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On the Merit of Equal Pay: When Influence Activities Interact with Incentive Setting

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When Influence Activities Interact with 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 allow employees to influence managers' decisions about rewards. We find that influence activities are pervasive and significantly lower organizational performance. Organizational performance suffers because principals offer weaker incentives when influence activities are allowed than when 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,
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"Where different parts of the organization have responsibility for different pieces of information relevant to a decision, we would expect some biases in information transmitted due to perceptual differences among the subunits and *some attempts to manipulate information* as a device for manipulating the decision."

Cyert & March, 1963, 2013, page 67

1. INTRODUCTION

Office politicking and influence activities at the workplace are pervasive and as much as 60% of employees "believe that involvement in office politics is at least somewhat necessary to get ahead" (Conner, 2013). As reported by Dobson and Dobson (2001, page 32), many individuals who get promoted "probably didn't get their promotions based solely on job performance".

Although workers may personally benefit from engaging in office politics that bias organizational decisions in their favor, the literature on influence activities has highlighted the substantial organizational costs associated to these timewasting activities. 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). The influence costs literature also plays an important role in enhancing our understanding of the boundaries of the firms and in providing a unified theory 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 as well as markets by having managers 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 constraints 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]

One important insight of the influence costs literature is to show that, in the presence of influence activities, it may 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, page 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, page 181).

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 likely to be 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

[1] These costly activities were early recognized by the proponents of the behavioral theory of the firm (Cyert and March, 1963).

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) theory of the firm according to which team monitoring 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. Our model uses a standard moral-hazard principal-agents setup in which the principal observes the output of effort of the agents without directly o their level of effort. We add to the standard setting the possibility for agents to engage in (unobservable) influence activities to manipulate the performance measure (output of effort) observed by the manager. We assume that influence activities are costly because they waste agents' productive time ultimately jeopardizing task success (see Milgrom, 1988). Nonetheless, an agent may personally gain from influence activities as long as they positively affect the principal's perception of the agent's output and lead to an increase in pay.

Our model first predicts that employees will be willing to engage in time-consuming influence activities that will ultimately damage organizational performance. The model also shows that influence activities will lead managers to set low-powered incentives making lesser use of possibly manipulated performance measures. Finally, our model predicts that limiting managerial discretion by using an equal pay policy will deter influence activities and lead to higher organizational 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 (Corgnet, Hernan-Gonzalez and Schniter, 2014). Our virtual organization setting was developed as a response to the call of Camerer and Weber (2013) for designing more "realistic" lab organizations. Our virtual workplace environment considers organizations with three employees and one manager who differ in the actions they can complete. In each organization, employees and managers can complete a work task that generates value to the organization or shirk. Depending on the treatment, employees could also engage in influence activities to inflate their task performance as viewed by the manager. Influence activities did not involve a direct monetary cost but, in line with Milgrom's conjecture (1988, page 43), they wasted employees' productive time: "*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*."

Regardless of the treatment, the manager always received a fixed share of the organization's profits. Depending on the treatment, the allocation of organization profits across employees was either fixed to an equal share of remaining profits or chosen by the manager at the end of the period. We implemented a 2×2 factorial design in which influence activities were either available or not, and profit allocation was either fixed (*fixed pay* treatments) or chosen by the manager (*discretionary pay* treatments).

In line with previous models in the influence costs literature, we report that influence activities were pervasive and led to substantial costs for the organization. In the treatments with *discretionary pay*, organizational production was significantly 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 that employees 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 manager) in the organization, even in the absence of monetary incentives to do so. 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 employees' pay. Furthermore, *equal pay* led to higher organizational 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 thus confirm the intuition of Gibbons (2005) and Gibbons, Matouschek and Roberts (2013) that influence costs may serve as a building block to develop a behavioral theory of the internal organization of firms (Powell, 2014).

2. CONCEPTUAL FRAMEWORK

We consider organizations which are composed of one principal and n agents interacting for q 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 (see Milgrom and Roberts, 1988; Gibbons, 2005; Powell, 2014).

Agents

In each period, agent $i \in \{1, \dots, n\}$ can either exert effort on the work task $(e_i = 1)$ or shirk $(e_i = 0)$. Working on the task implies a cost $c > 0$. Exerting effort will generate a positive output for the organization $(y_i = y)$ with probability $(\rho > 0)$ and no output otherwise $(y_i = 0)$. We assume that exerting high effort is efficient so that $(\rho y_i \geq c)$. Agents can also engage in influence activities $(f_i = 1)$ or not do so $(f_i = 0)$. Influence activities are costly because agents exerting effort on the task $(e_i = 1)$ will have a lower probability of generating a positive output $\left(\rho_f \leq \rho\right)$ when choosing to influence $(f_i = 1)$. This cost represents the fact that engaging in influence activities reduces the amount of time an agent can spend on the work task ultimately jeopardizing task success (see Milgrom, 1988). Nonetheless, agents may gain from influence activities as they may affect the principal's perception of the output of their effort $(\hat{y}_i)$. In particular, whenever agent i engages in influence activities, the principal will perceive his or her output as $\hat{y}_i$=y regardless of the actual value of agent i's output (y_i). 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). Signal-jamming has previously been used to model influence activities (e.g. Gibbons, 2005; Corgnet and Rodriguez-Lara, 2013; or Powell, 2014).

