Measuring the Output Reduction due to Incentive Ratcheting in the Field

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Abstract

Quantitative evidence from the field on the output loss due to incentive ratcheting is mixed. One possible explanation is that the output response to ratcheting varies with the level of ratcheting. This paper estimates how the output cost of ratcheting varies with the level of ratcheting using rich data from the restructuring of the salesforce of a large Chilean producer and distributor of beverages. We show that in response to reducing ratcheting by one standard deviation, the salespersons at this firm increase sales by an average of 19%. The output response is non-linear in ratcheting: Output reduction is greatest when workers move from low or no ratcheting to some ratcheting. Also, the response is immediate after changing incentive ratcheting and does not vary with the educational attainment of workers, showing that experienced workers in the firm exhibit a high degree of sophistication. We execute additional analysis to verify the causal nature of our result.

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1   Introduction

The use of formal incentives, or pay-for-performance, to motivate workers' effort in situations of moral hazard is a long-standing research area in economics (Prendergast, 1999; Gibbons and Roberts, 2012; Lazear, 2018). In this paper, we study the problem of "incentive ratcheting" that arises when incentive systems need updates over time. Incentive ratcheting can be defined as the practice of setting next period goals, piece rates or any other key incentive parameters based on current-period and past performance. From a static perspective this might be helpful in reducing asymmetric information regarding employees' productivity (Prendergast, 1999; Weitzman, 1980). However, from a dynamic perspective it gives the agents incentives to strategically restrict their output in order to prevent being subjected to higher standards in the future (Milgrom & Roberts 1992, Laffont and Tirole, 1988; Gibbons, 1987).

The theoretical prediction of output restriction is consistent with several long-lived business cases that describe strategic output restriction in response to incentive ratcheting. Using ethnography, Mathewson (1931) concludes that the main reason for output restriction in firms is the ratchet effect, as explained by a worker quoted in his book: "I have learned through sad experience that the more your superiors find they can get out of you the more they come to expect. The only way to protect yourself is never to work at anything like full capacity. I know that most restriction is due to the worker's desire to save and protect herself and not to any other motive" (p. 65). Other examples are found in Roy (1952), Lawler (1971), Edwards (1979), Montgomery (1979), Clawson (1980), and the famous Lincoln Case as discussed in Carmichael and McLeod (2000).

Despite the numerous accounts of the relevance of incentive ratcheting in firms, the empirical evidence in the prior literature on the output cost of ratcheting is mixed. The reason is that quantifying the total output reduction due to incentive ratcheting is a difficult task that more often than not depends on the context. As Charness et al. (2011) explain, the existence and magnitude of the ratchet effects depends on the existence and on the size of quasi-rents to both employer and employee, which in turn requires low competitiveness in the related

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markets. Furthermore, as Abeler, Huffman and Reymonds (2024) show, the complexity of dynamic incentives, such as ratcheting, imposes considerable strain on the cognitive skills of workers. In such cases, whether workers have the time to learn about how their firm operates impacts the effects of incentives. Moreover, credibly convincing a group of workers of a change in the level of ratcheting to which they will be subjected in the future is difficult in a real-world setting because it requires a strong and permanent commitment from the firm. Most importantly, the strategic response may be different when a worker moves from no ratcheting to some ratcheting and when the same worker moves from moderate to high ratcheting.

This paper estimate how the impact of incentive ratcheting on output varies with ratcheting by using a quasi-natural experiment that resulted from the re-structuring of the salesforce in a large Chilean beverages company. The restructuring consisted of the merging of all sales units into the already existent centralized sales division of the company (we refer to this event as centralization in the rest of the paper). Both before and after the restructuring, large part of the compensation of all salespersons is linked to the achievement of their sales goals. Before the centralization, in approximately half of the sales units, supervisors determined independently the goals of the salespersons whom they supervised. Thus, supervisors could ratchet up goals over time, i.e. increase them if and when past sales surpass the goals. After the centralization, these salesforces adopted the goal setting process that was in place at the centralized sales divisions: an automatic, uniform and publicly known formula to set goals for all.

We first measure the supervisors' pre-centralization degree of goal ratcheting, and document extensive variation, which is crucial for our identification strategy. With the restructuring, the salespersons in the affected sales units transition from heterogeneous, supervisor-specific goal ratcheting to uniform goal ratcheting based on a formula. Consequently, for some salespersons the level of ratcheting goes down, while for others it goes up. This allows us to identify how the effect of ratcheting on output varies with the level of ratcheting using a difference-in-difference regression analysis: The first difference is over time, before and after the restructuring, while the second is between different pre-centralization levels of supervisor

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ratcheting, which is a continuous variable. Crucially, the switch to automatic and uniform goal setting is perceived as permanent by the salespersons, since the new regime is established in the labor contracts, which are the same contracts that have been used in the centralized sales division for a long time.

Our results show that, in the context of the studied firm, in response to the reduction of supervisor ratcheting by one standard deviation sales increase by 19% on average. The effect of eliminating supervisor ratcheting varies considerably across salespersons due to large differences in the ratcheting that their supervisors apply. According to our most general non-linear specification, the effect of moving from no ratcheting to some ratcheting is double the effect of moving from average to high levels of ratcheting. Our specifications show that the complete elimination of incentive ratcheting in this firm would lead to an increase in total sales by approximately 34-39%, depending on the specification. In practice, a complete elimination of ratcheting is impossible, as it would require a credible commitment to never use past information to update current goals. We therefore interpret this figure as an upper bound on the possible benefits of reducing incentive ratcheting.

We also find that sales adjust immediately to changes in goal-setting when they are introduced. Our estimates reveal that salespersons are well-informed and sophisticated: There is no evidence that, controlling for current ratcheting, lagged ratcheting or past average ratcheting affect salespersons when choosing effort. In addition, we find no evidence that the output response to incentive ratcheting varies with the educational attainment of salespersons. These results are robust not only to changes in the way we measure output but also to changes in the specification, the measurement of ratcheting, and the source of ratcheting variation that we use. Moreover, they are driven neither by selective attrition nor selective assignment of salespersons to supervisors. Furthermore, we also exploit the fact that there are other sales units that are part of the centralized salesforce division throughout all of our observation period, and we perform a falsification test on that salesforce. Finally, we perform some robustness checks that rule out the existence of confounding effects associated with other changes that occurred with the centralization: We estimate a triple difference specification and control for

the potential heterogeneous impact on output of these changes using a rich survey-based set of control variables.

This paper is structured as follows. Section 2 discusses the empirical literature on output reduction due to incentive ratcheting. Section 3 introduces the institutional setting and offers evidence of ratcheting-driven effort withdrawal in our setting. Section 4 describes the data and presents our estimates of supervisor ratcheting. Section 5 summarizes our theoretical framework. Section 6 presents the empirical analysis: our empirical strategy, benchmark results, various robustness tests of the benchmark results, and our estimates of the non-linear output cost of ratcheting. Section 7 discusses the implications of the results and concludes.

2   Prior Literature

Several lab experiments have documented the reduction in output as a result of incentive ratcheting. Cardella and Depew (2018) show that, when output is measured individually, agents respond to incentive ratcheting by restricting output; while if output is measured collectively, communication among group members is needed to observe output restriction. Using a pool of Chinese students and firm managers, Cooper et al. (1999) also find strong evidence in favor of output restriction due to ratcheting behavior. Charness et al. (2011) find evidence supporting output restriction due to incentive ratcheting, but only if there is low competition in the market.

There is much non-experimental field evidence that in many firms, goals tend to increase with past performance (Indjejikian et al. 2014; Anderson et al., 2010).[1] However, the estimates of the effect of ratcheting on output in the field are mostly correlational (Bol and Gil, 2015; Anderson et al, 2010). An exception is Bellemare and Shearer (2014) and Shearer (2022) who study tree-planting workers and experimentally modify the expectation of the piece rates that workers receive. They show that workers reduce effort around 20% when they

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[1]However, see Allen and Lueck (1999) for evidence to the contrary: They studied agricultural contracts but failed to provide evidence of ratcheting. High competition in this setting could be responsible for the lack of evidence in this setting (Charness et al, 2011).

were told i) that the rate would be revised upwards if, after a few days, average productivity was lower than a benchmark, or ii) that the rate, which was set above the regular level, would be revised downwards if after a few days earnings were 'too high.' These studies manipulate expectations of future incentive ratcheting and measure how output changes as a result. They highlight the importance of credible commitment which is hard to achieve in an experimental setting. Brahm and Poblete (2018) use a field experiment to show that incentive ratcheting has a significant effect on how workers assign their effort among different tasks. Their experimental design, however, does not allow them to estimate the total impact of incentive ratcheting on output because they measure only the output change in one task relative to the other. Moreover, their experiment does not introduce a permanent change in incentives, so they do not control for the workers' expectations about ratcheting.

Macartney (2016) study the possibility of strategic behavior at the school level when secondary schools receive a bonus if their students meet a specific growth target that is conditional on prior average student score. Using a triple difference approach, they find that when using these schemes there is a drop in children's performance, which is consistent with teachers reducing effort strategically. In their paper, the individual contribution of teachers on student output is not observable, so they measure the average effect of the policy on student performance on the school level. Our approach allows us to measure individual performance and explore non-linearities in the response to ratcheting.

The most recent and closely related paper to ours is Abeler, Huffman and Raymond (2024). Our setting differs from that in Abeler et al (2024) in several ways with substantive consequences for the results. First, unlike the newly hired workers in Abeler at al (2024), the workers in our setting have many years to learn about ratcheting, so not surprisingly the estimated effect does not vary with tertiary education, our proxy for cognitive ability. Second, in our setting incentive ratcheting changes not temporarily but permanently and in a credible way, as the labor contracts are modified to reflect the changes. Moreover, while in their settting every worker was facing the same change in ratcheting policy, in our setting variaton in supervisor ratcheting allows us to estimate non-linear effects of ratcheting.

It is also interesting to notice that, unlike in the setting of Abeler et al (2024), we consider a firm that face low labor market competition (the main producer and distributor of beverages in Chile). As Charness et al (2011) predict, this environment is conducive to substantial ratchet effects, and consistent with these predictions we find a significant and nonlinear output cost of ratcheting.

3 Institutional Setting

We work with data from CCU, the largest Chilean producer and distributor of beverages. The company sells four categories of product: beer, soft drinks, spirits and wine. These have the following volume shares, respectively (sales revenue shares are shown in parentheses): 34% (42%), 60% (42%), 4% (10%), and 2% (6%).

CCU has a "mixed" organizational structure. There are four product divisions (or strategic business units, SBUs) that control and decide on product marketing, production and part of sales in a decentralized way. These divisions have their own CEOs and top management team (TMT) and produce their own financial statements. There are two strategic services units (SSUs) that provide logistics and sales services in a centralized manner to the SBUs. Both SSUs also have their own CEOs, TMT and financial statements.[2] With respect to logistics, the SBUs' product is sent to the distribution centers managed by the Logistics SSU, which then executes the delivery to customers. With regard to sales, before August 2016 the Sales SSU provided sales services using a multiproduct salesforce in non-metropolitan and less densely populated areas, roughly covering half of CCU's total sales. In contrast, in metropolitan areas with big population agglomerations (i.e., > 500K inhabitants), each SBU had its own decentralized salesforce, which operated independently of the Sales SSU.

This changed in August 2016, as from that month onwards all the beer and soft drink salesforces were absorbed by the sales SSU and, thus, sales became fully centralized in the company, with the Sales SSU now operating across the entire country. This change occurred

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[2]Other administrative units such as finance and human resources are centralized at the level of the CCU's corporate headquarters, but they are just cost centers without their own financial statements.

without affecting the total number of CCU salespeople: The salesforce of the company consists of approximately 600-630 salespersons and 120 sales supervisors before and after the restructuring. As we describe in detail in section 3.1 below, this restructuring meant that the salesforces that were absorbed by the centralized Sales SSU experience a change in the way their sales goals are set and thus how incentive ratcheting operates.

The compensation policy of salespeople is the same across sales units both before and after August 2016. This policy is as follows: Approximately 65% of the compensation of salespersons is contingent on performance, and 80% of this is driven by goal achievement, defined as sales to goals. This ratio of sales to goals is translated into a sales commission using a sigmoidal function, which starts paying at (approximately) 75% of goal achievement and is capped at 125% of goal achievement. Moreover, the payments for achieving the goals remain the same across units both before and after the centralization.

The daily activities of salespersons, across all salesforces, are as follows. Every morning there is a meeting of approximately one hour where the sales supervisors meet with their group of salespersons (on average seven) to discuss sales progress on an individual basis, to communicate company-wide events or policies (e.g., special promotions, new products) and to allow sharing of practices/tips/advice between salespersons. During the remainder of the day salespersons go out and visit their customers (around 30 plus customers). Half of their time is devoted to commuting between customers and the other half to selling activities at each customer site (e.g., preparing the visit, checking the customer's stock, discussing/negotiating the sales order, generating the digital sales order, etc.).

3.1 Goal Setting

The heterogeneity in goal setting and the changes experienced within CCU are the central elements we exploit in this paper. First, we explain the goal-setting system in the Sales SSU, and then we explain the goal setting in use by the SBU salesforce before centralization occurred in August 2016.

Since 2014, the Sales SSU has had a goal-setting process based on a pre-established uniform formula. A team of analysts in the Sales SSU headquarters generates a goal for each product category j for each salesperson i for the month t using the following equation:

$$Goal_{ijt} = 0.25 \; Sales_{ijt-1} + 0.15 \; Sales_{ijt-2} + 0.10 \; Sales_{ijt-3} + 0.50 \; Sales_{ijt-12} \qquad (1)$$

This calculation of goals has incentive ratcheting built into it: Higher sales today lead to higher goals in the future. Before 2014 the goal-setting process in the Sales SSU did not use a formula, but rather sales supervisors had complete freedom to set the goals of their assigned salespersons.

Before August 2016, the SBUs have the same process as the Sales SSU before 2014, namely the goals of salespersons are set at the discretion of the supervisors. As we show below in section 4.2, there is plenty of heterogeneity in the extent to which individual supervisors executed goal ratcheting before August 2016 in these SBUs. After August 2016, when the SBU salesforce is centralized, their goals become determined by the same formula-driven process as in the rest of the Sales SSU. Thus, these salespersons switch from having heterogeneous ratcheting, which depends on the supervisor, to homogenous ratcheting driven by the formula discussed above. This means that some salespersons experience an increase in ratcheting (because their SBU supervisors do little ratcheting before August 2016), while others experience a decrease in ratcheting (because their SBU supervisors do a lot of ratcheting before August 2016).

3.2 Evidence of Effort Withdrawal in Response to Ratcheting at CCU

Before estimating the impact on output of effort withholding, in this section we establish its presence in our setting. We present three pieces of evidence for ratcheting-driven effort withholding by CCU salespersons.

