretionary pay as under equal pay when influence activities are not available.*

**4. RESULTS**

In our analysis, we use linear and probit panel regressions with random effects and clustered standard errors at the firm level. We thus consider a total of 300 (60 subjects × 5 periods) observations for each of the *no influence* treatments and 320 observations for each of the *influence* treatments. One fourth of these observations correspond to data on principals while the remaining observations correspond to agents. We proceed by testing each of our four hypotheses.

*4.1. Influence Activities (Hypothesis 1)*

In line with Hypothesis 1i, we report that, under *discretionary pay*, a substantial proportion of agents (79.2%) engage at least once in influence activities. People who engage in influence activities spend on average 50.2 seconds (8.4% of their time) each period on influence activities in freeze mode being unable to complete other activities. Overall, people spend an average of 4.6% of their time on the influence activity screen. As a result, under *discretionary pay*, the time spent on the task is significantly lower in the treatment with influence (79.0% of their time) than in the treatment without influence (85.4%) (see *influence dummy* –which takes value 1 for treatments with influence and value 0 otherwise– in the regression analysis in Table 2).

|  |  |  |
| --- | --- | --- |
| **TABLE 2.** Linear panel regression with random effects  for agents’ working time –in seconds–. | | |
| Dependent variable | Working Time  *Discretionary Pay Equal Pay* | |
| Intercept | 485.15\*\*\* (20.92) | 6.219\*\*\* (.586) |
| Period Trend | 2.11 (3.45) | 4.91\*\* (2.08) |
| **Influence Dummy** | -**38.56\*\* (16.42)** | -**19.10 (12.26)** |
| Summation Skills | 1.28 (1.20) | 3.32\*\*\* (0.74) |
| Female Dummy | -2.64 (15.08) | 9.74 (13.94) |
| Observations (organizations)  R² | n = 465 (31)  0.035 | n = 465 (31)  0.048 |
| Estimation output using robust standard errors clustered at the organization level (in parentheses).  \*p-value<.10, \*\*p-value<.05, and \*\*\*p-value<.01 | | |

To study the effect of influence activities on agent pay under *discretionary pay*, we classify influence activities as either *detectable* or *undetectable*. In our setting, some influence activities can indeed be detected by the principal. Because each correct table generates 60 cents while an incorrect answer implies a 30 cent penalty, an agent production level is necessarily a multiple of 30. An agent who decides to exaggerate his or her production by an amount which is not a multiple of 30 is considered as engaging in *detectable* influence activities. An agent who is exaggerating his or her production by an excessive amount is also considered as engaging in *detectable* influence activities. Agents whose observed production is excessively high are more likely to have engaged in influence activities and may thus be punished by the principal.

We define as excessive the influence activities that lead agents’ production (as *observed* by the principal) to be in the top quartile of agents’ production levels in the *discretionary pay* treatment without influence (600¢). We consider some robustness analysis by considering different thresholds to define excessive influence activities: 540¢ (top 35%), 660¢ (top 20%) and 720¢ (top 10%) (see Table 3). These thresholds imply that the respective proportions of *undetectable* influence activities are 22.2%, 26.2%, 30.0% and 33.3%. Our regression results suggest that an agent can increase his or her *discretionary pay* by engaging in influence activities as long as such activities are *undetectable* (see *undetectable influence dummy* –which takes value 1 if an agent is engaging in influence activities which are defined as *undetectable* according to the corresponding threshold and value 0 otherwise– in the regression analysis in Table 3).

A special case of *undetectable* influence activities are those of agents who exaggerate their period production by the smallest possible amounts (one or two correct tables: 60¢ or 120¢). These amounts are actually the ones which are most likely to be chosen by agents (in 11.9% and 11.1% of the cases for 60¢ and 120¢). By defining *undetectable* influence activities as those involving exaggerating one’s own production by either 60¢ or 120¢, we obtain similar results as the ones presented in Table 3 where the p-values for the *undetectable* *influence activities* *dummy* is equal to 0.040. These results are robust to controlling for the possible negative effect of highly *detectable* influence activities. For example, after introducing a dummy variable that takes value one if an agent is exaggerating his or her production by more than two (three) [four] tables, the p-values associated to the *undetectable* *influence activities dummy* are 0.033 (0.031) [0.030].