Principals

In each period, the principal can either exert effort on the work task ($e_P = 1$) or not ($e_P = 0$). Similarly to agents, the principal incurs a cost $c > 0$ for working on the task. Exerting effort will generate a positive output for the organization ($y_P = y$) with probability ($\rho > 0$) and no output otherwise ($y_P = 0$). At the end of each period, the principal can observe neither agents' effort levels ($e_i = 0$), nor influence activities (f_i) and actual output levels (y_i). The principal can only observe the *perceived* output of each employee ($\hat{y}_i$). The principal's perception of agents' levels of output is subject to distorting influence activities. In the absence of influence activities: $\hat{y}_i = y_i$. The principal can, however, observe the organization's total output:

$$Y \coloneqq \sum_{i=1}^{n} y_i + y_P.$$

The pay of the principal (w_P) and the agents (w_i) are subject to the following budget-balanced restriction:

$$\sum_{i=1}^{n} w_i + w_P = \sum_{i=1}^{n} y_i + y_P$$

We also consider the case in which the principal is paid a fixed share ($0 < \alpha < 1$) of the organization output. The remaining share ($1 - \alpha$) of the organization output is entirely allocated to agents following one of two possible schemes: *equal pay* or *discretionary pay*. Under *equal pay*, the remaining share of the organization output is allocated equally across the *n* agents. Under *discretionary pay*, the remaining share of the organization output is allocated at the discretion of the principal after observing $\hat{y}_i$ and *Y*.

Efficient equilibrium when influence activities are not available

Making influence activities unavailable to agents is equivalent to solve the previously described model assuming $\rho_f = \rho$. Given that $\rho y_i \geq c$, an efficient equilibrium is such that $e_P^* = 1$ and $e_i^* = 1$ for all $i \in \{1, \dots, n\}$. The principal's payment scheme consists in paying nothing to agents with a zero output ($y_i = 0$) and in paying a share of total output $\frac{(1-\alpha)Y}{k}$ to each of the remaining $k \leq n$ agents with a positive output ($y_i = y$). The efficient equilibrium holds as long as each agent is not willing to shirk:

$$\frac{c}{y} \leq (1 - \alpha)\frac{n+1}{n}\rho \qquad [1]$$

The principal will also exert effort as long as: $\frac{c}{y} \leq \alpha\rho$ [2]. As a result of conditions [1] and [2], the maximum sustainable pay for the principal is $\alpha_P = max\{1 - \frac{\frac{n}{n+1}c}{y\rho}, 0\}$. 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. We consider both the cases in which the principal prevents influence activities in equilibrium (influence-free contracts: $f_i^* = 0$) or not (influence contracts: $f_i^* = 1$).

Efficient equilibrium when influence activities are available

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 $\hat{y}_i = 0$ and in paying $\frac{(1-\alpha)Y}{k}$ to each of the remaining $k \leq n$ agents for which $\hat{y}_i = y$. Under equal pay, all agents will be paid the same amount $\frac{(1-\alpha)Y}{n}$ regardless of $\hat{y}_i$. Evidently, the principal can also implement a mixed contract using equal pay with probability (ξ) 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 A). 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:

$$\frac{c}{y} \leq (1-\alpha)\frac{\rho}{n} \qquad [3]$$

This condition is more stringent than [1]. As a result of condition [3], the maximum sustainable pay for the principal is $\alpha_P^- = max\{1 - \frac{nc}{y\rho}, 0\}$ where $\alpha_P^- \leq \alpha_P$. 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.

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 $(f_i^* = 1)$ that implements the efficient equilibrium for the largest set of parameters is performance pay (see Appendix A). A condition for this equilibrium to exist is that agents will not shirk:

$$\frac{c}{y} \leq (1-\alpha)\frac{\rho_f}{n} \qquad [4]$$

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:

$$\rho^2 \leq \rho_f \qquad [5]$$

Although we have solved our model considering a single period, our analysis extends to any finite number of periods $1 < q < \infty$ periods. In our setting, relational contracts are not sustainable as the willingness for principals 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 $(1-\alpha)$. This implies that the principal cannot punish all agents at once after observing influence activities in the organization. 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 $(\hat{y}_i)$ for payment. In the spirit of Milgrom and Roberts (1988), principals will be using inaccurate information to make their payment allocation. In equilibrium, however, all agents will engage in influence activities (in the influence contracts equilibrium) implying that each agent will be rewarded an equal share of organizational output. An agent who deviates from equilibrium and decides not to engage in influence activities would receive a lower pay than an agent who engages in influence activities.

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 agents who engaged in influence activities to obtain a higher pay than those who do not engage in influence activities.
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 (y_i) and pay (w_i) 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 $(\hat{y}_i)$ than on the actual value of output (y_i). Hypothesis 2 states 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 are less likely to exert effort in an environment in which influence activities are available leading to lower organizational production. We summarize this conjecture in Hypothesis 3.

Hypothesis 3 *(Influence costs)*
Under discretionary pay, we expect the production of the organization to be lower 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 organizational production. In particular, we have shown that, when influence activities are available, *discretionary pay* cannot lead to higher organizational production than *equal pay*. The opposite is true when influence activities are not available as *equal pay* cannot lead to higher organizational 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)*
i) We expect the production of the organization to be at least as high under equal pay as under discretionary pay when influence activities are available.
ii) We expect the production of the organization to be at least as high under discretionary pay as under equal pay when influence activities are not available.