First, Brahm and Poblete (2018) conduct a field experiment in CCU in 2013 in which they manipulate goals across two product categories, creating a significant exogenous goal

increase in one and a significant exogenous goal decrease in the other. They document that the ratcheting that supervisors exert varies, and that salespersons whose supervisors do not perform goal ratcheting increase (decrease) sales in the category for which the return to effort increases (decreases) due to the goal decrease (increase). However, this result reverts completely for salespersons whose supervisor execute high levels of ratcheting: salespersons decrease sales in the category with a goal decrease and increase it in the category with a goal increase. A formal model and several tests show that these results are consistent with effort withholding in response to ratcheting.

Second, we are granted access to a company document from 2017 related to firm-union negotiations, in which the leaders of a union representing a large proportion of CCU salespersons reveals that the goal-setting practices of the company introduce perverse incentives that act as a "brake on sales". The document shows that CCU employees are reluctant to outperform their current sales goals, since high performance translates into more demanding future goals. In the words of an anonymous salesperson quoted in the report, 'I just meet the goals...' We consider this a "smoking gun" regarding the consequence of ratcheting on salespersons' behavior.[3]

Third, we estimate a regression that investigates how daily sales depend on the proximity to reaching or outperforming the monthly goals (see Appendix A.1 for details). In this analysis, we use daily sales data by product category from the period March 2017 – June 2019 (i.e. after the centralization), which contains 507,152 observations. In this period all salespersons are subject to homogeneous sales ratcheting determined by equation (1). We regress logged individual daily sales on dummies that capture the degree of goal achievement until the previous day, controlling for a large array of fixed effects. We find evidence consistent with the presence of ratcheting. As cumulative monthly sales approach the sales goal of the month, sales slowdown: If the ratio of cumulative monthly sales to the sales goal of the month is between 80 and 90 at the start of the day, sales are 8.3% lower in the current day relative to daily sales when the ratio is between 0 and 80. Similarly, when the ratio is between 90 and 100,

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[3]For confidentiality reasons we cannot provide direct evidence or share this document (but verification possibilities are available upon request).

daily sales decrease 11.8%. The trend becomes more pronounced once the goal is attained: if the ratio is between 100 and 110, the reduction in daily sales is 18.1%; if the ratio is between 110 and 120, the reduction grows to 23.4%; and if the ratio is more than 120, the reduction reaches 27.0%. The large array of fixed effects (FEs) — day of the month FEs, month FEs, and salesperson times product-category FEs—allows us to conclude that this finding captures a behavioral response and not some "natural" decay in sales (due to, for example, sales being lower on Mondays or higher/lower on a particular day of the month). This behavioral response is consistent with effort withholding in response to incentive ratcheting.

4   Data and Ratcheting Estimates

This section presents the data and their main features. Section 4.1 reviews the salesperson-month database used in our main empirical analysis and presents some basic summary statistics that reveal how sales vary with ratcheting before and after the centralization event. Section 4.2 then reviews how we estimate supervisor ratcheting, which is used in our subsequent regression analysis.

4.1   Data

We use data from CCU's SBU salesforce that got centralized into the Sales SSU for the period January 2015 and July 2017. The data from the first year, January-December 2015, are used to estimate the ratcheting measure discussed in section 4.2. The data for the period January 2016 to July 2017 are used in our main empirical analysis. In each month, we observe approximately 55 supervisors and the 320 salespersons in their teams. For some analysis (falsification test in section 6.3.3 and triple difference analysis in section 6.3.4), we also use the data from the salesforces in the Sales SSU, which are equivalent in size to the salesforces that got centralized.

To perform our analysis, we exclude temporary salespersons or newly-hired salespersons who are still in their trial period. As a result, for the period January 2016-July 2017, the

database includes 8105 observations for which we have detailed employment history, a full set of demographic characteristics, and time-varying controls. For the salespersons and their supervisors, we observe their age, gender, educational attainment, unionization status, start and end dates in the company (and therefore tenure), residence and work locations, type of employment contract, civil status, and monthly sales.

In 90 percent of the cases, each supervisor is responsible for a team of 5 to 11 salespersons every month, with the median being 7. Throughout the period January 2016-July 2017, the average monthly probability that a salesperson changes supervisors is just below 5%, which translates into an annual reassignment rate of approximately 45%. The only exception is in August 2016 when close to 50% of salespersons in the SBU salesforce change supervisors as they are centralized into the Sales SSU. Finally, we note that the salespersons and supervisors on indefinite contracts, whose behavior we study, have a very low monthly attrition of just below 0.05%.

Table 1 reports the total sales per salesperson of the SBU salesforce before and after August 2016. It presents both the raw total sales data, and sales adjusted for seasonal and location-specific variation in the data. Without controlling for seasonal and location effects, the average total sales in the SBU salesforce increase from 74.7 mln Chilean pesos (CLP) under supervisor ratcheting until August 2016 to 81.9 mln CLP under automatic ratcheting after August 2016, which amounts to an increase of 9.6%. Furthermore, the total sales are characterized by very high variance both before and after August 2016.

To adjust for seasonal and location effects, we perform a fixed effects regression of sales on location and monthly dummies. We define the adjusted sales as the difference between the observed sales and the estimated location and monthly dummies. The adjusted average total sales for the SBU salesforce increase by 20%, from 70.5 mln CLP under supervisor ratcheting to 84.1 mln CLP under objective uniform ratcheting. The adjustment for seasonal and location effects also significantly reduces the variation in sales. Finally, the last two columns of Table 1 show that before and after the organizational change, the number of supervisors and salespersons remains stable.

4.2 Estimates of Supervisor Ratcheting

To measure supervisor ratcheting we follow the methodology used by Brahm and Poblete (2018). Using the monthly data for the period January-December 2015, we estimate a panel data regression of the growth in sales goals on the ratio of past monthly sales to past monthly goals and time dummies:

$$\frac{Goal_{it}}{Goal_{it-1}} = \rho_0 + \sum_{j=1}^{J} R_j \frac{Sales_{it-1}}{Goal_{it-1}} Supervisor_j + \varphi_j + \gamma_i + \delta_t + \varepsilon_{it} \tag{2}$$

where φ_j, γ_i, δ_t are supervisor, salesperson, and time effects, respectively, and R_j is the supervisor-specific coefficient that represents the supervisor's subjective ratcheting. The supervisor-specific coefficient R_j and effect φ_j apply to all salespersons under their supervision in month t. We use R_j as our primary measure of supervisor ratcheting.

Figure 1, Panel 1.1 presents the point estimates of supervisor ratcheting, along with the associated 95% confidence intervals. It shows that most of the supervisors apply some form of subjective ratcheting that is statistically higher than zero: Outperforming past goals translates into an increase in future goals. The density distribution of supervisor ratcheting is presented in Panel 1.2. The average supervisor ratcheting is 0.62 and the standard deviation is 0.34, while 90% of the estimated levels of ratcheting lie in the range 0.11 to 1. Figure 1, Panel 1.2 also show the quartile thresholds for the distribution of supervisor ratcheting: 0.37, 0.59, and 0.85 for the 25th, the 50th, and the 75 percentiles, respectively.

5 Theoretical Framework

Our setting can be represented in a simple model that follows Weitzman (1980). Each period, salespeople need to decide how much effort to dedicate to selling activities, s, at a quadratic cost.[4] Each period salespeople receive a commission that is proportional (based on parameter α) to the difference between sales (s) and the goal (g) in the current period. Therefore, each

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[4]The quadratic assumption is not necessary but simplifies the discussion.

period the agent solves

$$\max_{s_t} \sum_{t=1}^{\infty} \delta^{t-1} \left[\alpha (s_t - g_t) - \frac{s_t^2}{2} - \beta_i s_t \right] \tag{3}$$

The parameter β_i represents each person's leisure preferences, or client characteristics that might be different for each salesperson. The parameter δ is a discount factor.

Incentive ratcheting implies that goals in period t are set as a function of sales in period $t-1$ (among other variables). Let ρ be the partial derivative of the goal in period t (g_t) with respect to sales in the previous period (s_{t-1}). The parameter ρ represents the ratcheting parameter R in the estimation of equation (2). The optimal effort level in t in the agent's problem is then given by the first order condition

$$\alpha (1 - \delta\rho) - \beta_i = s_t \tag{4}$$

This implies that, all other things being equal, sales are expected to be decreasing in the level of ratcheting ρ.

To explain the main test that we perform in this paper, consider two salespersons who before the policy change in August 2016 faced different ratcheting levels ρ_i and ρ_j, with sales of $s_i(\rho_i)$ and $s_j(\rho_j)$. After the policy change, both are subject to the same level of expected ratcheting, $\bar{\rho}$, with sales $s_i(\bar{\rho})$ and $s_j(\bar{\rho})$. Note that the difference in sales before and after the policy change for each agent is:

$$s_i(\bar{\rho}) - s_i(\rho_i) = \alpha\delta(\rho_i - \bar{\rho})$$
$$s_j(\bar{\rho}) - s_j(\rho_j) = \alpha\delta(\rho_j - \bar{\rho})$$

Thus, the theory predicts that, if $\rho_i > \rho_j$, then

$$\delta(\rho_i - \bar{\rho}) > \delta(\rho_j - \bar{\rho})$$

and we should observe that the change in sales for the person who faced more ratcheting

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should be larger than the change for the salesperson who faced less ratcheting:

$$s_i\left(\overline{\rho}\right) - s_i\left(\rho_i\right) > s_j\left(\overline{\rho}\right) - s_j\left(\rho_j\right)$$

6 Empirical Analysis

This section is organized as follows. We start by laying out our empirical strategy. Then we estimate our difference-in-difference model for the salesforce of the SBUs, which we consider as our benchmark model. Then, we introduce additional analysis to test the robustness of the benchmark model, including tests that show that the results are not driven by the selective assignment and reassignment of salespeople to supervisors. As a falsification test, we re-estimate the benchmark model, but for the salesforce that did not undergo centralization in August 2016. Then, we test for confounding effects due to other changes that occurred at the time of the centralization event. Finally, we consider nonlinear specifications to evaluate how the incremental output cost varies with ratcheting.

6.1 Empirical Strategy

Our empirical strategy applies the difference-in-differences methodology using only the salesforce that used to be delegated to the SBUs and then is centralized. The data includes all the workers in the SBUs salesforces, who after the reorganization of the company in August 2016 switch from heterogeneous ratcheting by their supervisors to automatic and uniform goal setting that comes from equation (1). The differences that we exploit are over time, before and after August 2016, and across different levels of supervisor-specific ratcheting, where this second difference constitutes a continuous treatment.

The continuous treatment comes from the ratcheting that salespersons experience before August 2016. After August 2016, a salesperson reporting to a supervisor with high ratcheting before August 2016 experiences a decline in the expected ratcheting; and the higher the pre-August ratcheting, the greater the reduction in ratcheting expectation. Similarly, the

salespersons reporting to a supervisor with low ratcheting before August 2016 increase their expectation of ratcheting; and the lower the pre-August 2016 ratcheting level, the greater the increase in ratcheting expectation.

A numerical example conveys the intuition of our test. Consider two groups of salespersons in the SBUs. Group A is under the supervision of a manager with high ratcheting before the change and generates output before and after the centralization of 100 and 120, respectively. Group B is identical to A except that it is under the supervision of a manager with low ratcheting before the change, with corresponding output before and after the centralization of 100 and 110, respectively. Thus, moving from high supervisor ratcheting to automatic ratcheting leads to an output increase of 20, while moving from low supervisor ratcheting to automatic ratcheting leads to an output increase of 10. A difference-in-difference approach implies that moving from high to low ratcheting leads to an increase of 10 in output (and vice versa, when moving from low to high, leads to a decrease of 10 in output).

Formally, we estimate a difference-in-difference model to recover the causal response of switching from high to low ratcheting using panel data only for the SBU salesforce defined by the months of sale, t, and an identifier for the salespersons, i. Our primary outcome of interest is total sales generated by a salesperson, since it is the primary measure of output monitored by the CCU.[5] Let T_t be equal to 0 for the months prior to the introduction of centralization and 1 otherwise. Furthermore, let R_{ijt} be a measure of the ratcheting that salesperson i receives from the supervisor j that is assigned to her or him in period t. The regression model that we take to the data is:

$$s_{it} = \alpha_0 + \alpha_1 R_{ijt} + \alpha_2 R_{ijt} T_t + X_{it}\alpha + \gamma_i + \lambda_t + \varepsilon_{it} \qquad (5)$$

where s_{it} is log sales, λ_t is period fixed effects, γ_i is a salesperson fixed effect, and the error term ε_{it} is assumed to be exogenous to the preceding regressors.

The difference-in-difference estimate of the effect of switching from ratcheting R to ratch-

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[5]For robustness, we will also consider alternatives to total sales that capture different dimensions of performance.

eting R' is:[6]

$$[E\left(s_{it}\,|T_t=1,R_{ijt}=R\right)-E\left(s_{it}\,|T_t=0,R_{ijt}=R\right)] \tag{6}$$
$$-\left[E\left(s_{it}\,|T_t=1,R_{ijt}=R'\right)-E\left(s_{it}\,|T_t=0,R_{ijt}=R'\right)\right]$$
$$=\alpha_2\left(R-R'\right)$$

To ensure the exogeneity of our ratcheting measures in our regression model (5), we follow the methodology of Chetty, Friedman, and Rockoff (2014) and estimate equation (2) using data from January to December 2015, while the data used for the estimation of (5) covers the period from January 2016 to July 2017. The identification of model (5) relies on the parallel trends assumption, as discussed in Angrist and Pischke (2008). The ratcheting R_{ijt} by supervisor j of salesperson i in month t is also assumed to be exogenous to the model parameters and to the error. Furthermore, we maintain that ratcheting exhibits sufficient variation to identify the effect of ratcheting on output over the whole of its admissible range $[0,1]$.

In our empirical setting, the theoretical model predicts that $\alpha_2>0$. If $R-R'>0$, the coefficient α_2 represents the improvement in output due to a decrease in supervisor ratcheting; if $R-R'<0$, the coefficient α_2 represents the decrease in output due to an increase in supervisor ratcheting.

The average increase in output due to the complete elimination of supervisor ratcheting is, therefore, $\alpha_2\overline{R}$, where $\overline{R}$ is the average level of supervisor ratcheting across salespeople in the SBUs. In the rest of the paper, we refer to this effect as the Average Effect of Supervisor Ratcheting (AESR). To measure how the effect of supervisor ratcheting on sales changes with the level of supervisor-specific ratcheting, we consider $\alpha_2\sigma$, where σ represents one standard deviation of the distribution of supervisor ratcheting; we call this the (average) Marginal

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[6]We obtain the formula for the difference-in-difference estimator of the effect of ratcheting by following Angrist and Pischke (2008). First, we define the conditional mean function $E\left(s_{it}\,|T_t,\,R_{ijt}\right)$. For any two distinct levels of ratcheting R and R', this function can take on at least four values. There are four coefficients of interest, so the model is (over)identified. We equate each of the empirically observed expected values to equation (5) and use the resulting system of equations to solve for the parameters in equation (5), which yields the difference-in-difference estimate in (6).