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| --- | --- | --- | --- | --- | --- |
| **TABLE 3.** Linear panel regressions assessing the effect of undetectable influence activities on discretionary pay. | | | | | |
|  | *Undetectable influence activities thresholds* | | | | |
|  | Top 35%  540¢ | Top 25%  600¢ | | Top 20%  660¢ | Top 10%  720¢ |
| Dependent variable | *Discretionary pay (%)* | | | | |
| Intercept | 21.296\*\*\*  (2.999) | | 21.225\*\*\*  (3.015) | 21.704\*\*\*  (2.938) | 21.738\*\*  (2.921) |
| Period Trend | -0.027  (0.049) | | -0.009  (0.059) | -0.001  (0.029) | -0.013  (0.035) |
| Actual contribution (%) | 0.351\*\*\*  (0.094) | | 0.351\*\*\*  (0.094) | 0.344\*\*\*  (0.094) | 0.345\*\*\*  (0.095) |
| **Undetectable Influence Dummy** | **4.294\*\***  **(2.195)** | | **4.299\*\***  **(2.118)** | **1.953◊**  **(1.370)** | **2.030◊**  **1.345** |
| Summation Skills | -0.044  (0.185) | | -0.049  (0.178) | -0.050  (0.185) | -0.054  (0.181) |
| Female Dummy | -1.114  (2.039) | | -1.119  (2.108) | -1.169  (2.176) | -1.167  (2.160) |
| Number of observations (organizations)  R² | n = 240 (16)  0.255 | | n = 240 (16)  0.259 | n = 240 (16)  0.242 | n = 240 (16)  0.243 |
| Estimation output using robust standard errors clustered at the organization level (in parentheses).  \*p-value<.10, \*\*p-value<.05, \*\*\*p-value<.01 and **◊***one-tailed* p-value<.10. | | | |  |  |

Surprisingly and in contrast with Hypothesis 1ii, a substantial proportion of agents (68.7%) also engaged at least once in influence activities under *equal pay* compared to 79.2% under *discretionary pay* (Normal proportion test, p-value = 0.18). On average, agents spent 3.2% of their time undertaking influence activities under *equal pay* compared to 4.6% under *discretionary pay*. This difference is not statistically significant, however(p-value = 0.68). Under *equal pay*, influence led agents to reduce the proportion of their time agent spent on the task (82.8% compared to 85.5% without influence), although this difference was not statistically significant (see *influence dummy* in column 2 of Table 2).

As a result, influence activities are almost as pervasive under *equal pay* as they are under *discretionary pay*. This suggests that, in addition to the potential monetary gains associated to influencing the principal, agents care about their relative standing in the organization. This finding is consistent with recent evidence showing that people are willing to engage in costly actions to improve their relative standing despite being paid a flat wage (Charness, Masclet and Villeval, 2014).

Our findings show that influence activities prevail not only in environments where pay is discretionary but also in environments where pay is fixed. This finding echoes previous literature on influence costs and information manipulation because it confirms that influence activities are widespread. At the same time, it calls for extending previous models to the case in which people engage in manipulative attempts for status considerations in addition to pecuniary motives.