To test these conjectures, we develop the computerized experimental environment detailed below.

3. EXPERIMENTAL DESIGN AND PROCEDURES

3.1. The Virtual Workplace

We use an environment in which participants can undertake a real-effort task while having access to Internet (real leisure alternative) at any point in time during the experiment (Corgnet, Hernan-Gonzalez and Schniter, 2014). The introduction of Internet as the real leisure alternative 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.

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]

We considered 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.

We consider a particularly long, laborious and effortful summation task (e.g. Dohmen and Falk, 2011; Eriksson, Poulsen and Villeval, 2009; Niederle and Vesterlund, 2007) which was designed to reduce as much as possible the intrinsic motivation derived from the task itself. 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. An example of the work task is shown in Figure 1.

	Column1	Column2	Column3	Column4	Column5	Column6	
	3.00	0.00	0.00	2.00	3.00	0.00	
	2.00	3.00	3.00	2.00	1.00	1.00	
	1.00	1.00	2.00	0.00	0.00	1.00	
	3.00	0.00	0.00	1.00	1.00	2.00	
	1.00	3.00	0.00	3.00	2.00	1.00	
	1.00	3.00	1.00	1.00	2.00	3.00	
Sum Column:							

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

Each table completed correctly generated 60 cents of individual production while a penalty of 30 cents was subtracted from individual production for each incorrect answer. Penalties did not

[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.

apply when individual production was equal to zero so that individual production could not be negative.[3]

At any point during the experiment, agents and principals could switch from the work task to the leisure activity (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 so that the experimenter could keep record of the exact amount of time participants spent on each activity.

In addition to working on the task and browsing the internet, 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. That is, principals were not informed of whether agents were spending time on the work task or on the web. 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.[4]

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 sum of the individual production of the principal and the three agents. Regardless of the treatment, the principal was always rewarded 40% of organizational production. In the *equal pay* treatments, each of the three agents was rewarded an equal share of 20% of organizational production. In the *discretionary pay* treatments, the principal chose how to allocate the remaining 60% of organizational production to the three agents.

3.2. 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 *observed* production. Agents could use the *boost* options as many times as they wanted in a given period. After clicking on a confirmation

[3] Individuals could thus not sabotage the organization's output.
[4] 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.

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 represented 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.[5] 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 organization at the end of each period. In case agents engaged in influence activities, the *actual* production of the organization differed from the sum of the *observed* individual productions in the principal's monitoring summary. In addition, individual influence activities could have been detected by the principal if an agent chose an influence amount which was not a multiple of 30. This is the case because each correct table generated 60 cents while an incorrect answer implied a 30 cent penalty. Thus, choosing an influence amount which was not a multiple of 30 would have indicated to the principal that the agent had engaged in influence activities.[6]

In the *no influence* treatments, influence activities were not available to agents as the *boost* option was disabled. We conducted four treatments as part of a 2×2 factorial design (see Table 1).

TABLE 1. Factorial design.

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

[5] This estimate was computed using data from Corgnet, Gómez-Miñambres and Hernan-Gonzalez (2014) who used the same task for a different experiment on goal setting.
[6] The fact that agents could engage in *detectable* influence activities was not considered in the model presented in Section 2. The rationale was that individuals would not engage in such *detectable* influence activities in equilibrium.

3.3. Procedures

Our participant pool consisted of students from a major US University. The experiments took place in March and April 2014. In total, 248 participants completed the experiment (52% females), divided in 20 sessions. We conducted five sessions for each treatment. In each session, we had a total of either 8, 12 or 16 participants, which corresponds to either 2, 3 or 4 organizations 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 and the average earnings on this task were $1.5. The number of correct answers is what we refer to as "summation skills" in the rest of the paper.

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 $25.1, including a show-up fee of $7. Experimental sessions lasted on average an hour and a half.

4. RESULTS

In our analysis, we use linear and probit panel regressions with random effects and clustered standard errors at the organization 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%) engaged (at least once) in influence activities. On average, agents used influence 4.6 times in the duration of the experiment. In a given period, an average of 55.0% of the agents engaged in influence activities (see Table 2). On average, the agents who engaged in influence activities exaggerated their production by 148.0¢ which represented 45.7% of their production level.

TABLE 2. Summary of Influence activities for both the *discretionary pay* and the *equal pay* treatments.