Effect of Supervisor Ratcheting (MESR).

Additional Source of Ratcheting Variation   There is another source of variation that can be used to recover the effect of ratcheting: the variation in output and ratcheting before centralization. As we mention above, around 3% to 7% of salespersons change supervisor in any given month. This variation, when occurring before 2016, provides valid estimates of the effect of ratcheting under the assumption that the salespersons across supervisors and across units are randomly assigned. It can be used to obtain alternative estimates of AESR and MESR that are comparable in magnitude but with a negative sign, and they are captured by $\alpha_1 \overline{R}$ and $\alpha_1 \sigma$, respectively, where α_1 is the coefficient of ratcheting. The theoretical model predicts $\alpha_1 < 0$. Table 2 summarizes our theoretical predictions.

6.2   Benchmark specifications

We consider three different specifications of the difference-in-differences model with linear continuous treatment. Specification (1) of the model is a regression of log sales on the variables of interest, dummies for location and month, and salesperson fixed effects. Specification (2) is a more general version that includes the following additional controls: salesperson tenure, supervisor tenure, unionization status, classification of employment contract, family status and dummies for residential location. In Specification (3), to check robustness, we introduce supervisor fixed effects, to control for supervisors' unobservable characteristics. The results, displayed in Table 3, are very similar across all specifications.[7]

Table 3 shows that the elimination of ratcheting has a positive and significant effect[8] on total sales by salespersons in the SBUs. In all models, the coefficient α_2 of the interaction of supervisor ratcheting with the indicator for the change in goal-setting after August 2016 is positive and statistically significant. We note that the coefficients are very similar across the specifications: 0.5 in Specification (1), 0.56 in Specification (2) and 0.52 in Specification (3).

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[7]As a preliminary step, we reject the hypothesis that the log-log or the levels specifications offer a better fit to the data. The results are reported in Appendix A.2.

[8]Throughout the paper we test whether the estimated coefficients are statistically different from zero and evaluate statistical significance at the 5% significance level.

Given the large differences in the ratcheting that supervisors apply, the effect of eliminating supervisor ratcheting varies considerably across salespersons. To characterize this heterogeneity, we consider the first and second moments of the distribution of causal responses to the elimination of supervisor ratcheting: the previously defined average (AESR) and marginal (MESR) effects of supervisor ratcheting, respectively. The average effect of eliminating supervisor ratcheting (AESR) is positive and statistically significant in all three specifications. The estimated AESR is 0.3 in Specification (1), 0.33 in our benchmark Specification (2), and 0.31 in Specification (3). This result implies that the complete elimination of supervisor ratcheting would lead, on average, to a 34%-39% increase in sales, depending on the specification. In practice, however, a complete elimination of ratcheting is difficult to implement because it requires a credible commitment to the unrealistic policy never to use employment history to set future goals. For this reason, we interpret the AESR estimate as an upper bound of the possible benefits of eliminating incentive ratcheting.[9]

Recall that MESR captures how the effect on sales varies between two salespeople subjected to different levels of ratcheting that are one standard deviation apart from each other. It offers a standardized unit measure of the heterogeneity in the output increase due to eliminating supervisor ratcheting. The MESR coefficient is positive, statistically significant and similar in magnitude across specifications: It is 0.16 in Specification (1), 0.18 in Specification (2), and 0.17 in Specification (3). In percentage terms, this result implies that someone who faces one standard deviation more ratcheting than (for example) the average salesperson increases sales by approximately 17-20% more than the average when the firm switches to a uniform ratcheting rule. Note that by design, this "marginal" effect of higher ratcheting on sales does not vary with the point of reference, whether it is the average salesperson or the salesperson who experiences similar levels of ratcheting before and after centralization. In section 6.3.4, we examine the validity of this feature of our model of linear continuous treatment.

With respect to the variation in sales with ratcheting before the centralization in August

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[9]Appendix A.3 shows that the results are not driven by influential observations.

2016, Table 3 shows that the effect of ratcheting before August 2016, represented by α_1, has the theoretically predicted negative effect on output in specifications (1) and (2), but the coefficients are not statistically significant. The point estimates are -0.25 in Specification (1) and -0.19 in Specification (2).[10]

Finally, specification (3) shows that including supervisor fixed effects does not affect the estimated effects of ratcheting, α_2. This result implies that, apart from the differences in supervisors' subjective propensity to ratchet, observed and unobserved heterogeneity across supervisors does not impact our estimates.[11]

6.3   Robustness Analysis of the Benchmark Specification

6.3.1   Ratcheting Heterogeneity and Selection

One key concern that needs to be addressed relates to the allocation and reallocation of salespersons to supervisors and their associated subjective ratcheting. The identification of the effect of ratcheting on output in our model relies on the assumption that the allocation and reallocation of supervisors to salespersons is unrelated to the performance of salespersons in terms of sales and its dynamics. To test that assumption, we adopt a variation of the classical test for non-random attrition by Nijman and Verbeek (1992). We include, in our benchmark specification (2) of Table 3, dummies for past, current, or future changes of supervisor. Under the null hypothesis, the reasons why a salesperson changes supervisors are not related to the salesperson's output and these dummies are, therefore, all zero. If one of the dummies is statistically different from zero, then the estimates suffer from selectivity bias, and they are not valid.

Formally, we include as a regressor the variable from the classical test for non-random attrition proposed by Nijman and Verbeek (1992): a dummy equal to one if the salesperson changes supervisors in the following month and zero otherwise. We allow the effect of this

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[10]As discussed in Section 6.1, we use a different source of variation to identify and estimate this parameter than our main parameters of interest. Since there is less variation in this source, we obtain less precise estimates.

[11]Appendix A.4 shows that we obtain very similar results when we use net sales or gross profit margin as the dependent variable.

dummy to be different for the periods before and after centralization. A negative coefficient of this dummy, for example, would indicate that the CCU reallocates salespeople to a new supervisor after a month of unexpectedly poor sales. In any event, a non-zero statistically significant coefficient of the dummy is evidence for non-random reassignment of salespersons to supervisors that is influenced by past performance. We also incorporate a dummy that is equal to one if the salesperson changes supervisors in the current month and zero otherwise. Again, we allow the effect of the dummy to be different before and after the centralization. Finally, we add to the list of explanatory variables an indicator that is equal to one if in the preceding month the salesperson changed supervisors, with the effect again possibly different for the periods before and after the centralization.

Table 4 presents the test results for non-random reassignment for each of our specifications in Table 3. The results show that in all specifications none of the test dummies is statistically significant neither before nor after the centralization event. The only exception is the dummy indicating that the salesperson changes supervisors in the following month in Specification (1) which is statistically significant for the period before the centralization. However, in Specifications (2) and (3) its coefficient ceases to be significant after the introduction of controls. Thus, we conclude that changes of supervisors are not driven by considerations related to sales. In technical terms, we find no evidence for selective reassignment of salespersons to supervisors.

Table 5 provides further evidence concerning the assignment of salespersons to supervisors. We consider two related regressions for each observable characteristic of the salespersons. In the first specification, we regress the characteristics on location and time dummies only; and in the second regression, we regress the same characteristics on supervisor fixed effects, supervisor tenure, and location and time dummies. The table reports the F-tests for the joint significance of supervisor tenure and fixed effects, along with the increase in the explained share of the variance due to the addition of supervisor fixed effects and supervisor tenure. Under the hypothesis that the company uses a specific policy to match salespersons of certain characteristics to supervisors of certain characteristics, it is possible to predict the

characteristics of a salesperson from the characteristics of the supervisor. Since most of the characteristics of the supervisors do not change during the period of study, in a panel setting they reduce to supervisor fixed effects. Table 5 shows that the supervisor fixed effects and tenure "explain" at most 12% of the variation in the characteristics of salespersons. Moreover, the most important variable that explains the ratcheting that salespersons face is their location.

In combination, Tables 4 and 5 reveal that the allocation and reallocation of salespersons to supervisors is largely driven by the geographical location and intertemporal shocks that are unrelated to the performance of salespersons.

6.3.2   Adjustment to Changes in Ratcheting

We performed several robustness checks to explore the validity of our main specification by testing for gradual adjustment to the change in goal setting, different effects of ratcheting based on educational attainment, and alternative behavioral responses to ratcheting.

Gradual Adjustment   First, we investigate whether the adjustment in effort and, therefore, sales takes place immediately or over several months before and after the official switch from supervisor to uniform ratcheting. One may well expect that the preparation for the change in the goal-setting policy takes place in the months immediately beforehand and that the management communicates some information to the salespeople in advance, in order to manage expectations. One might also expect that the workers do not immediately adjust their behavior to the new goal-setting policy due to behavioral, organizational, or communicational issues.

Table 6 presents the results from estimating the benchmark difference-in-differences model of Table 3 for total sales that include month-specific effects of ratcheting for the months just before and after the change in goal setting. Specification (1) incorporates such a month-specific effect for August 2016 that captures the adjustment and disruption to sales during the month of transition to automatic goal setting. The results show that its coefficient is close to zero

and not statistically significant. Specification (2) also includes additionally interactions of ratcheting with the dummies for the month before and the month after August 2016. These new variables also have coefficients close to zero and have no statistically significant effect on total sales. We obtain very similar results in Specification (3) where we incorporate into the baseline model interactions of ratcheting with monthly dummies for all the months in the period June 2016 – October 2016. Relative to Specification (2), the additional interactions are neither economically nor statistically significant. Most importantly, the inclusion of the new variables does not change the estimates of our main effects of interest.

These results show that we can rule out the hypothesis that total sales adjusted gradually before or after August 2016 They show that salespersons in the SBUs adjusted their behavior to the change in the goal-setting policy over the course of just one month, August 2016.

Average Past Ratcheting Second, we considered the hypothesis that the average ratcheting before August 2016, and not the ratcheting of the current supervisor, determines the gains from the elimination of supervisor ratcheting.

Table 7 presents the related results. Specification (1) replaces the ratcheting of the current supervisor with the average past ratcheting that a worker experiences before August 2016. The estimated average effect of eliminating supervisor ratcheting (AESR) is 0.27 while this coefficient in the benchmark specification in Table 3 is 0.33. With respect to the MESR coefficient, we obtain 0.15, while in the benchmark model it is 0.18. The regression does not include as a regressor ratcheting. The reason is that, since ratcheting varies only across salespersons but not over time, its effect on sales becomes absorbed by the salesperson fixed effects.

Relative to Specification (1), Specification (2) in Table 7 also investigates whether changing the supervisor at the same time as changing the goal setting policy has a distinct effect on the behavior of salespersons. The switch from supervisor to automatic ratcheting may be more credible, when the firm shuffles supervisors, so that salespersons and their supervisors

start under the new policy with a clean slate and without the burden of a common history. We interact a dummy for a change of supervisor in August 2016 with average past ratcheting and the indicator for the change in the goal setting policy. We add these new variables to the list of regressors in Specification (1). The results show that none of the new variables is statistically significant, rejecting the hypothesis that the reallocation of supervisors in August 2016 lends credibility to the switch from supervisor to automatic goal setting.

Finally, Specification (3) tests formally whether current or average past supervisor ratcheting is better at explaining the behavior of salespersons. To perform the test, we take the difference between average and current supervisor ratcheting and interact it with the indicator for the introduction of automatic goal setting. We then incorporate these interactions in the benchmark specification (2) of Table 3. The null hypothesis is that current supervisor ratcheting is sufficient to explain the behavior of salespersons and the coefficients of the difference between average past and current ratcheting is zero. The estimates in Table 7 confirm that this is indeed the case.

Lagged Past Ratcheting   Third, we explored whether the response to ratcheting depends disproportionately on the experience of the salesperson in the recent past. This form of myopia may have its roots in bounded rationality, rational inattention, or some form of behavioral bias.

Specification (1) in Table 8 estimates a version of the benchmark model where supervisor ratcheting is represented not by current ratcheting but by lagged ratcheting, i.e., the supervisor ratcheting in the preceding month. The hypothesis is that recent events have a greater impact on the expectations of salespersons and the best predictor of the near future is the recent past. The estimated effect of eliminating lagged ratcheting is statistically significant with a point estimate of 0.39. Specification (2) incorporates lagged ratcheting for each of the last three months, but none of the newly added variables are statistically significant.

Finally, Specification (3) tests formally whether current or lagged supervisor ratcheting is better at explaining the behavior of salespersons. To perform the test, we take the difference

between lagged and current supervisor ratcheting and interact it with the indicator for the introduction of automatic goal setting. We then incorporate this interaction in the benchmark specification (2) of Table 3. As discussed in the preceding section, the null hypothesis is that current supervisor ratcheting is sufficient to explain the behavior of salespersons and the coefficients of the interaction is zero. The estimates in Table 8 confirm that this is indeed the case.

Cognitive Skills and Ratcheting Fourth, we investigate whether the salespersons with no university education have a distinctly different response to incentive ratcheting than their presumably more sophisticated colleagues with at least some form of tertiary education. In this way, we explore whether people of different cognitive skills (approximated by educational attainment) have different behavioral response to complex incentive schemes.

To operationalize the hypothesis, we re-estimate our benchmark models reported in Table 3 but only for the subsample of salespersons who have at most a high school diploma. The results, presented in Table 9, show that for all model specifications the coefficients of eliminating supervisor ratcheting for the salespersons without any tertiary education are very similar to their counterparts reported in Table 3. The same applies for the estimated AESR and MESR: For example, in our most general model specification (3) the AESR for the salespersons without any tertiary education is 0.38 which is similar to the AESR for the whole sample, 0.31, while the MESR for the salespersons without any tertiary education is 0.21 which is similar to the MESR for the whole sample, 0.17. Thus, we conclude that the results reported in Table 3 are not driven by the behavioral response of only the more sophisticated salespersons with tertiary education. We attribute this result to the fact that, contrary to the setting in Abeler et al. (2024), in our setting salespeople have had years to learn about ratcheting incentives; The "smoking gun" quote in section 3.2 is further evidence that employees understand how ratcheting incentives work in the firm.

6.3.3 Falsification Test

As a falsification test, we repeat exactly the same estimation procedure but for the Sales SSU that does not undergo any changes in its goal-setting policy. The salesforce that has always been in the Sales SSU are subjected to automatic ratcheting both before and after August 2016. Appendix A.5 presents the derivation and statistical properties of our measure of supervisor ratcheting for the Sales SSU. Since the SSU salespersons have a common expectation of goal ratcheting before and after August 2016, they should not change their effort. Thus, the coefficient α_2 and α_1 in this falsification test should be zero. We estimate the same three specifications of Table 3 for the Sales SSU. The results are reported in Table 10.