*4.2. Low-powered incentives (Hypothesis 2)*

We study Hypothesis 2i by assessing the relationship between an agent’s *observed contribution* to the organization and his or her actual pay. We define an agent’s *observed contribution* as the ratio between his or her observed production as *observed* by the principal and the *observed production* of all agents in the organization. We define an agent’s *observed production* as the sum of his or her *actual production* and the amount by which the agent has exaggerated his or her production by engaging in influence activities. In Table 4, we port our regression findings for the whole sample (see last two columns of Table 4) as well as for the more sensitive case in which principals did monitor the agent’s production. In the case in which principals did not spend any time monitoring the agent’s production, the relationship between agents’ contributions and pay is necessarily inexistent regardless of the treatment. Even though monitoring the agent was quick and easy, two principals (out of 15) in the *discretionary pay* treatment without influence activities did not spend any time in the whole experiment monitoring agents’ production. In the *discretionary pay* treatment with influence activities, all principals spend at least some time monitoring the agents’ production. However, the *influence* treatment did not significantly differ in the percentage of time principals allocated to watching agents (5.5%) compared to the treatment without influence (4.0%) (p-value = 0.84).[[5]](#footnote-5)

The results reported in Table 4 are in line with Hypothesis 2i. The relationship between an agent’s pay and his or her *observed* contribution is weaker in the treatment with influence activities than in the treatment without influence activities. This result follows from the fact that the coefficient associated to the interaction terms between an agent’s *observed* contribution and the *influence* *treatment dummy* are negative and largely significant. The significance of these coefficients is only marginal when including in the analysis principals who did not watch the agent (see last two columns of Table 2). We report similar results when looking at the relationship between an agent’s *actual contribution* and his or her actual pay (Hypothesis 2ii). The interaction term between an agent’s actual contribution and the *influence treatment dummy* is negative and significant when considering the case of principals who monitored the agents. However, the interaction term is still negative but no longer significant when considering the whole sample (p-value = 0.252).

It is also interesting to observe that the interaction term between an agent’s summation skills and the *influence* *treatment dummy* is negative and significant for all regressions in Table 4. This finding complements Hypothesis 2 by showing that influence activities not only weaken the relationship between effort and pay but that they also weaken the relationship between ability and pay. That is, the use of low-powered incentives by the principal in the *influence* treatment tends to level off agents’ performance across ability levels.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **TABLE 4.** Linear panel regressions for *discretionary pay* as a function ofagents’ contributions for the treatments with and without influence activities | | | | |
| Dependent variable | *Discretionary share (in %)* | | | |
|  | *Monitoring Principals* | | *All sample* | |
| Intercept | 5.986\*  (3.103) | 5.757\*  (3.065) | 11.486\*\*\*  (3.716) | 11.373\*\*\*\*  (3.688) |
| Period Trend | -0.004  (0.016) | 0.001  (0.018) | -0.001  (0.001) | -0.001  (0.001) |
| Influence Dummy | 16.675\*\*\*  (4.181) | 17.738\*\*\*  (4.876) | 12.820\*\*\*  (4.402) | 13.494\*\*\*  (4.984) |
| Actual contribution (%) | 0.556\*\*\*  (0.065) | - | 0.556\*\*\*  (0.065) | - |
| **Act. cont. × Influence Dummy** | -**0.231\*\***  **(0.094)** | - | -**0.138**  **(0.121)** | - |
| Observed contribution (%) | - | 0.568\*\*\*  (0.065) | - | 0.403\*\*\*  (0.104) |
| **Obs. cont. × Influence Dummy** | - | -**0.323\*\***  **(0.142)** | - | -**0.201◊**  **(0.153)** |
| Summation Skills | 0.558\*\*\*  (0.144) | 0.558\*\*\*  (0.143) | 0.571\*\*\*  (0.184) | 0.573\*\*\*  (0.184) |
| **Sum. Skill. × Influence Dummy** | -**0.598\*\***  **(0.243)** | -**0.467\*\***  **(0.236)** | -**0.534\*\***  **(0.240)** | -**0.441\***  **(0.327)** |
| Female Dummy | -0.832  (1.509) | -0.454  (1.176) | -0.248  (1.331) | -0.575  (1.116) |
| Number of observations (organizations)  R² | n = 366 (29)  0.526 | n = 366 (29)  0.502 | n = 465 (31)  0.394 | n = 465 (31)  0.363 |
| Estimation output using robust standard errors clustered at the organization level (in parentheses).  \*p-value<.10, \*\*p-value<.05, \*\*\*, p-value<.01 and **◊***one-tailed* p-value<.10. | | | | |