Influence Activities	**Payment Scheme**		
(*Standard deviation in parentheses*)	*Discretionary Pay*	*Equal Pay*	*p-value*[7]
Proportion of agents who engaged in influence activities in a given period	55.0% (49.8%)	35.8% (48.0%)	0.001
Average number of times agents engaged in influence activities in the whole experiment	4.6 (8.2)	3.1 (7.9)	0.058
Average influence amount	148.0¢ (175.4¢)	110.8¢ (171.4¢)	0. 253
Influence amount as proportion of agent production	45.7% (83.1%)	38.7% (62.9%)	0.652

Each time an agent engaged in an influence activity (clicking on the *boost* option), a frozen screen appeared for 30 seconds preventing agents from completing any other activities such as working on the task or browsing the web. As a result of influence activities, agents spent an average of 4.6% of their time with a frozen screen. Agents who engaged in influence activities in a given period spent an average of 50.2 seconds (8.4% of their time) with a frozen screen. It follows that, under *discretionary pay*, the time agents spent on the task was significantly lower in the treatment with influence (79.0% of their time) than in the treatment without influence

[7] Consistently with the rest of the results section, this p-value was computed using a panel regression with random effects. The reported p-value corresponds to the coefficient of the *influence dummy* (which takes value one for *influence* treatments and value zero for *no influence* treatments). We use a gender dummy and summation skills as controls. Similar results are obtained using standard parametric and non-parametric tests.

(85.4%) (see *influence dummy* –which takes value 1 for treatments for which influence activities were available and value 0 otherwise– in the regression analysis in Table 3).

TABLE 3. Linear panel regression with random effects for agents' working time –in seconds–.

Dependent variable	Working Time *Discretionary Pay*	Working Time *Equal Pay*
Intercept	485.15*** (20.92)	453.276*** (16.54)
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)
Gender Dummy (1 if female)	-2.64 (15.08)	9.74 (13.94)
Observations (organizations)	n = 465 (31)	n = 465 (31)
R^2	0.035	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

The conjecture summarized in Hypothesis 1ii regarding the positive effect of influence activities on agent pay is based on the assumption that influence activities are *undetectable* by the principal. Thus, to test Hypothesis 1ii, we first need to classify influence activities as either *detectable* or *undetectable*. Because each correct table generated 60 cents while an incorrect answer implied a 30-cent penalty, an agent production level was necessarily a multiple of 30. An agent who decided to exaggerate his or her production by an amount which was not a multiple of 30 was thus engaging in *detectable* influence activities. An agent who exaggerated his or her production by a large amount could also be seen as engaging in *detectable* influence activities.

An example of influence activities which are very unlikely to be detected by the principal are such that agents exaggerated their period production by the smallest possible amounts (i.e. one or two correct tables).[8] These amounts were actually the ones which were most likely to be chosen by agents. As is shown in Figure 2, the large majority (68.2%) of influence activities involved exaggerating one's own production by two tables or less. Agents who exaggerated their

[8] We refer to the smallest integer number of tables. In our setting, agents could also exaggerate their production by even less than one table.

production by two tables or less boosted their production by 26.6% on average. By contrast, agents who exaggerated their production by at three tables or more boosted their production by 131.3% on average.

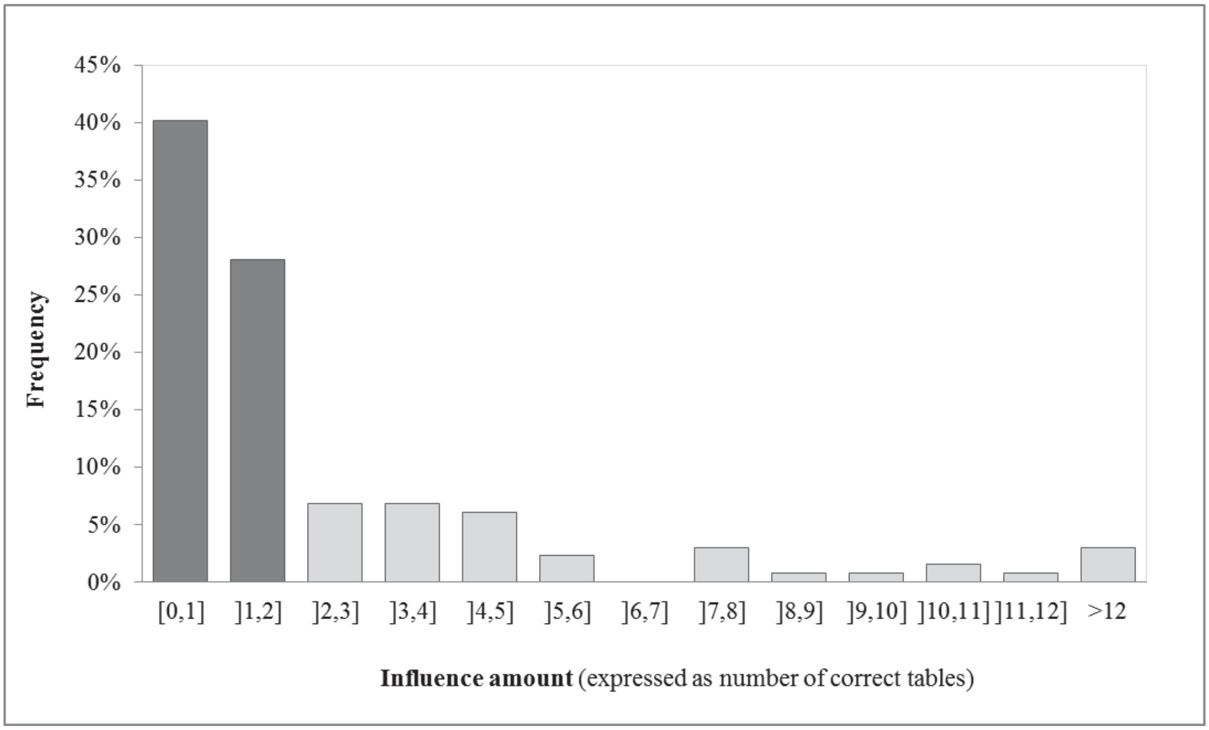

FIGURE 2.- Histogram for the amount (expressed as the number of tables) by which an agent decided to exaggerate his or her *observed* production in a given period.