In contrast to the results for the SBUs, the estimates of the coefficient α_2 for the Sales SSU are of small magnitude, and never statistically significant from zero in any of the three specifications. Ratcheting before the centralization also does not affect sales: the estimates of the coefficient α_1 in Specifications (1) and (2) are small in magnitude and not statistically different from zero. Thus, the estimates show that prior supervisor ratcheting has no effect on the workers in the Sales SSU neither before nor after August 2016.

6.3.4 Other Effects of Centralization

The centralization of the SBUs salesforces into the SSU sales division means that not only all salespersons are subject to the same goal setting process, but that other things also change for the employees in the original SBUs, such as logistics, tasks, etc. All these common changes associated with centralization lead to a common shift in sales that salespersons in the SBUs experience, relative to salespersons in the Sales SSU. Furthermore, the centralization might have had other heterogenous effects on performance, for example due to difficulties in merging cultures and coordination problems, which might correlate with ratcheting and thus constitute a possible confound. To take care of these issues, we estimate two specifications: a triple difference regression that allows us to control for the baseline effect of the centralization; and a triple difference regression that also adds controls based on a survey of the supervisors about

the centralization.

The triple difference model uses the data from both the SBUs and Sales SSU. In our context, the first difference is between the SBUs and the Sales SSU, the second is between the periods before and after August 2016, and the third is between the ratcheting levels of different supervisors. With the triple difference, we explicitly control for and estimate the effect of the additional common changes, represented by a common shift, that salespersons in the SBUs experienced during the centralization, relative to the salesforce already in the Sales SSU. Moreover, the triple difference specification allows us to compare formally the effect of eliminating supervisor ratcheting in the SBUs, relative to the Sales SSU. Finally, it improves the efficiency of estimating some parameters.

Formally, let D_i be an indicator equal to 1, if the salesperson i belongs to the SBUs, and 0, if the salesperson belongs to the Sales SSU. The regression model that we take to the data is:[12]

$$s_{it} = \beta_0 + \beta_1 D_i T_t + \beta_2 R_{ijt}(1-D_i) + \beta_3 R_{ijt} D_i + \tag{7}$$

$$\beta_4 R_{ijt} T_t (1-D_i) + \beta_5 R_{ijt} T_t D_i + X_{it}\beta + \gamma_i + \lambda_t + \varepsilon_{it}$$

The identification of models (7) relies on the parallel trend assumption and the additional and stronger identifying assumption that the firm does not take into consideration output when assigning salespersons to supervisors, i.e., the assignment to supervisor ratcheting is exogenous with respect to output considerations.[13] We already showed in section 6.3.1 that this assumption holds in our empirical context.

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[12]This model is equivalent to the more usual specification of triple difference models:

$$s_{it} = \gamma_0 + \gamma_1 T_t + \gamma_2 D_i + \gamma_3 R_{ijt} + \gamma_4 T_t D_i + \gamma_5 R_{ijt} T_t + \gamma_6 R_{ijt} D_i + \gamma_7 R_{ijt} T_t D_i + X_{it}\gamma + \gamma_i + \lambda_t + \varepsilon_{it}$$

Note that in this specification $\gamma_1 T_t$ represents the difference between the average monthly effects before and after the change in goal-setting, while $\gamma_2 D_i$ represents the difference between the average salesperson effect in the Sales SSU and the SBUs. These statistics can be readily calculated after the estimation of equation (7) if necessary. The equivalence between the two specifications can be established by expanding equation (7) and then gathering terms. We estimate equation (7) because it allows us to readily test the hypothesis that for the Sales SSU the change in goal-setting has no effect.

[13]De Chaisemartin and D'Haultfœuille (2020) and Callaway, Goodman-Bacon and Sant'Anna (2021) show the necessity of the latter assumption when the treatment variable is continuous or in doses. The effect of

In this triple difference setting, the theoretical model predicts that $\beta_5 > 0$ and $\beta_4 = 0$. The AESR is equal to $(\beta_5 - \beta_4)\,\overline{R}$, while the MESR is equal to $((\beta_5 - \beta_4)\,\sigma$. The variation in output and ratcheting before the centralization again provides alternative estimates of the AESR and the MESR (similar in magnitude but with opposite sign), which are equal to $(\beta_3 - \beta_2)\,\overline{R}$ and $(\beta_3 - \beta_2)\sigma$, respectively, where β_3 and β_2 are the coefficients of the interactions of ratcheting with the indicators for the SBUs and the Sales SSU, respectively. The theoretical model predicts $\beta_3 < 0$ and $\beta_2 = 0$. The coefficient β_1 stands for the common shift in sales that salespersons in the SBUs experience due to common changes associated with centralization, relative to the Sales SSU.

Table 11 presents the results. We consider three specifications of equation (7) as in Table 3 of section 6.2. The estimate of the common shift parameter β_1 is -0.16 in Specification (1), -0.2 in Specification (2), and -0.11 in Specification (3). None of these estimates is, however, statistically significant. The rest of the results confirm that the elimination of ratcheting has a positive and significant effect on total sales for salespersons in the SBUs: In all models, the coefficient β_5 of the interaction of supervisor ratcheting with the indicators for the SBUs and for the change in goal setting after August 2016 is positive and statistically significant. In contrast, the estimates of the corresponding coefficient β_4 for the Sales SSU are of small magnitude, and never statistically significant from zero in any of the three specifications. Since the estimated effects for the Sales SSU are statistically zero, our triple difference estimates of the effect of eliminating ratcheting coincide in practice with those of the difference-in-differences model for the SBUs.[14]

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switching from ratcheting R to ratcheting R' is:

$$
\begin{aligned}
&\{[E\left(s_{it}\,|T_t=1,\ D_i=1,\ R_{ijt}=R\right) - E\left(s_{it}\,|T_t=0,\ D_i=1,\ R_{ijt}=R\right)] \\
&- \left[E\left(s_{it}\,|T_t=1,\ D_i=1,\ R_{ijt}=R'\right) - E\left(s_{it}\,|T_t=0,\ D_i=1,\ R_{ijt}=R'\right)\right]\} \\
&- \{[E\left(s_{it}\,|T_t=1,\ D_i=0,\ R_{ijt}=R\right) - E\left(s_{it}\,|T_t=0,\ D_i=0,\ R_{ijt}=R\right)] \\
&- \left[E\left(s_{it}\,|T_t=1,\ D_i=0,\ R_{ijt}=R'\right) - E\left(s_{it}\,|T_t=0,\ D_i=0,\ R_{ijt}=R'\right)\right]\}
\end{aligned}
$$

We obtain the formula for the triple difference estimator of the effect of ratcheting by following Angrist and Pischke (2008). First, we define the conditional mean function $E\left(s_{it}\,|T_t,\ D_i,\ R_{ijt}\right)$. For any two distinct levels of ratcheting R and R', this function can take on at least eight values. As explained earlier there are eight coefficients of interest, so the model is (over)identified. We equate each of the empirically observed expected values to equation (7) and use the resulting system equations to solve for the parameters in equation (7), which yields the triple difference estimator.

[14]In Appendix A.6, we reject the hypothesis that a more general specification, which allows all explanatory

As a complementary analysis we use survey-based controls to rule out the possibility that centralization has heterogeneous effects across supervisors that are different from the effect of eliminating supervisor ratcheting. For example, we want to rule out that the centralization problem was more complex for some supervisors than for others and some of the results that we observe are due to this difference in complexity. Right after the centralization, the firm conducted a survey that among other things asked supervisors about the challenges associated with the centralization. The questions of interest to us are related to the complexity of centralization, how potential clash of divisional cultures affected the implementation, how centralization affected coordination, how centralization affected the relations with clients, and how centralization affected the ability to respond to market changes. Supervisors responded to each question on a scale from one to five. For each question, we group the responses into two categories: agree and disagree with the statement of the question. Then, we interact the resulting dummies with the indicator for the centralization event and introduce these new variables in our triple difference specifications presented in Table 11. In all specifications, the estimates of the effect of eliminating ratcheting do not change after the introduction of the new variables. Moreover, in our most general specification (3), none of the coefficients of the new variables is significant. The online Appendix A.7 presents the results in more detail.

6.4 Non-Linear Specifications

The linear specification that we have presented so far is easy to interpret and provides clear results, yet it relies on strong assumptions (De Chaisemartin and D'Haultfœuille, 2020; and Callaway, Goodman-Bacon and Sant'Anna, 2021). To show that linearity is not driving our results, we now present two different non-parametric specifications that yield similar results. These specifications also have the advantage that they allow us to estimate how sales change in a nonlinear way as ratcheting increases.

variables to have different coefficients for the Sales SSU and the SBUs, offers a better fit to the data.

6.4.1 Quartiles Specification

We examine a "discretized" difference-in-differences regression in which ratcheting is replaced by dummies representing its quartiles. To obtain the full set of regressors, we interact the dummies representing the quartiles with the indicators for the change from supervisor to automatic ratcheting in August 2016. Apart from the replacement of ratcheting by its quartiles, the specifications that we consider are the same as specifications (1), (2) and (3) in Table 3, with careful consideration of the reference groups for coefficient interpretation.[15]

Formally, we consider the following version of model (5): The effect of eliminating supervisor ratcheting corresponds to the difference-in-differences defined across two dimensions—before and after August 2016; and relative to the first quartile of ratcheting. In contrast to the benchmark model, the quartile specification allows the effect of ratcheting on output to change from one quartile to the next. As it can be seen from equation (6), the difference between the coefficients of successive quartiles identifies how the effect of ratcheting on sales evolves from one ratcheting quartile to the next. By definition, this incremental effect does not vary in the linear specification. Thus, the quartile specification allows us to examine the validity of this assumption.

Table 12 reports the regression results. We consider the same three specifications as in Table 3. In Specification (1), the estimates for the interaction of ratcheting quartiles with the indicator for the change in goal setting after August 2016 are all positive and statistically significant: The coefficient for the second quartile is 0.57; for the third, 0.45; and for the fourth, 0.86. In our benchmark Specification (2), the coefficients of the effect of eliminating supervisor ratcheting for the second, third and fourth quartiles are 0.57, 0.49 and 0.88, respectively. In Specification (3), we introduce supervisor fixed effect and obtain somewhat smaller effects of eliminating ratcheting relative to the benchmark first quartile: 0.48, 0.48, and 0.68 for the second, third, and fourth quartiles respectively. Still, none of the coefficients of interest in

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[15] The constant in the regression absorbs the effect of the first ratcheting quartile and provides the reference with respect to which we evaluate the effect of the second, third, and fourth ratcheting quartile on sales. The average of the monthly dummies after August 2016 provides the reference with respect to which we evaluate the change in the effect of the second, third, and fourth ratcheting quartile on sales after August 2016.

Specification (3) are statistically different from those found in Specification (2). Thus, we confirm that even in this more general specification, the non-linear effects of ratcheting on output are not driven by observable and unobservable characteristics of supervisors, once we control for supervisor ratcheting. With respect to statistical significance, the estimates of the effect of supervisor ratcheting in all three specifications are all statistically different from their counterpart for the reference category, the first quartile.

Moreover, the estimates of the quartiles regression show that there are pronounced differences in the way ratcheting affects sales when comparing salespersons subjected to little or no ratcheting, salespersons subjected to intermediate levels of ratcheting, and salespersons subjected to high levels of ratcheting. We explore further these nonlinearities in section 6.4.3.

6.4.2 Specification with Supervisor-Specific Effects

As an even more general robustness test, we run a specification in which the effects of switching the goal-setting policy are estimated fully non-parametrically with individual dummies for each supervisor. In this way, we impose no restrictions on how the supervisor's characteristics affect sales as a result of the restructuring of August 2016.

Specifically, we estimate a panel data regression of log sales, s_{it}, on supervisor effects that apply for the whole period of study, φ_j, supervisor effects that apply only for the period after August 2016, ρ_j, control variables, X_{it}, that include the tenure of the salesperson and of the supervisor, salesperson fixed effects, unit location dummies, time dummies and dummies for the salesperson's residential area:

$$s_{it} = \beta_0 + \rho_j T_t + \varphi_j + X_{it}\beta + \gamma_i + \lambda_t + \varepsilon_{it} \tag{8}$$

The supervisor effects, φ_j, for the whole period represent the impact of each supervisor on the performance of salespersons both before and after the centralization. The supervisor effects for the period after August 2016, ρ_j, capture the effect of the centralization at the level of

each supervisor.[16] For the rest of this section, we refer to these effects as the supervisor effect of eliminating supervisor ratcheting (SEER). For the SBUs, there are three predictions: (1) there is significant variation in the supervisor effects ρ_j; (2) this variation in ρ_j can be traced back to differences in supervisor ratcheting; and (3) the supervisor effects φ_j and ρ_j are negatively related if the supervisor effect largely reflects supervisor ratcheting, since the elimination of supervisor ratcheting should imply that the latter cancels the former. Figure 2, Panel 2.1 presents the estimated SEER. It shows that there is heterogeneity in the estimated coefficients, and that they are precisely estimated.

Panel I in Table 13 reports the results from regressing ρ_j (the supervisor effect of changing from supervisor to uniform ratcheting) on φ_j (the overall supervisor effects). As expected, the relationship between the two is negative and statistically significant, with a coefficient estimate of -0.19. To visualize the regression analysis, Figure 3 plot the two sets of supervisor effects against each other. Panel II of Table 13 goes further and shows that, for the SBUs, there is a positive relationship between the SEER and our benchmark measure of supervisor ratcheting. It shows that an increase in initial supervisor ratcheting is associated with a larger SEER. Finally, Panel III investigates whether the relationship from Panel II survives the introduction of control variables that represent how managers allocate their time across tasks.[17] The relationship survives the introduction of these additional controls: In fact, for the SBUs the coefficient of ratcheting more than doubles in size, from 0.66 in Panel II to 1.43 in Panel III.

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[16]This specification allows us to test whether unobservable characteristics of the supervisor have a differential effect after the organizational change. The changes that supervisors experience are minor in comparison to salespersons: the core task of supervisors remain unchanged, namely, managing a group of salespersons (salesperson training/mentoring, tracking performance and motivation, administrative work, and daily team meetings). This leads us to think that supervisor traits that might be relevant in periods of change, such as "openness to experience" or "resistance to change", did not play a large role, especially in contrast to the big and significant changes in ratcheting expectations.

[17]In April 2017, the company conducted a survey about the allocation of supervisor time to various managerial activities. For the survey, the company asked a subsample of 100 supervisors how they allocate their time in the first trimester of 2016 and for the first trimester of 2017. Thus, the first wave of the survey reflects the allocation of supervisor time to managerial activities before the changes in August 2016 while the second reflects managerial practices after the change in August 2016.