*4.3. Influence costs (Hypothesis 3)*

We start by assessing Hypothesis 3i according to which, under *discretionary pay*, firm production is expected to be lower in the *influence* treatment than in the *no-influence* treatment. We confirm this conjecture as average firm production per period is on average 12.7% lower in the *no-influence* treatment ($16.25) compared to the *influence* treatment ($18.62). This difference is significant because the coefficient associated to the *influence dummy* (which takes value 1 if influence activities are available and 0 otherwise) is negative and significant (see Table 5 below).[[6]](#footnote-6)

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| **TABLE 5.** Linear panel regression with random effects for firm production, agents’ production and principals’ production per period (in cents) for the two *discretionary pay* treatments. | | | | | |
| Dependent variable | Firm  Production | Agents  Production | | Principals’  Production | |
| Intercept | 1483.13\*\*\*  (118.38) | | 20.88  (42.38) | | 53.55  (88.02) |
| Period Trend | 126.290\*\*\*  (22.126) | | 31.16\*\*  (6.56) | | 32.81\*\*\*  (8.22) |
| **Influence Dummy** | -**236.75\*\***  **(122.54)** | | -**59.34\***  **(31.52)** | | -**98.30\*\***  **(43.86)** |
| Ability (Summation Skills) | - | | 21.84\*\*\*  (2.62) | | 18.54\*\*\*  (4.35) |
| Female Dummy | - | | -22.30  (33.57) | | -82.23\*  (45.37) |
| Observations (organizations)  R² | n = 155 (31)  0.187 | | n = 465 (31)  0.271 | | n = 155 (31)  0.307 |
| Estimation output using robust standard errors clustered at the organization level (in parentheses).  \*p-value<.10, \*\*p-value<.05, and \*\*\*p-value<.01 | | | | |  |

In line with Hypothesis 3ii, we also report that both agents’ and principals’ production levels are significantly lower in the *discretionary pay* treatment with influence activities ($4.07 and $4.03 for agents and principals) than without influence activities ($4.58 and $4.88) (see columns 2 and 3 of Table 5).

*4.4. On the merit of equal pay (Hypothesis 4)*

In line with Hypothesis 4, we observe that the gap in average firm production between the *discretionary* and *equal pay* treatments is lower for *influence* treatments (-$1.39) than for *no-influence* treatments ($2.28). This difference is statistically significant because the interaction term between the *influence dummy* and the *discretionary pay dummy* (which takes value 1 if principals can use discretionary pay) is negative and significant (p-value = 0.03, see Table 6). Interestingly, *equal pay* even leads to a higher level of production (18.62) than *discretionary pay* (16.25) when influence activities are available. This difference is not significant, however (p-value = 0.19).[[7]](#footnote-7)