The fact that most agents decided to exaggerate their production by the smallest possible amount is in line with the deception literature according to which people are willing to avoid major lies (e.g. Shalvi, Handgraaf and De Dreu, 2011).

In Table 4, we analyze the effect of influence activities on agent pay. We include as independent variable the dummy variable ("Two tables or less" *influence dummy*) that aims at capturing *undetectable* influence activities. This variable takes value one whenever an agent exaggerated his or her production in a given period by a total amount either equal to 30¢, 60¢, 90¢ or 120¢ (that is two tables or less, see column [1] in Table 4). In columns [2] and [3], we also include in our regression analysis a dummy variable that takes value one whenever an agent exaggerated his or her production by three tables or more. Finally, we control for the actual contribution of the agent as it is supposedly a main driver of agent pay even when influence activities are available. We also control for agents' ability on the task (summation skills).

We report a positive and significant effect of *undetectable* influence activities on agent pay (see "Two tables or less" *influence dummy* in column [1] of Table 4). By contrast, we do not observe such positive effect of influence activities on agent pay when considering only those influence activities by which agents exaggerated their production by three tables or more (see "Three tables or more" *influence dummy* in columns [2] and [3] in Table 4). In Appendix B, we provide additional analysis regarding the effect of influence activities on agent pay. These findings show the robustness of the positive effect of *undetectable* influence activities on agent pay.

Interestingly, summation skills appear to have no effect on agent pay, regardless of the regression. That is, in the presence of influence activities, ability on the task is not driving agent performance.

Our regression results suggest that an agent could increase his or her *discretionary pay* by engaging in influence activities as long as such activities were *undetectable*.

TABLE 4. Linear panel regressions assessing the effect of *undetectable* influence activities on *discretionary pay* for the treatment with influence activities and *discretionary pay*.

Dependent variable	*Discretionary pay* (%)		
	[1]	[2]	[3]
Intercept	21.883*** (2.724)	21.867*** (2.707)	22.987*** (2.862)
Period Trend	0.016 (0.042)	0.032 (0.059)	0.037 (0.051)
Actual contribution (%)	0.338*** (0.094)	0.337*** (0.095)	0.340*** (0.094)
"Two tables or less" Influence Dummy	**3.882** (1.700)**	**3.779** (1.768)**	-
"Three tables or more" Influence Dummy	-	-0.719 (2.346)	-1.618 (2.172)
Summation Skills	-0.070 (0.179)	-0.064 (0.167)	-0.042 (0.185)
Gender Dummy	-1.190 (2.139)	-1.244 (2.210)	-1.364 (2.128)
Observations (organizations)	n = 240 (16)	n = 240 (16)	n = 240 (16)
R^2	0.254	0.253	0.232

Estimation output using robust standard errors clustered at the organization level (in parentheses).
*p-value<.10, **p-value<.05, and ***p-value<.01.

Surprisingly, and in contrast with Hypothesis 1iii, a substantial proportion of agents (35.8%) also engaged in influence activities in a given period under *equal pay*. This proportion is however substantially lower than under *discretionary pay* (55.0%; p-value = 0.001, see Table 2). Under *equal pay*, influence activities led agents to reduce the proportion of time spent on the task (82.8% compared to 85.5% in the absence of influence activities), although this effect was not statistically significant (see *influence dummy* in the *equal pay* column in Table 3).

We thus found influence activities to be surprisingly pervasive under *equal pay*. This finding is especially surprising given that the influence activity which led to a 30-second frozen screen was not particularly salient and appealing to agents compared to a work task that yielded monetary rewards or compared to unlimited Internet browsing. Yet, a proportion of the influence activities we observed under *equal pay* could still have resulted from subjects' curiosity regarding the *boost* option (influence activities) or from demand effects. The fact that influence activities did not vanish overtime, however, seems inconsistent with subjects acting out of curiosity motives (see Appendix C, Table C.1). Also, the fact that the great majority of agents engaged in the same thoughtful and moderate influence activities (exaggerating their production by two tables or less) under *equal pay* than under *discretionary pay* seems inconsistent with influence activities being initiated for non-strategic motives such as demand effects (see Appendix C, Figure C.1).

This suggests that, in addition to the potential monetary gains associated to influencing the principal, agents may 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 even when they were 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 by showing that influence activities are indeed widespread. At the same time, it calls for extending previous models to contemplate the case in which people engage in manipulative attempts for status considerations in addition to pecuniary motives. We summarize our first finding as follows.

Result 1 *(Influence activities)*

i) Under discretionary pay, a large majority of agents engaged in influence activities spending less time working on the task. ii) Under discretionary pay, agents boosted their pay by engaging in undetectable influence activities. iii) Unexpectedly, a large majority of agents also engaged in influence activities under equal pay.