6.4.3 Nonlinear Output Cost of Ratcheting

The nonlinear specifications in sections 6.4.1 and 6.4.2 allow us to formally investigate how the output cost varies with the level of supervisor ratcheting in the SBUs and to determine whether these incremental changes in the effect of ratcheting are statistically significant. Thus, the nonlinear specifications allow us to detect if there is a range in which ratcheting has its greatest effect on sales. Following Angrist and Imbens (1995), we define the incremental change in sales as ratcheting increases from one quartile to the next as the causal response of sales to ratcheting.

To rigorously estimate the causal responses, we consider the triple difference equivalents of the non-linear difference-in-differences specifications in sections 6.4.1 and 6.4.2, where ratcheting in equation (7) is replaced by its quartiles or by supervisor fixed effects. Online appendix A.8 presents the results. With these triple difference specifications, we verify that ratcheting has no effect for the Sales SSU workforce, even when we allow for a nonlinear output cost of ratcheting. Thus, the estimated effects of ratcheting in the triple difference regressions coincide in practice with their counterparts for the difference-in-differences regressions. Specifically, we find that the effect of eliminating supervisor ratcheting monotonically increases but at a decreasing rate.

Figure 4 presents the estimated causal responses, while Figure 5 plots the cumulative output changes due to the switch away from supervisor subjective ratcheting to uniform ratcheting after the centralization. The results for the quartile specification, plotted in Panel 2 of Figure 4, show that the causal response decreases from 0.53 to 0.25 and 0.08 as we move from the first to the second, from the second to the third, and from the third to the fourth quartile, respectively. In percentage terms, these estimates translate to a marginal decrease in output from the first to the second, from the second to the third, and from the third to the fourth quartile of ratcheting of 69%, 28%, and 8%, respectively. The first two estimated causal responses are statistically different from zero. Thus, ratcheting has its greatest impact on output when salespeople move from no ratcheting to some ratcheting, while moving from average ratcheting to high ratcheting leads to a more limited behavioral response. Putting

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all effects together, Panel 2 of Figure 5 shows how sales change for salespeople in each of the ratcheting quartiles. Salespeople who before the centralization have "good" supervisors (i.e., supervisors who engage in low ratcheting) decrease their productivity, while the opposite happens for salespeople who have "bad" supervisors (i.e., supervisors who engage in high ratcheting). Panel 3 of Figure 4 and Panel 3 of Figure 5 show that when we estimate the model with supervisor fixed effects for the Sales SSU and the SBUs, we obtain similar results to what we find under the quartiles specification.

To compare the causal responses of the linear and quartile specification, we define the quartile equivalents for the linear specification. We compute the predicted increase in sales due to the elimination of ratcheting evaluated at the median of each quartile of ratcheting. The causal response then corresponds to the difference between the predicted median values from one quartile to the next.[18] Panel 1 of Figure 4 shows that, not surprisingly, the causal response under the linear specification does not change across quartiles and is around 0.14. In percentage terms, this estimate translates into a 15% incremental increase in the effect of supervisor ratcheting from one quartile to the next. In contrast, the quartile specification shows decreasing causal responses indicating that output reduction is larger when people move from an environment with no ratcheting to one with moderate ratcheting than when they move from moderate to high ratcheting levels. However, the flexibility of the quartile specification comes at the price of a reduction in the precision of the estimates, relative to the linear specification.

7   Discussion and Conclusion

This paper estimates the non-linear effect of ratcheting on output in the context of a large Chilean company engaged in the distribution and sale of beverages. The institutional setting and the change from supervisor ratcheting to a common method of goal-setting for sales allows

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[18]The causal response in the linear model, when moving from quartile q to quartile $q + 1$, is

$$(\beta_5 - \beta_4)(R_{q+1} - R_q)$$

where R_q is the median ratcheting in quartile q.

us to estimate the output cost of ratcheting as supervisor ratcheting varies.

We find that, in the context of this firm, one standard deviation reduction in supervisor ratcheting leads to an increase in output by 19%. Our flexible specifications show that the incremental effect of increasing ratcheting is greater when salespersons face low to moderate levels of ratcheting. We show that these results are not driven by educational or cognitive differences among the employees. In fact, the results show that the salespeople of the firm are quite sophisticated: Their behavior is not driven by the past but by future expectations about ratcheting, and they adjust quickly to changes in goal-setting. Moreover, the non-linearity in their response to ratcheting persists even under the most general specifications where we control for the identity and the characteristics of their supervisors. These results are not driven by non-random assignment of salespersons to supervisors. We leave to future research to explore the causes of this non-linear response.

The results also caution that transparency, uniformity and formal commitment are not by themselves a solution to strategic output reduction caused by ratcheting, if the firm commits to a rule that still conditions goals on past performance. Moreover, they show that the good managerial practices of some supervisors who have a record of not behaving opportunistically can have a large effect on productivity without the need for institutional formal commitment. Our estimates help identify supervisors who do not opportunistically ratchet up goals when they learn about the productivity of their subordinates. As a result, the salespersons in their units turn out to have higher sales before the centralization. These supervisors manage to achieve this outcome even in the absence of formal transparency and firm commitment.

The value of such "good" managers becomes even greater in view of our finding that the output cost of ratcheting is likely nonlinear: The incremental output cost of switching from a "good" manager who does little or no ratcheting to a manager who applies an intermediate level of ratcheting is greater than the output cost associated with switching from a supervisor who applies an intermediate level of ratcheting to a supervisor who applies high level of ratcheting. Thus, our findings suggest that firms would do well if they identify such "good" managers and investigate whether their managerial practices could be adopted more broadly.

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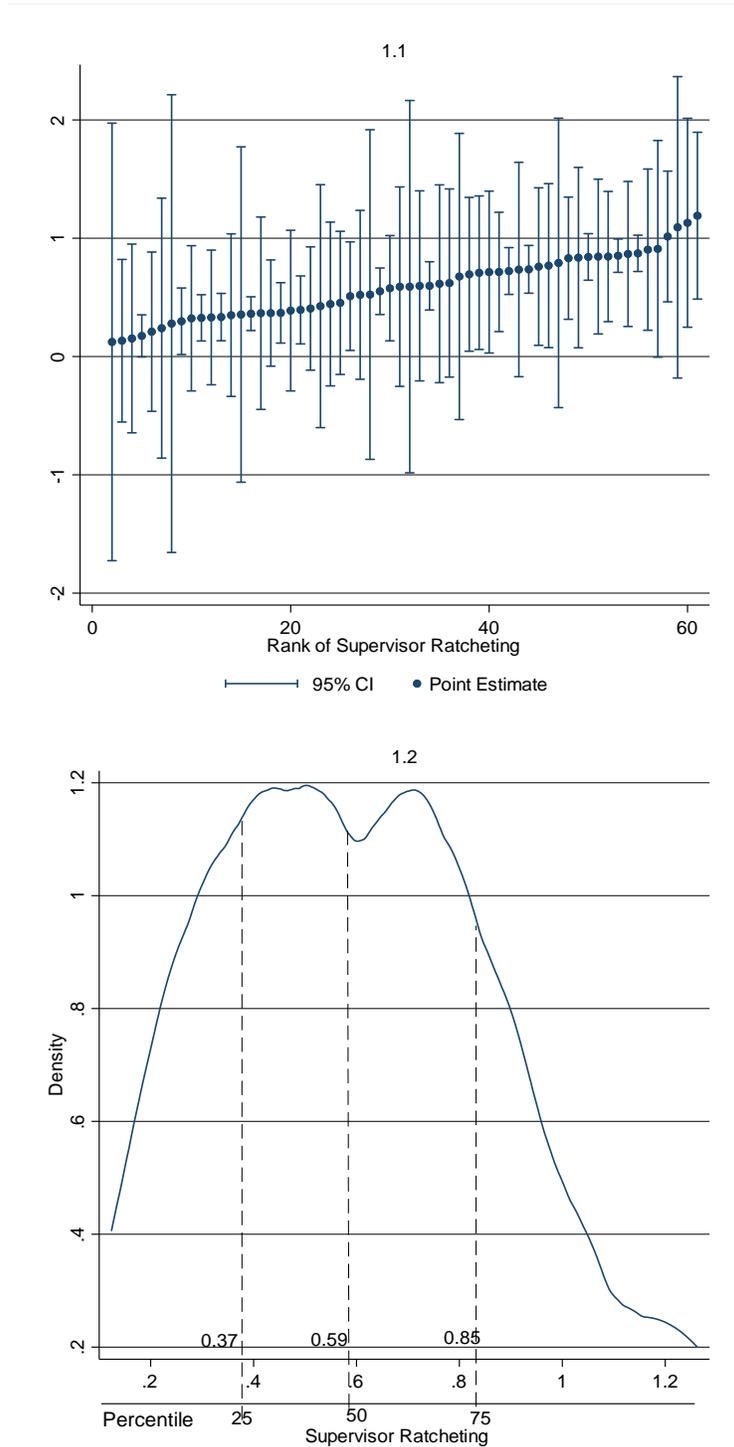
Figure 1: Supervisor Ratcheting, SBUs. Panel 1.1 presents the estimated supervisor ratcheting for the SBUs, which we obtain following the methodology in Brahm and Poblete (2018). Panel 1.2 presents the density distributions of the estimated supervisor ratcheting for the SBUs.
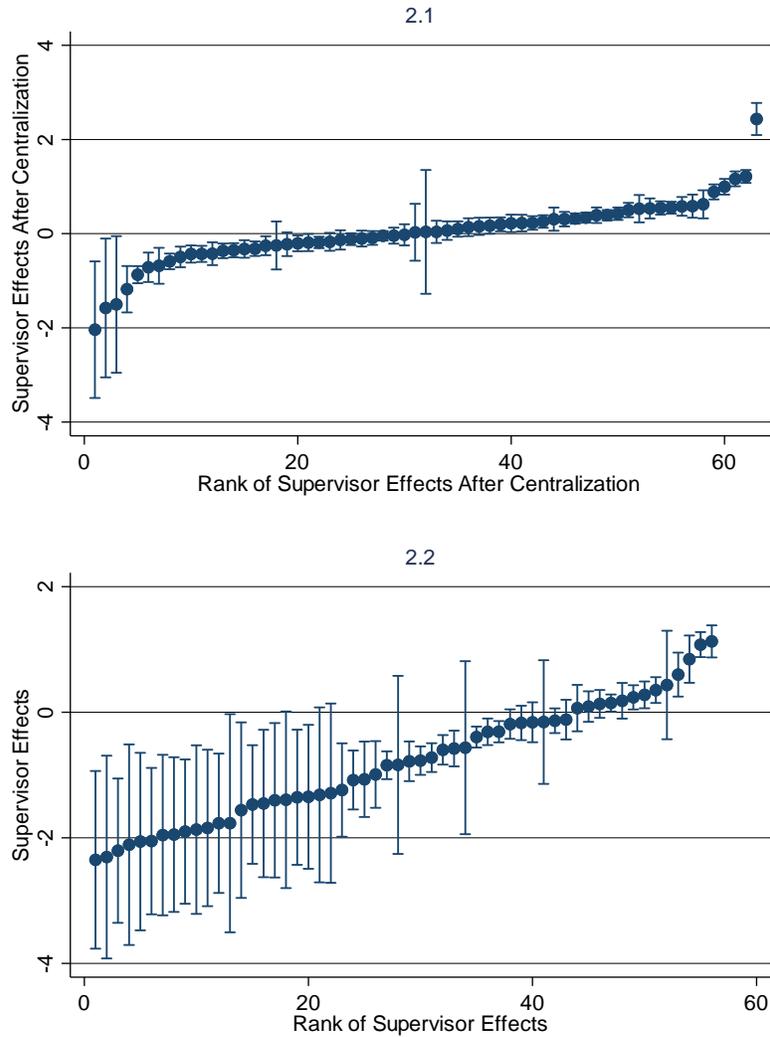
Figure 2: Supervisor Effects. Panel 2.1 presents the estimated supervisor effects on sales after the centralization of the SBUs (measured in ln(sales)) introduced in section 6.4.2. The variation in the estimated supervisor effects after centralization captures the effect of eliminating supervisor ratcheting on sales. Panel 2.2 presents for the SBUs the supervisor effects that apply to the whole period of study (measured by ln(sales)). We obtain the fixed effect estimates from the regression of log total sales on supervisor fixed effects that apply for the whole period of study, distinct supervisor fixed effects for the period after centralization, supervisor tenure, salesperson tenure, and location and time dummies.
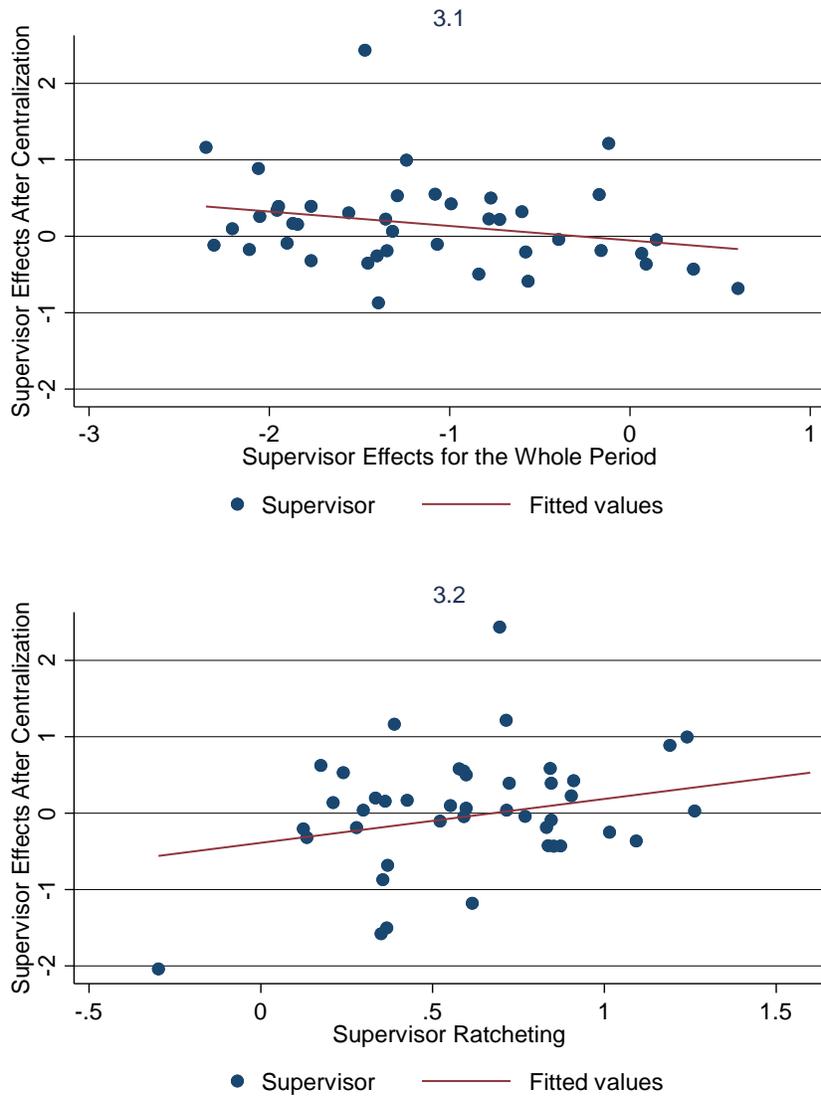
Figure 3: Relationship Between Supervisor Effects and Supervisor Ratcheting. Panels 3.1 presents for the SBUs the relationships between the supervisor effects for the period after the centralization and the supervisor effects that apply to the whole period of study (measured by ln(sales)), introduced in section 6.4.2. The variation in the estimated supervisor effects after the centralization represents the effect of eliminating supervisor ratcheting on sales. Panel 3.2 presents for the SBUs the relationship between the supervisor effects for the period after the centralization and our measure of supervisor ratcheting. We obtain the fixed effect estimates from the regression of log total sales on supervisor fixed effects that apply to the whole period of study, and from distinct supervisor fixed effects for the period after centralization, supervisor tenure, salesperson tenure, and location and time dummies.
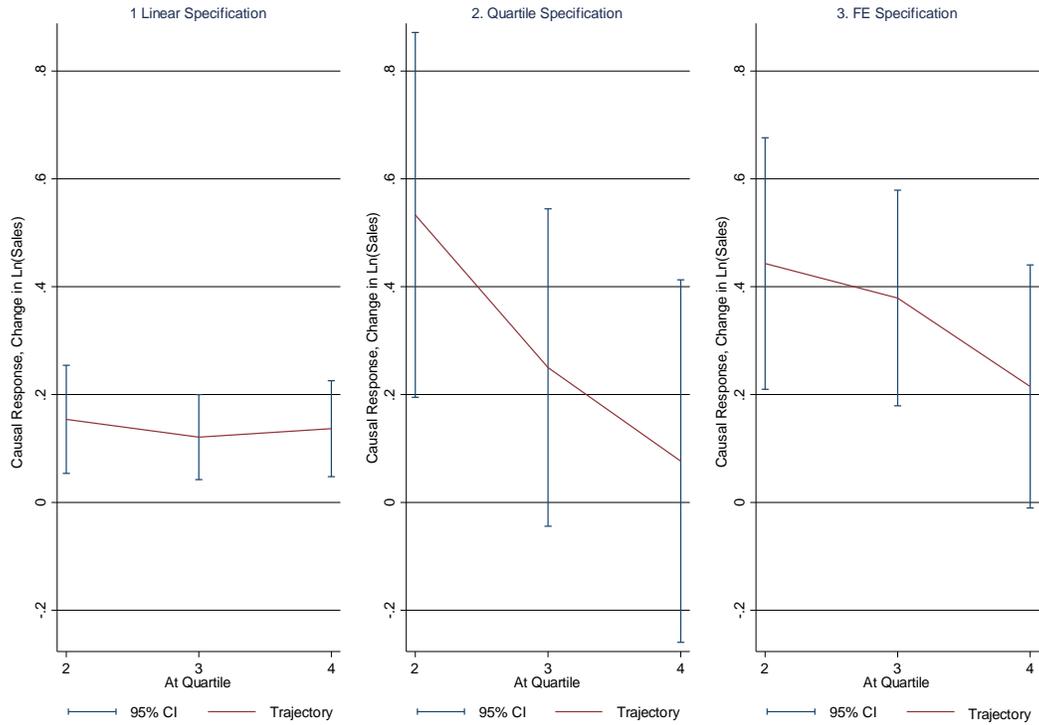
Figure 4: Causal Responses of Eliminating Ratcheting under Centralization. The figure presents the estimated causal responses of eliminating supervisor ratcheting under the linear, quartile, and fixed effects specifications (Panels 1, 2, and 3, respectively). The causal response is defined as the change in the effect of eliminating supervisor ratcheting on sales from one quartile to the next. The causal response of moving from quartile q-1 to q is reported at quartile q, where q=2, 3, and 4. In Panels 1 and 2, the quartiles are defined with regard to the distribution of supervisor ratcheting for the SBUs. In Panel 3, the quartiles are defined with regard to the distribution of estimated supervisor effects for the SBUs in the period after August 2016. For the linear model, the effect of eliminating supervisor ratcheting and the associated causal response are evaluated at the median for each quartile. For the fixed effects specification, the effect of eliminating supervisor ratcheting for each quartile is equal to the corresponding median supervisor effects for the period after August 2016 for each quartile.
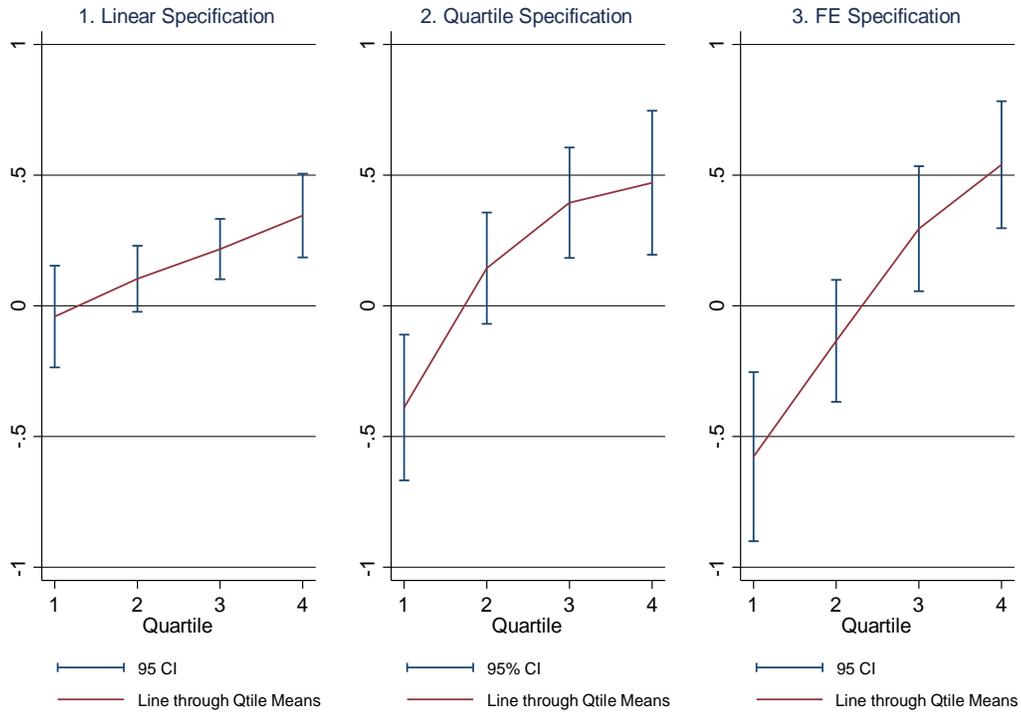
Figure 5: Output Changes due to the Elimination of Ratcheting under Centralization. The figure presents the estimated output change due to the switching from supervisor to uniform ratcheting under centralization for the linear, quartile, and fixed effects specifications (Panels 1, 2, and 3, respectively). In both Panels 1 and 2, the quartiles are defined with regard to the distribution of supervisor ratcheting for the SBUs. Panel 1 plots the point estimates and the associated confidence intervals of the linear specification, evaluated at the medians of each of the ratcheting quartiles. Panel 2 plots the point estimates and the associated confidence intervals of the quartile specification. Panel 3 plots the point estimates and the associated confidence intervals of the supervisor fixed effects specification. In Panel 3, the quartiles are defined with regard to the distribution of estimated supervisor effects for the SBUs in the period after August 2016.