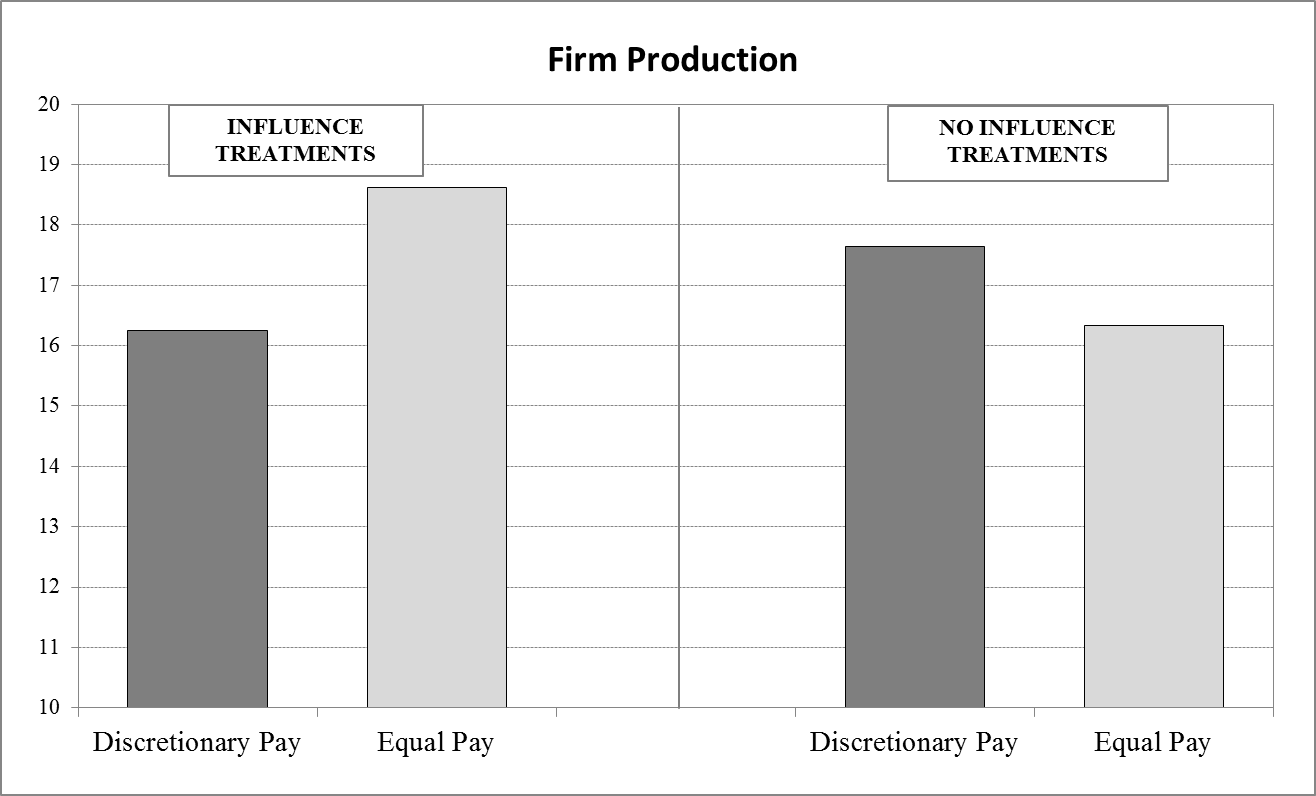


FIGURE 3.- Average firm production across treatments.

**TABLE 6.** Linear panel regression with random effects for firm production (in cents)

across all treatments as a function of treatment dummies.

|  |  |
| --- | --- |
| Dependent variable | Organization  Production |
| Intercept | 97.28 (252.16) |
| Period Trend | 114.34\*\*\* (14.90) |
| Influence Dummy | 64.56 (95.22) |
| Discretionary Pay Dummy | 186.65\* (106.97) |
| **Disc. Pay Dummy × Inf. Dummy** | -**311.65\*\* (143.57)** |
| Ability (Summation Skills) | 20.01\*\*\* (3.97) |
| Observations (organizations)  R² | n = 310 (62)  0.288 |

Our findings thus support the general recommendation derived from the influence cost literature to give away managerial discretion to limit the negative consequences of influence activities.

**5. DISCUSSION**

Organizational scientists have relied on influence costs and politicking to explain why firms cannot grow indefinitely (e.g. Gibbons, 2005; Powell, 2014). One important implication of the influence costs literature is that bureaucratic rules limiting managerial discretion may be optimal to deter manipulative attempts (Milgrom, 1988; Milgrom and Roberts, 1988, 1992; Fairburn and Malcomson, 2001; Bloom and Van Reenen, 2007; Powell, 2014). However, the influence cost literature does not provide direct evidence of the pervasiveness of influence activities in organizations and of their impact on the internal organization of firms. Our study attempts to fill this gap by assessing, in a controlled yet realistic work environment, the magnitude of influence costs and the implications of influence activities on compensation policies.