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 in the *discretionary pay* treatments. We define an agent's *observed contribution* as the ratio between his or her production as *observed* by the principal and the sum of *observed productions* of all three 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 exaggerated his or her production by engaging in influence activities. In Table 5, we report our regression findings for the whole sample (see last two columns of Table 5) 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 spent at least some time monitoring agents' production. The *influence* treatment did not significantly differ in the percentage of time principals allocated to watching agents (5.5%) compared to the *no influence* treatment (4.0%) (Proportion test, p-value = 0.842).[9]

The results reported in Table 5 are in line with Hypothesis 2i. The relationship between agent pay and *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 coefficients associated to the interaction terms between an agent's *observed* contribution and the *influence dummy* are negative and largely significant. The significance of these coefficients is only

[9] Using the same panel regression as in Table 2 with the amount of time (in seconds) principals spent watching agents in a given period, the p-value on the *influence dummy* coefficient was 0.125.

marginal when including in the analysis principals who did not watch the agent (see last two columns of Table 5). 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 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 = 0.253).

TABLE 5. Linear panel regressions for *discretionary pay* as a function of agents' contributions for the treatments with and without influence activities

Dependent variable	*Discretionary pay (%)*			
	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**$^{\lozenge}$ **(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)**
Gender Dummy	-0.832 (1.509)	-0.454 (1.176)	-0.248 (1.331)	-0.575 (1.116)
Observations (organizations)	n = 366 (29)	n = 366 (29)	n = 465 (31)	n = 465 (31)
R^2	0.526	0.502	0.394	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 $^{\lozenge}$*one-tailed* p-value<.10.

It is also interesting to observe that the interaction term between an agent's summation skills and the *influence dummy* is negative and significant for all regressions in Table 5. This finding complements Hypothesis 2 by showing that influence activities not only weaken the relationship between effort and pay but 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. We summarize our findings below.

Result 2 *(Low-powered incentives)*

i) Principals relied less on observed performance to set agents' pay in the influence treatment than in the baseline in which influence activities were not available. ii) As a result, the relationship between agents' output and pay was significantly weaker in the influence treatment than in the baseline.

4.3. Influence costs (Hypothesis 3)

We find support for Hypothesis 3 by showing that average organizational production (per period) was on average 12.7% lower in the *influence* treatment ($16.25) compared to the *influence* treatment ($18.62). We show that this difference is statistically significant in Table 6 (see *Influence dummy* coefficient).[10]

TABLE 6. Linear panel regression with random effects for organization production (in cents) for the two *discretionary pay* treatments.

Dependent variable	Organization Production
Intercept	469.885 (367.264)
Period Trend	126.293*** (22.199)
Influence Dummy	**-245.236**** **(110.274)**
Ability (Average Summation Skills)	3.283*** (1.145)
Observations (organizations)	n = 155 (31)
R^2	0.187

Estimation output using robust standard errors clustered at the organization level (in parentheses).
*p-value<.10, **p-value<.05, and ***p-value<.01.

[10] Note that organization production increased 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, 2015a, 2015b).

We summarized our results as follows.

Result 3 *(Influence costs)*

In the discretionary pay treatments, the production of the organization was lower when influence activities were available than when they were not.

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

In line with Hypothesis 4, we find that *discretionary pay* fares better than *equal pay* when influence activities are present whereas the opposite is true wen influence activities are absent (see Figure 3). Organizational production was on average $2.28 lower under *discretionary pay* than under *equal pay* in the *no influence* treatments but it was $1.39 higher in the *influence* treatments.

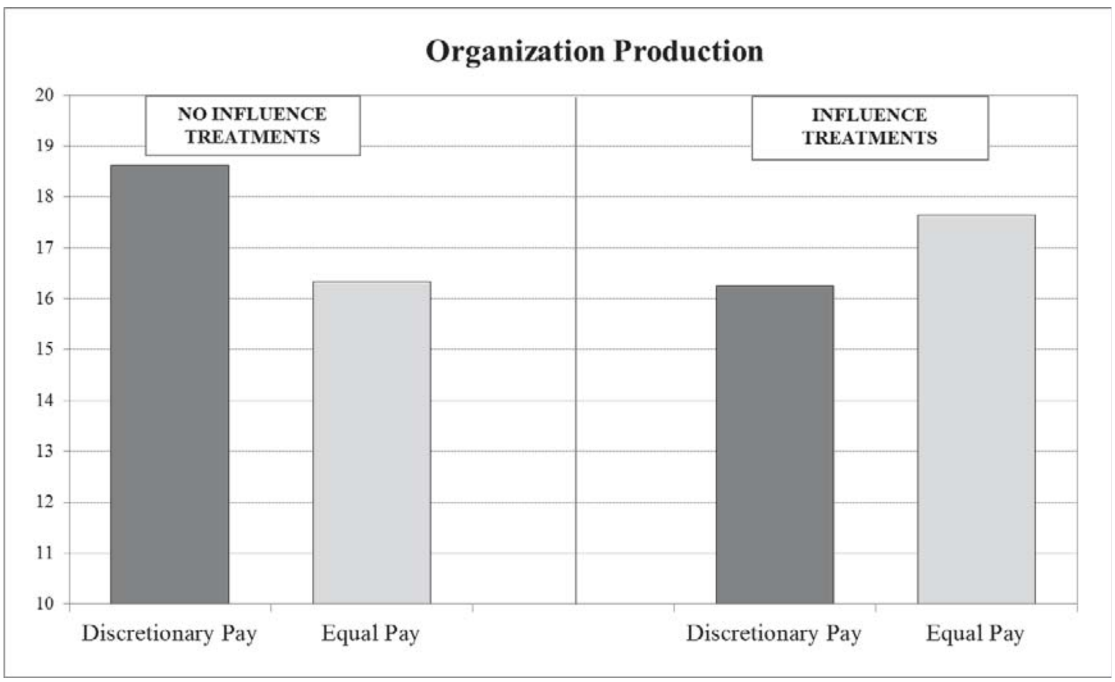

FIGURE 3.- Average organizational production across treatments.