Table 1: Total Sales Before and After Centralization, SBUs salesforce

	Raw Sales (mln CLP)		Adjusted Sales (mln CLP)		Monthly Averages	
	Avg. Sales	Std. Dev.	Avg. Sales	Std. Dev.	Supervisors	Salespeople
Under Ratcheting	74.68	71.83	70.51	58.42	55.14	364.14
Post Ratcheting	81.91	79.10	84.14	73.27	53.08	387.23

Note: Workers in the SBUs switch from supervisor ratcheting to automatic goal setting in August 2016. The sample includes data for the period January 2016 - August 2017. Adjusted sales stand for sales for which monthly and location-specific variation is eliminated.

Table 2: Theoretical Predictions for the Benchmark Model

Difference-in-Differences Model: Equation (5)			
Variable	Notation	Coefficient	Prediction
Ratcheting	R_{ijt}	α_1	< 0
Ratcheting*Post	$R_{ijt}T$	α_2	> 0
Effect of Eliminating Ratcheting	Notation		Prediction
AESR	$\alpha_2 R$		> 0
MESR	$\alpha_2 s$		> 0

Note: This table presents the theoretical predictions about the estimates of our benchmark difference-in-differences model defined by equations (5). "Post" represents the switch from supervisor ratcheting to automated goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. R stands for average ratcheting and s stands for one standard deviation of the distribution of ratcheting. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting.

Table 3: Benchmark Estimates: Log-Level Difference-in-Differences Regressions of Total Sales

	Model (1)			Model (2)			Model (3)		
Panel I.	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.253	0.152	-1.66	-0.191	0.154	-1.24			
Ratcheting*(Post)	0.499	0.167	2.99	0.556	0.163	3.41	0.523	0.190	2.75
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effects of Eliminating Ratcheting									
AESR	0.298	0.100	2.99	0.332	0.097	3.41	0.313	0.114	2.75
MESR	0.160	0.054	2.99	0.178	0.052	3.41	0.168	0.061	2.75
Obs.	4751			4751			4751		
R-sqr, within	0.28			0.30			0.46		

Note: The table reports the estimates of our benchmark log-level difference-in-differences regressions of total sales defined by equation (5) for the SBUs. Workers in the SBUs switch from supervisor ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

Table 4: Testing for Non-Random Reassignment of Supervisors: Log-Level Difference-in-Differences Regression of Total Sales

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.233	0.152	-1.53	-0.159	0.152	-1.05			
Ratcheting*Post	0.588	0.171	3.44	0.655	0.163	4.03	0.590	0.182	3.24
Change	-0.003	0.061	-0.05	0.026	0.089	0.30	-0.001	0.078	-0.01
Change, F1	0.116	0.057	2.1	0.123	0.069	1.77	0.094	0.064	1.47
Change, L1	0.064	0.067	0.95	0.110	0.088	1.25	0.045	0.078	0.57
(Change)*Post	0.104	0.077	1.35	0.101	0.099	1.03	0.087	0.095	0.91
(Change, F1)*Post	-0.082	0.075	-1.09	-0.070	0.078	-0.89	-0.022	0.077	-0.29
(Change, L1)*Post	-0.019	0.074	-0.25	-0.045	0.090	-0.5	-0.016	0.082	-0.2
Location, Period, Salesperson FE	Yes			Yes			Yes		
Time-Varying Controls	No			Yes			Yes		
Supervisor FE	No			No			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Ratcheting									
AESR	0.351	0.102	3.44	0.391	0.097	4.03	0.353	0.109	3.24
MESR	0.189	0.055	3.44	0.210	0.052	4.03	0.189	0.058	3.24
Obs.	4301			4301			4301		
R-sqr, within	0.41			0.42			0.57		

Note: The table reports the estimates of log-level triple-difference regressions of total sales for the SBUs, which test the hypothesis that reassignments of salespersons to supervisors are unrelated to sales. "Change" is a dummy equal to 1 if the salesperson changes the supervisor and 0 otherwise. L1 and F1 denote lag and forward operators for the variable "Change". Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. "Post" represents the change in compensation policy that takes place in August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying variables of the salesperson include tenure, supervisor tenure, unionization status, residential area dummies, type of employment contract, and family status. The table reports robust standard errors.

Table 5: Test for Random Allocation of Salespersons to Supervisors

Salesperson characteristics	F-test	Restricted R-sqr	Unrestricted R-sqr	R-sqr diff
High School Education	25.463	0.225	0.335	0.111
University Education	19.107	0.197	0.274	0.078
Female	18.113	0.115	0.172	0.056
Union Members	16.152	0.431	0.433	0.002
Married	21.316	0.179	0.274	0.095
Age	19.154	0.243	0.322	0.079
Tenure	26.879	0.203	0.321	0.118

Note: The table presents statistics from the regression of salespersons' characteristics on location and time dummies, supervisor fixed effects and supervisor tenure. The statistics summarize the extent to which variation in supervisor fixed effects (i.e., supervisor characteristics) explains variation in the characteristics of salespersons. "Restricted" refers to the specification in which we regress the characteristics of the salespersons only on location and time dummies. "Unrestricted" refers to the specifications in which we also include the supervisor fixed effects and tenure as regressors. The difference in the R-squared of the unrestricted and of the restricted specification measures the improvement in the fit from the introduction of supervisor fixed effects and tenure. The related F-tests are statistically significant.

Table 6: Log-Level Difference-in-Differences Regressions of Total Sales with Gradual Adjustment to Changes in Ratcheting

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.239	0.161	-1.48	-0.189	0.157	-1.21	-0.192	0.159	-1.21
Ratcheting*Post	0.686	0.176	3.9	0.548	0.167	3.28	0.551	0.168	3.27
Ratcheting*(06/2016)							-0.008	0.099	-0.08
Ratcheting*(07/2016)	-0.028	0.106	-0.26	-0.011	0.098	-0.11	0.016	0.042	0.38
Ratcheting*(08/2016)				0.013	0.109	0.12	0.013	0.111	0.11
Ratcheting*(09/2016)				0.085	0.070	1.21	0.084	0.074	1.14
Ratcheting*(10/2016)							-0.004	0.043	-0.1
Time-Varying Controls	Yes			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Ratcheting									
AESR	0.410	0.105	3.9	0.328	0.100	3.28	0.330	0.101	3.27
MESR	0.220	0.057	3.9	0.176	0.054	3.28	0.177	0.054	3.27
Obs.	4751			4751			4751		
R-sqr, within	0.40			0.40			0.40		

Note: The table reports the estimates of log-level difference-in-difference regressions of total sales, which test the hypothesis of gradual adjustment of sales to the expected or actual changes in goal setting. The regressions include interactions of ratcheting with time dummies for the months from June to October 2016. Workers in the SBUs switch from ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. It is estimated following the methodology in Brahm and Poblete (2018) using data for the period January-December 2015. The dummy "Post" represents the change in compensation policy that takes place in August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying variables of the salesperson include tenure, supervisor tenure, unionization status, residential area dummies, type of employment contract, and family status. The table reports robust standard errors.