We find support for the main conjectures of the influence costs literature by first showing that influence activities are pervasive and are largely detrimental to firm performance. In the treatments with *discretionary pay*, firm production was 12.7% lower when influence activities were available than when they were not. Unexpectedly, we found that workers also engaged in influence activities when rewarded an *equal pay*. This finding which seems consistent with recent research showing that people value their relative standing (as perceived by the boss) in the organization suggests extending previous influence-cost model to include status and non-monetary considerations.

Consistently with previous theoretical literature, we showed that influence activities led managers to set low-powered incentives. This was the case because managers reacted to pervasive influence activities by overlooking observed performance when deciding upon workers’ pay. Also in line with theoretical conjectures, *equal pay* led to higher firm performance than *discretionary pay* when influence activities were available whereas the opposite was true when influence activities were not available. This supports the idea that prevalent influence costs and politicking activities may account for the widespread use of bureaucratic, and apparently inefficient, compensation rules in organizations.

Our work thus provides large empirical support for a number of key predictions derived from the influence costs literature. Yet, further empirical research is necessary to assess the role that influence costs should play in developing a unified theory of the firm. For example, one could assess, using a similar setting as the present study, the relationship between influence costs and firm size. More generally, we believe that the empirical approach presently developed could be further extended to provide a comprehensive test of competing theories of the firm.

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**7. APPENDIX**

INFLUENCE-COST MODEL

*Influence-free contracts*

We show that the influence-free contract that implements the efficient equilibrium for the largest set of parameters is equal pay. To show this, we consider the general case of mixed contracts that combine both equal pay (w.p. ) and performance pay (w.p. ).

A mixed contract will implement the efficient equilibrium as long as the following conditions are satisfied:

- Agents are not willing to deviate by exerting low effort in equilibrium:

[A1]

- Agents are not willing to engage in influence activities in equilibrium:

[A2]

Where condition [B2] is relaxed as increases. This implies that the largest set of parameters for which this condition holds is for equal pay () in which case [A2] always holds. Given that [A1] does not depend on , we can conclude that equal pay is the influence-free contract that implements the efficient equilibrium for the largest set of parameters.

*Influence contracts*

Using a similar approach to the case of influence-free contracts, the condition for influence contracts to implement the efficient equilibrium are such that:

- Agents are not willing to deviate by exerting low effort in equilibrium:

[A3]

- Agents are not willing to avoid influence activities in equilibrium:

[A4]

Where condition [A4] is relaxed as decreases. This implies that the largest set of parameters for which this condition holds is for performance pay (). For , [A4] holds whenever . Given that [A3] does not depend on , we can conclude that performance pay is the influence contract that implements the efficient equilibrium for the largest set of parameters.

1. These costly activities were early recognized by the proponents of the behavioral theory of the firm (March and Cyert, 1963). [↑](#footnote-ref-1)
2. A video presentation of the basic features of the software is available here:

   <https://sites.google.com/site/vopeerpressure/home/videos/baseline-treatments>.

   Note that in the current study no clicking task was used. [↑](#footnote-ref-2)
3. Individuals could thus not sabotage the organization’s output. [↑](#footnote-ref-3)
4. This result also holds when considering influence contracts since the set of parameters implementing the efficient equilibrium under influence contracts is smaller than under influence-free contracts. [↑](#footnote-ref-4)
5. Using the same panel regression as in Figure 2 with the amount of time (in seconds) principals spend watching agents in a given period, the p-value on the *influence dummy* coefficient was 0.120. [↑](#footnote-ref-5)
6. Note that firm production as well as agents’ and principals’ production levels increase overtime, regardless of the treatment (all p-values < 0.005 for the Period Trend coefficient when using a similar regression as in Table 6 for each of the four treatments separately). This is consistent with the learning effects commonly found in real-effort tasks involving mental calculations (Charness and Campbell, 1988; Corgnet, Hernan-Gonzalez and Rassenti, 2014a, 2014b). [↑](#footnote-ref-6)
7. The p-value follows from testing whether the coefficients associated to the following two variables are equal: Disc. Pay Dummy × Inf. Dummy and Discretionary Pay Dummy. [↑](#footnote-ref-7)