In Table 7, we show that this difference is statistically significant because the interaction term between the *influence dummy* and the *discretionary pay dummy* (which takes value 1 if principals are in the *discretionary pay* treatment) is negative and significant (p-value = 0.032). Interestingly, *equal pay* even leads to a higher level of production ($17.64) than *discretionary pay* ($16.25) when influence activities are available. This difference is not significant, however (p-value = 0.194).[11] This may not be surprising given that our *discretionary pay* environment allowed principals to mimic *equal pay* by rewarding each agent an equal share of production (which they did one fourth of the times).[12]

TABLE 7. Linear panel regression with random effects for organizational production per period (in cents) across all treatments as a function of treatment dummies.

Dependent variable	Organization Production
Intercept	97.284 (252.165)
Period Trend	114.343*** (14.904)
Influence Dummy	64.563 (95.221)
Discretionary Pay Dummy	186.657* (106.974)
Disc. Pay Dummy × Inf. Dummy	**-311.651** (143.572)**
Ability (Average Summation Skills)	20.014*** (3.973)
Observations (organizations)	n = 310 (62)
R^2	0.288

Estimation output using robust standard errors clustered at the organization level (in parentheses).
*p-value<.10, **p-value<.05, and ***p-value<.01.

Our analysis thus confirms Hypothesis 4, showing that *equal pay* is a more effective payment scheme when influence activities are present than when they are not. More generally, our findings support the recommendation derived from the influence cost literature to give away managerial discretion to limit the negative consequences of influence activities. Result 4 summarizes our findings.

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

The production gap between the discretionary pay and the equal pay treatments was smaller when influence activities were available than when they were not. Moreover, the production of the organization was larger under equal pay than under discretionary pay when influence activities were available whereas the opposite was true when influence activities were not available.

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[11] 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.

[12] If one discards the observations (22.5% of the *discretionary pay* data) for which the principal chose to pay all agents the same share of organization output the p-value comes down to 0.11.

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 as they deter organizational members to engage in 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 provides no direct evidence of the pervasiveness of influence activities 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 both pervasive and detrimental to organizational performance. In the treatments with *discretionary pay*, the production of the organization was significantly lower when influence activities were available than when they were not. Unexpectedly, we found that employees also engaged in influence activities when rewarded *equal pay*. This finding which seems consistent with recent research showing that people value their relative standing in the organization suggests extending previous influence cost models to include status and non-monetary motives as possible determinants of influence activities.

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 employees' pay. Also in line with theoretical conjectures, *equal pay* led to higher organizational 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 the role influence costs play in substantially larger organizations. More generally, further research could build on our experimental setting to study the relationship between influence costs and firm size.

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7. APPENDIX

Appendix A. 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. ξ) and performance pay (w.p. $1 - \xi$).

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:

$$\frac{c}{y} \leq \frac{(1-\alpha)\rho}{n} \qquad [A1]$$

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

$$\xi \frac{n\rho+\rho_f}{n} + (1-\xi)((1+\frac{1}{n})\rho_f + \left(1-\rho_f\right)\rho) \leq \frac{n+1}{n}\rho \qquad [A2]$$

Where condition [A2] is relaxed as ξ increases. This implies that the largest set of parameters for which this condition holds is for equal pay ($\xi = 1$) in which case [A2] always holds. Given that [A1] does not depend on ξ, 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:

$$\frac{c}{y} \leq \frac{(1-\alpha)\rho_f}{n} \qquad [A3]$$

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

$$\xi\,\frac{2\rho+(n-1)\rho_f}{n} + (1-\xi)\,\frac{\rho^2+\rho+(n-1)\rho_f}{n} \leq \frac{n\rho_f+\rho}{n} \qquad [A4]$$

Where condition [A4] is relaxed as ξ decreases. This implies that the largest set of parameters for which this condition holds is for performance pay ($\xi = 0$). For $\xi = 0$, [A4] holds whenever $\rho^2 \leq \rho_f$. Given that [A3] does not depend on ξ, we can conclude that performance pay is the influence contract that implements the efficient equilibrium for the largest set of parameters.

Appendix B. Detectable/Undetectable influence activities

We provide additional robustness checks regarding the positive effect of *detectable* influence activities on agent pay. We define as *detectable* influence activities those for which:

[1] Agents boosted their production by an amount which was not a multiple of 30.

[2] Agents boosted their production by an *excessive* amount.