Table 7: Log-Level Difference-in-Differences Regressions of Total Sales with Average Ratcheting

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Avg. Ratcheting*Post	0,451	0,131	3,45	0,510	0,163	3,12			
Ratcheting							-1,116	0,506	-2.2
Ratcheting*Post							0.680	0.168	4.05
(Avg Ratcheting-Ratcheting)*Post							0.151	0.539	0.28
(Change, 08/2016)*Post				-0.160	0.139	-1.15			
Ratcheting*(Change, 08/2016)*Post				-0.005	0.145	-0.03			
Salesperson, Period, Location FE,	Yes			Yes			Yes		
Time-Varying Control	Yes			Yes			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Ratcheting									
AESR	0.270	0.078	3.45	0.305	0.098	3.12	0.407	0.100	4.05
MESR	0.145	0.042	3.45	0.164	0.052	3.12	0.218	0.054	4.05
Obs.	4751			4751			4751		
R-sqr, within	0.41			0.36			0.37		

Note: The table reports the estimates of log-level difference-in-differences regressions of total sales, in which the effect of ratcheting is estimated using the average ratcheting experienced by salespersons before the introduction of uniform automatic goal setting. The difference between average past ratcheting and current ratcheting in Model (3) tests the hypothesis that not only current but also average ratcheting drive the behavior of salespersons. It is so if this variable is significant. Workers in the SBUs switch from ratcheting to automatic goal setting in August 2016. The dummy "Post" represents the change in compensation policy that takes place in August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

Table 8: Log-Level Difference-in-Differences Regressions for Log(Total Sales) with Lagged Ratcheting

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
(Ratcheting, L1)	-0.202	0.145	-1.39	-0.152	0.168	-0.9			
(Ratcheting, L1)*Post	0.386	0.178	2.17	0.418	0.202	2.07			
(Ratcheting, L2)				0.184	0.123	1.49			
(Ratcheting, L2)*Post				-0.181	0.128	-1.41			
(Ratcheting, L3)				-0.221	0.118	-1.88			
(Ratcheting, L3)*Post				0.159	0.147	1.08			
Ratcheting							-0.212	0.159	-1.34
Ratcheting*Post							0.571	0.172	3.32
(Ratcheting-Ratcheting, L1)							0.283	0.481	0.59
(Ratcheting-Ratcheting, L1)*Post							-0.546	0.666	-0.82
Time-Varying Controls	Yes			Yes			Yes		
Salesperson, Period, Location FE	Yes			Yes			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Subjective Ratcheting									
AESR	0.231	0.106	2.17	0.237	0.109	2.17	0.341	0.103	3.32
MESR	0.124	0.057	2.17	0.127	0.059	2.17	0.183	0.055	3.32
Obs.	4514			4356			4514		
R-sqr, within	0.28			0.29			0.30		

Note: The table reports the estimates of log-level difference-in-differences regressions of total sales, in which the effect of ratcheting is estimated using ratcheting from previous months. The difference between current ratcheting and last month's ratcheting in Model (3) tests the hypothesis that not only current but also past ratcheting drive the behavior of salespersons: this is confirmed if this variable is significant. Workers in the SBUs switched from supervisor ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. L1 to L3 represent lag operators. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

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Table 9: Estimates for Salespersons without any Tertiary Education: Log-Level Difference-in-Difference Regressions of Total Sales

	Model (1)			Model (2)			Model (3)		
Panel I.	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.249	0.169	-1.47	-0.164	0.171	-0.96			
Ratcheting*(Post)	0.366	0.194	1.89	0.445	0.180	2.48	0.642	0.230	2.79
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effects of Eliminating Ratcheting									
AESR	0.219	0.116	1.89	0.266	0.107	2.48	0.384	0.138	2.79
MESR	0.118	0.062	1.89	0.143	0.058	2.48	0.206	0.074	2.79
Obs.	2,998			2,998			2,998		
R-sqr, within	0.28			0.30			0.46		

Note: The table reports the estimates of our benchmark log-level difference-in-difference regressions of total sales defined by equation (5) for the SBU unit for the subsample of salespersons without any tertiary education. Workers in the SBU unit switched from supervisor ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. AESR stands for average effect of supervisor ratcheting and HESR for heterogeneity effect of supervisor ratcheting. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

Table 10: Falsification Test: Log-Level Difference-in-Differences Regressions of Total Sales, Sales SSU

Panel I.	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	0.073	0.094	0.78	0.096	0.084	1.15	-0.034	0.053	-0.64
Ratcheting*(Post)	-0.087	0.049	-1.75	-0.056	0.056	-1			
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effects of Eliminating Ratcheting									
AESR	-0.05	0.03	-1.75	-0.032	0.032	-1	-0.019	0.030	-0.64
MESR	-0.028	0.016	-1.75	-0.018	0.018	-1	-0.011	0.017	-0.64
Obs.	3354			3354			3354		
R-sqr, within	0.26			0.27			0.38		

Note: The table reports the estimates of our benchmark log-level difference-in-differences regressions of total sales defined by equation (5) for the Sales SSU. Workers in the Sales SSU always operate under automatic goal setting in the period of study. The dummy "Post" represents the period after August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. AESR stands for average effect of supervisor ratcheting and HESR for heterogeneity effect of supervisor ratcheting. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

Table 11: Effect of Centralization: Log-Level Triple Differences Regressions of Total Sales

	Model (1)			Model (2)			Model (3)		
Panel I	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting*SBU	-0.253	0.152	-1.66	-0.227	0.153	-1.48			
Ratcheting*Post*SBU	0.503	0.167	3.02	0.540	0.164	3.29	0.508	0.188	2.7
Ratcheting*SSU	0.034	0.095	0.35	0.019	0.096	0.2			
Ratcheting*Post*SSU	-0.004	0.070	-0.06	0.000	0.071	-0.01	0.007	0.069	0.11
SBU*Post	-0.162	0.137	-1.18	-0.198	0.137	-1.44	-0.105	0.161	-0.65
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Ratcheting									
AESR	0.323	0.116	2.78	0.344	0.114	3.01	0.319	0.128	2.48
MESR	0.173	0.062	2.78	0.184	0.061	3.01	0.162	0.065	2.48
Obs.	8105			8105			8105		
R-sqr, within	0.27			0.27			0.43		

Note: The table reports the estimates of the log-level triple differences regressions of total sales defined by equation (7). Workers in the SBUs switch from supervisor ratcheting to automatic goal setting in August 2016. Workers in the Sales SSU always operated under automatic goal setting. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. AESR stands for average effect of supervisor ratcheting and HESR for heterogeneity effect of supervisor ratcheting. The table reports robust standard errors.

Table 12: Log-Level Difference-in-Differences Regressions for Total Sales with Ratcheting Quartiles

Ref: Quartile 1	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
(Q2 Ratcheting)	-0.444	0.149	-2.97	-0.406	0.139	-2.93			
(Q3 Ratcheting)	-0.357	0.162	-2.2	-0.325	0.151	-2.16			
(Q4 Ratcheting)	-0.628	0.197	-3.19	-0.531	0.190	-2.8			
(Q2 Ratcheting)*Post	0.573	0.166	3.46	0.569	0.151	3.78	0.477	0.117	4.08
(Q3 Ratcheting)*Post	0.448	0.165	2.72	0.493	0.149	3.31	0.474	0.108	4.38
(Q4 Ratcheting)*Post	0.858	0.181	4.75	0.876	0.171	5.11	0.683	0.135	5.04
Time-Varying Controls	No			Yes			Yes		
Salesperson, Period, Location FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Obs.	4751			4751			4751		
R-sqr, within	0.29			0.31			0.50		

Note: The table reports the estimates of log-level difference-in-difference regressions of total sales that use the quartiles of supervisor ratcheting. Workers in the SBUs switch from supervisor ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. Qx Ratcheting represents the x quartile of the distribution of ratcheting for the SBUs. The first quartile of ratcheting is the reference category. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

Table 13: Analysis of Supervisor Effects of Eliminating Ratcheting

Panel I: FE After Centralization	Coef.	Std. Err.	t
FE Ratcheting	-0.189	0.095	-1.98
Const	-0.055	0.157	-0.35
Obs		42	
R-sqr		0.11	
Panel II: FE After Centralization	Coef.	Std. Err.	t
Ratcheting	0.661	0.324	2.04
Const	-0.371	0.241	-1.54
Obs		45	
R-sqr		0.09	
Panel III: FE After Centralization	Coef.	Std. Err.	t
Ratcheting	1.431	0.419	3.41
Coaching time after change	0.020	0.015	1.28
Planning after change	0.036	0.030	1.18
Admin work after change	-0.005	0.022	-0.25
Prep coaching after change	0.136	0.049	2.8
Const	-2.992	1.078	-2.78
Obs		34	
R-sqr		0.4	

Note: We perform a panel data regression of log total sales on supervisor effects that apply to the whole period of study, supervisor fixed effects for the period after the centralization (which capture the impact of centralization at the level of each supervisor), salesperson tenure, supervisor tenure, and location and time dummies. Panel I presents the regression of the supervisor fixed effects for the period after centralization on the supervisor fixed effects for the whole period of study. Panel II presents the regression of the supervisor effects for the period after centralization on our baseline measure of ratcheting. Panel III presents the regressions of the supervisor effects after August 2016 on our baseline measure of ratcheting and the time devoted to various managerial activities. The table reports robust standard errors.

Online Appendix

A.1 Estimation of Ratcheting Behavior Using Daily Data

As a preliminary step in our empirical work, we present evidence for the existence of output restriction in response to ratcheting. To do so, we use daily data for the period March 2017 – June 2019, which contain 507,152 observations. Unfortunately, we have no access to such daily data for the period when the change from supervisor ratcheting to automatic uniform ratcheting took place. The daily data contains the same variables about the employment history and the demographics of salespersons and their supervisors. The main difference is that they also contain detailed records of daily sales by product category for each salesperson in the company, as well as the monthly goals for each salesperson by product category. Thus, the daily data allow us to study whether and how sales (and therefore effort) adjust as salespersons approach their monthly goals established by the automatic goal setting policy after August 2016.

We estimate the following equation:

$$Ln\left(Volume_{ijtd}\right) = \gamma_0 + \gamma_1 GA_{ijt(d-1)}^{0-80} + \gamma_2 GA_{ijt(d-1)}^{80-90} + \gamma_3 GA_{ijt(d-1)}^{90-100} + \gamma_4 GA_{ijt(d-1)}^{100-110}$$
$$+ \gamma_5 GA_{ijt(d-1)}^{110-120} + \gamma_6 GA_{ijt(d-1)}^{120-} + u_{ij} + v_d + w_t + \varepsilon_{ijtd}$$

where the subindex i denotes salesperson, the subindex j denotes the category (which we restrict to beer and soft drinks in this analysis), the subindex t denotes the year-month period (26 periods in total), and the subindex d denotes the day of the month (from 1 to 31). The variable $GA_{ijt(d-1)}^{0-80}$ indicates whether the salesperson has achieved less than 80 percent of the monthly goal and so on. Thus, we regress the natural logarithm of daily volume on a set of dummies that capture the degree of goal achievement until the previous day. To create these dummies, we first compute the ratio of cumulative sales in the period t up to the day $d-1$ over the goal for the period t (this ratio is equal to 0 on the first day of the month). We then create six dummies that capture when this variable is between 0% and 80%, between 80% and 90%, 90% and 100%, 100% and 110%, 110% and 120%, and at more than 120%. To

estimate the model, we drop the first dummy between 0% and 80%. We add the fixed effects of day, period and salesperson-category.

This model captures how the level of goal achievement "at the start of the day" affects the daily effort decisions of salespersons. If ratcheting is an important consideration, we expect that $\gamma_6 < \gamma_5 < \gamma_4 < \gamma_3 < \gamma_2 < \gamma_1 < 0$; that is, that effort should "slow down" as they get closer to the goal and that this slowing down should become more pronounced as they surpass the goal.

The results are displayed in Table A1. We find evidence that is consistent with the presence of ratcheting. As cumulative sales approach the goal, sales slow: if the ratio of cumulative sales to goal is between 80% and 90% "at the start of the day", sales are 8.3% lower in the current day in comparison to the daily sales when the ratio was between 0% and 80%. If the ratio is between 90% and 100%, the reduction is 11.8%. This reduction becomes much more pronounced once the goal is attained: if the ratio is between 100% and 110%, the reduction in daily sales is 18.1%; if the ratio is between 110% and 120%, the reduction is 23.4%; once the ratio is beyond 120%, the reduction is 27.0%.

It could be argued that this gradual reduction might simply reflect the fact that, if goals are well set/calibrated (in the sense that they capture "true" customer demand) and the salespersons exert the same effort across all days of the month, sales would get harder to achieve after the goal is reached. There are two arguments against this. First, while this dynamic might be possible in settings where demand is discrete or "restricted" by a monthly budget, customer demand in our setting is continuous and there is no reason to expect that it changes dramatically according to the day of the month. Second, the estimates of v_d show that there is a pronounced "day of the month" effect, such that daily sales at the end of the month are higher than at the start; specifically, the last (first) four days have 15% higher (lower) sales on average than are made between days 5–26 (this is an effect frequently documented in the sales literature). Thus, there is room to move sales upward or downwards at the end of the month, which is when quota achievement tends to be reached.

Table A1. Impact of goal achievement on daily sales

Dependent Variable:	Ln(Volume)
Goal achievement 0%-80%	(omitted)
Goal achievement 80%-90%	-0.083*** (0.005)
Goal achievement 90%-100%	-0.118*** (0.006)
Goal achievement 100%-110%	-0.181*** (0.008)
Goal achievement 110%-120%	-0.234*** (0.013)
Goal achievement 120% or more	-0.270*** (0.017)
Salesperson-category fixed effects	Yes
Period fixed effects	Yes
Day of the month fixed effects	Yes
Observations	507,152
R-square	0.48

A.2 Level and Log-Log Specifications

Preliminary Box-Cox specification tests show that a log-level specification provides a better fit to the data than a specification in levels or a log-log specification for the baseline model.

In the interest of transparency, we show in Table A2 that the choice of a linear regression model does not affect the nature of our results. In more detail, Table A2 compares the results from the estimation of the benchmark model using a level and a log-log specification with the results in the baseline regression. The results show that there are some minor differences in significance and the magnitude of some coefficients, but none of these affects the main result: For all specifications, the coefficients that represent the effect of eliminating supervisor ratcheting have the same qualitative effects: They display the same signs, similar magnitude and estimation precision.

Table A2. Linear, Log-Level and Log-log Regressions for Total Sales

	Model (1): Log-Linear			Model (2): Linear			Model (3): Log-Log		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.191	0.154	-1.24	-26.007	14.129	-1.84	-0.112	0.083	-1.35
Ratcheting*Post	0.556	0.163	3.41	57.800	15.149	3.82	0.316	0.076	4.14
Time-Varying Controls	Yes			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Obs.	4751			4751			4751		
R-sqr, within	0.30			0.24			0.40		

Note: Workers in the SBUs switch from ratcheting to automatic goal setting in August 2016. The regression sample includes data for the period January-December 2015. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy and it is estimated following the methodology in Brahm and Poblete (2018) using the data for the period January-December 2015. The dummy "Post" represents the change in compensation policy that takes place in August 2016. Time-varying variables of the salesperson include tenure, supervisor tenure, unionization status, residential area dummies, type of employment contract, and family status. The table reports robust standard errors.