In particular, we define as *excessive* those influence activities that led agents' production (as *observed* by the principal) to be in the top 35% (25%) [20%] {10%} of agents' production levels in the *discretionary pay* treatment without influence: 540¢ (600¢) [660¢] {720¢}. These thresholds imply that the respective proportions of *undetectable* influence activities are 22.2%, 26.2%, 30.0% and 33.3%.

Our regression results (see Table B.1) are consistent with Table 4 showing that agents can increase their *discretionary pay* by engaging in *undetectable* influence activities.

TABLE B.1 Linear panel regressions assessing the effect of *undetectable* influence activities on *discretionary pay* for the treatment with influence activities and *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***	21.225***	21.704***	21.738**
	(2.999)	(3.015)	(2.938)	(2.921)
Period Trend	-0.027	-0.009	-0.001	-0.013
	(0.049)	(0.059)	(0.029)	(0.035)
Actual contribution (%)	0.351***	0.351***	0.344***	0.345***
	(0.094)	(0.094)	(0.094)	(0.095)
Undetectable Influence Dummy	**4.294****	**4.299****	**1.953**◊	**2.030**◊
	(2.195)	**(2.118)**	**(1.370)**	**1.345**
Summation Skills	-0.044	-0.049	-0.050	-0.054
	(0.185)	(0.178)	(0.185)	(0.181)
Gender Dummy	-1.114	-1.119	-1.169	-1.167
	(2.039)	(2.108)	(2.176)	(2.160)
Observations (organizations)	n = 240 (16)	n = 240 (16)	n = 240 (16)	n = 240 (16)
R^2	0.255	0.259	0.242	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.

Appendix C. Influence activities under *equal pay*

TABLE C.1. Linear panel regression with random effects for time spent by agents engaging in influence activities (in seconds) as a function of a Period Trend.

Dependent variable	*Time spent on influence activities (in seconds)*	
	Discretionary Pay	*Equal Pay*
Intercept	20.255 (13.301)	11.051 (9.678)
Period Trend	**2.911 (1.804)**	**2.039 (1.914)**
Internet Time (in seconds)	0.011 (0.011)	0.092** (0.046)
Summation Skills	-0.573 (0.858)	-0.542 (0.874)
Gender Dummy	13.608 (9.170)	18.223 (12.468)
Observations (organizations)	n = 240 (48)	n = 240 (48)
R^2	0.025	0.039

Estimation output using robust standard errors clustered at the organization level (in parentheses).
*p-value<.10, **p-value<.05, and ***p-value<.01.

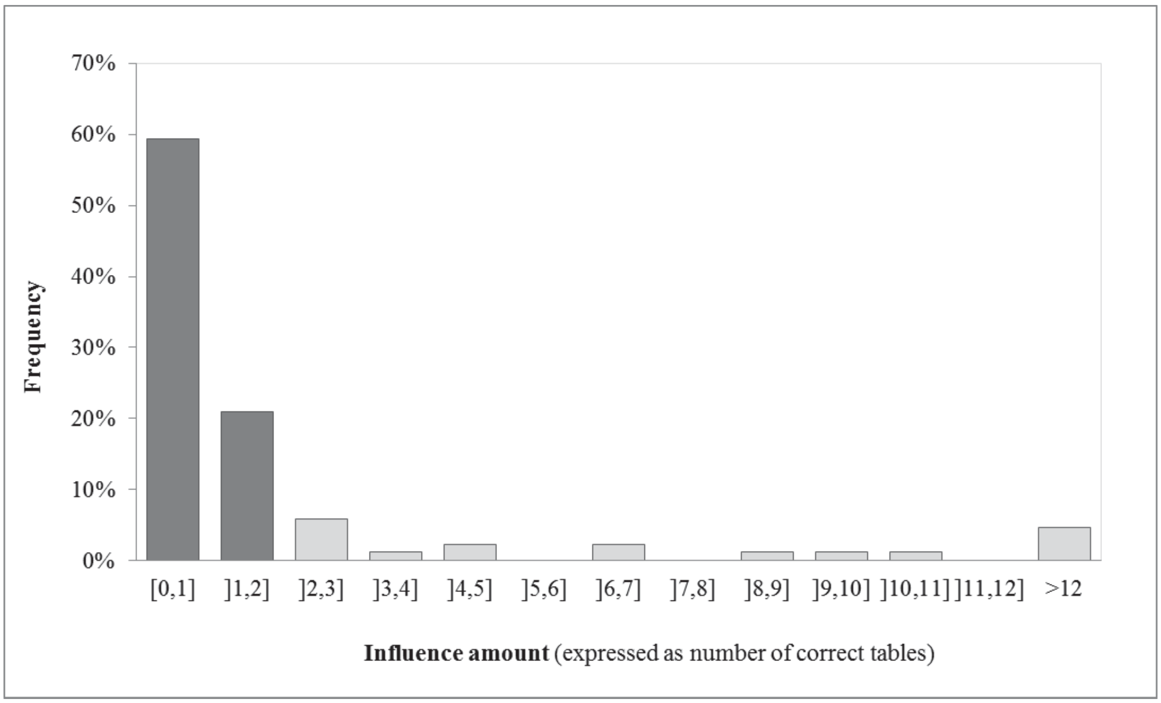

FIGURE C.1.- Histogram for the amount (expressed as the number of tables) by which an agent decided to exaggerate his or her *observed* production in a given period for the *equal pay* treatment.