A.3 Influential Observations

We investigate whether the observations from August 2016, which was a period of transition, influence the estimates of our benchmark model. Another issue that we need to investigate is whether observations associated with extreme levels of supervisor ratcheting, around 1 and 0, drive our benchmark results. Finally, we consider whether the estimates change if we restrict our analysis to the six months before and after centralization.

Specification (1) presents the estimates of our benchmark model as presented in Specification (2) of Table 3, excluding data from August 2016. The results show that the exclusion of the observations of August 2016 from the sample in the estimation of the benchmark model does not lead to any changes in the estimated coefficients in comparison with the benchmark estimates. In Specification (2), we estimate our benchmark model, excluding data from August 2016 and data associated with the top and bottom 5% of ratcheting. The estimated coefficients are similar to those in the benchmark model. Thus, we can conclude that the nature of our results does not depend on the inclusion or exclusion of these data.

In Specification (3), we exclude the data from August 2016, the observations associated with extreme ratcheting, and restrict the sample to a six-month period before and after August 2016. The estimated coefficients of this specification are very similar in terms of sign, size and significance to those in the benchmark Specification (2) in Table 3.

Based on these results, we conclude that our main results do not depend on including in the sample observations from the transitional month of August 2016 or observations where supervisors apply extreme levels of ratcheting. Moreover, the results show that the estimates of the effect of ratcheting and its elimination on sales do not depend on the end horizon of the study.

Table A3. Log-Level Difference-in-Differences Regressions of Total Sales: Sample Restrictions

	Model (1)			Model (2)			Model (3)		
Panel I	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.192	0.155	-1.24	-0.244	0.163	-1.49	-0.235	0.185	-1.27
Ratcheting*Post	0.567	0.164	3.45	0.697	0.178	3.92	0.614	0.184	3.33
Time-Varying Controls	Yes			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Subjective Ratcheting									
AESR	0.339	0.098	3.45	0.417	0.106	3.92	0.367	0.110	3.33
MESR	0.182	0.053	3.45	0.224	0.057	3.92	0.197	0.059	3.33
Obs.	4608			4486			2912		
R-sqr, within	0.32			0.32			0.32		

Note: The table reports the estimates of log-level difference-in-difference regressions of total sales for various subsamples: excluding data from August 2016 (1), excluding data from August 2016 and data associated with the top and bottom 5 percent of ratcheting (2), and finally using data for the period January 2016- January 2017 and the above two restrictions (3). Workers in the SBUs switch from ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy and it is estimated following the methodology of Brahm and Poblete (2018) using the data for January–December 2015. The dummy "Post" represents the change in compensation policy that takes place in August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

A.4 Results for Net Sales and the Profit Margin

CCU's ratcheting policies show that total sales are the primary outcome of interest. Nevertheless, there are alternative measures that reflect additional dimensions of worker performance. Our goal in this section is to investigate whether our conclusions about the effect of ratcheting vary with the measure of performance. We apply our empirical framework to the analysis of net sales and the gross profit margin. These measures shed light on additional aspects of the behavior of salespersons in the presence of ratcheting.

We obtain the net sales generated by a salesperson by subtracting discounts, promotions, and sales-related expenditure from total sales. Thus, net sales take into account the promotional policies that salespersons use to increase their total sales, which may be particularly relevant in periods of transition and rapid expansion. Another important measure of performance is the profit margin generated by each salesperson. In theory, this should be an object of interest at least as important as total sales, since it includes the costs of distribution, transport, etc. associated with the generation of a certain revenue from sales. Relatedly, we note that CCU's costs, especially transportation costs, are non-negligible and quite stable, which lends additional credibility to the observed profit margin as a valid measure of performance.

Table A4 reports the results from the estimation of Specification (2) of Table 3, using as the dependent variable the alternative performance measures presented above. It shows that for the SBUs the elimination of ratcheting has a positive and statistically significant effect on all alternative measures of performance. The estimated MESR are 16% and 21% for the net sales and the profit margin, respectively, while the AESR from the specifications for net sales and the profit margin are 32% and 42%, respectively, which are statistically close to the estimated effect for total sales of 39%. The similarity in the results for total sales, net sales and profit margins implies that salespersons do not engage in strategic behavior to expand their total sales through marketing practices that may undercut net sales or the profit margin.

Table A4. Log-Level Difference-in-Differences Regressions of Net Sales and Profit Margin

Panel I	Model (1): Net Sales			Model (3): Profits		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting	-0.162	0.131	-1.24	-0.257	0.140	-1.83
Ratcheting*Post	0.465	0.141	3.30	0.588	0.154	3.83
Time-Varying Controls		Yes			Yes	
Location, Period, Salesperson FE		Yes			Yes	
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Subjective Ratcheting						
AESR	0.278	0.084	3.30	0.352	0.092	3.83
MESR	0.149	0.045	3.30	0.189	0.049	3.83
Obs.	4751			4751		
R-sqr, within	0.41			0.42		

Note: The table reports the estimates of log-level difference-in-differences regressions of net sales and profits by salespersons. Workers in the SBUs switched from ratcheting to automatic goal setting in August 2016. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. The dummy "Post" represents the change in compensation policy that takes place in August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying variables of the salesperson include tenure, supervisor tenure, unionization status, residential area dummies, type of employment contract, and family status. The table reports robust standard errors.

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A.5 Ratcheting for the Sales SSU

As in the case of the SBUs, we estimate supervisor ratcheting following the methodology used by Brahm and Poblete (2018). Using the monthly data for the period January-December 2015, we estimate a panel data regression of equation (2). The results are presented in Figure A.5. The density distribution for the Sales SSU is similar to its counterpart for the SBU unit. The average ratcheting is 0.61, while the standard deviation is 0.37.

The origins of the heterogeneity in the observed ratcheting for the Sales SSU are more complicated than for the SBUs. According to the CCU management, goals were set subjectively until the end of 2014, and after that the algorithmic goal setting process—as represented by formula (1)—has been in place. Even though automatic ratcheting applied throughout the period January-December 2015 (using formula (1)), heterogeneity in the estimated ratcheting across supervisors (using equation (2)) emerges naturally even in the case of the Sales SSU. This variation comes from the variation in sales growth from periods $t-12$ to $t-1$, from $t-3$ to $t-1$ and from $t-2$ to $t-1$ which has sources at the salesperson and the supervisor level, as well as random shocks; Some simple calculations can clarify this.

Using a bit of algebra we can clarify the sources of variation. For simplicity, let us assume for now that equation (1) is

$$Goal_{ijt} = 0.25 Sales_{ijt-1} + 0.75\ Sales_{ijt-12}$$

(i.e., sales from $t-2$ and $t-3$ were assigned to $t-12$; the analysis could keep equation (1) unchanged without affecting the conclusion we obtain). Then this relationship holds:

$$Sales_{ijt-1} = \left(1 + r_{ij}^{t-12}\right) Sales_{ijt-12}$$

where r_{ij}^{t-12} is the rate of change in sales between $t-12$ and $t-1$ for salesperson i and

supervisor j. This rate can be modelled as follows:

$$r_{ij}^{t-12} = g + g_i + g_j + g_{ij} + e_i$$

where g is a common trend (e.g., overall economy or industry forces), g_i is a salesperson specific trend (e.g., learning by the salesperson), g_j is a supervisor specific trend (e.g., a change in sales given by the level of ratcheting of supervisor j in 2014, which after disappearing in 2015 may cause higher (lower) sales if ratcheting was high (low)), g_{ij} is a supervisor-salespersons match trend (e.g., novice salespersons do better with experienced supervisors) and e_i is a random shock on top of the previous structure (e.g., the route of salesperson i experienced some exogenous changes such as extensive migration). Some algebra shows that

$$\frac{Goal_{ijt}}{Goal_{ijt-1}} = \left(0.25 + \frac{0.75}{(1 + g + g_i + g_j + g_{ij} + e_i)}\right) \frac{Sales_{ijt-1}}{Goal_{ijt-1}}$$

If this is the data generating process for the estimation of equation (2) in the Sales SSU, then

$$\widehat{R}_J \cong 0.25 + \frac{0.75}{(1 + g + g_i + g_j + g_{ij} + e_i)}$$

(this expression can explain the shape of figure 2.2: positive skewness with a floor at ~ 0.30 and then uniform decay which, according to our simple toy framework, is generated by heterogeneous growth rates). Thus, $var(\widehat{R}_J)$ is a function of $var(g_i)$, $var(g_j)$, $var(g_{ij})$, and $var(e_i)$, all of whom are positive and potentially significant, and the covariances. There is a minimal value for the covariances, above which $var(\widehat{R}_J) > 0$.

However, more important than the explanation of where the variation comes from, is that these estimates of supervisor ratcheting for the Sales SSU for the period January-December 2015 should be irrelevant to the behavior of salespersons in the Sales SSU for the period of study, January 2016-August 2017, as it doesn't affect their expectation of ratcheting (which is driven by equation (1)).
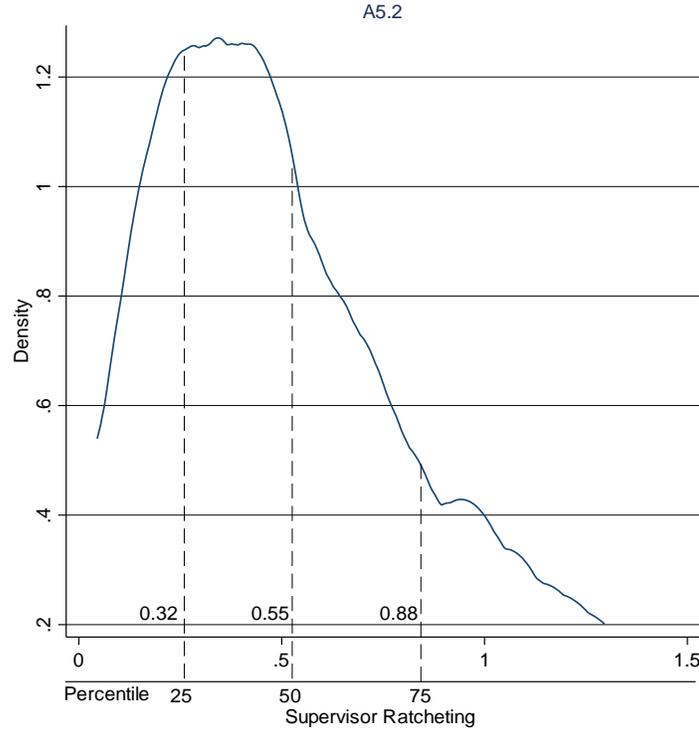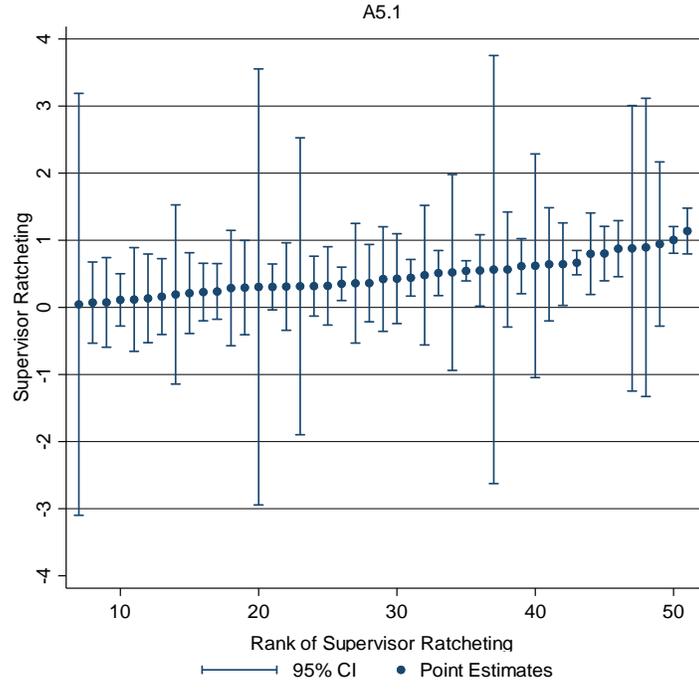
A5.1

A5.2

Figure A5. Panel A5.1 presents the estimated supervisor ratcheting for the Sales SSU, which we obtain following the methodology in Brahm and Poblete (2018). Panel A5.2 presents the density distributions of the estimated supervisor ratcheting for the Sales SSU.

A.6 A General Triple-Difference Specification

We estimate a general version of the triple difference specifications in which all variables are allowed to have distinct coefficients for the SBUs and the Sales SSU. Table A6 reports the associated estimates.

First, we discuss the results for the SBUs. The estimates in all specifications are very similar to their counterparts in Table 3 and 11. In all specifications, supervisor ratcheting has a similar effect to that in Table 3 and 11: negative but not statistically significant. The interaction between supervisor ratcheting, the indicators for the SBUs and the indicators for the change in goal-setting has a large, positive, and statistically significant effect on total sales. The interaction between the indicators for the SBUs and the indicators for the change in goal-setting has a negative sign. The results for the Sales SSU show that neither supervisor ratcheting nor its elimination and replacement by automatic goal setting affects sales.

Based on these results, under the most general Specification (3), the estimated AESR coefficient is 0.35, while the estimated MESR coefficient is 0.18. Thus, we conclude that our benchmark triple difference model and this more general specification yield similar estimates of the effect of ratcheting and its elimination on output for the SBUs and the Sales SSU. A formal F-test also shows that the more general specification yields no significant improvement in the regression fit.

Table A6. Log-Level Triple Differences Regressions of Total Sales, Generalized

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting*SBU	-0.253	0.152	-1.66	-0.191	0.154	-1.24			
Ratcheting*Post*SBU	0.499	0.167	2.99	0.556	0.163	3.41	0.523	0.190	2.75
Ratcheting*SSU	-0.087	0.049	-1.75	0.096	0.084	1.15			
Ratcheting*Post*SSU	-0.056	0.056	-1	-0.056	0.056	-1	-0.034	0.053	-0.64
SBU*Post	-0.322	0.141	-2.28	-0.130	0.455	-0.29	0.058	0.726	0.08
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		
Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Subjective Ratcheting									
AESR	0.373	0.111	3.36	0.390	0.110	3.55	0.355	0.126	2.82
MESR	0.189	0.056	3.36	0.198	0.056	3.55	0.180	0.064	2.82
Obs.	8105			8105			6238		
R-sqr, within	0.27			0.28			0.35		

Note: The table reports the estimates of a generalized triple-differences model of total sales, where the location and period dummies, as well as the time-varying controls, have separate coefficients for the SBUs and Sales SSU. Workers in the SBUs and Sales SSU switch from supervisor ratcheting to automatic goal setting in August 2016. Workers in the Sales SSU always operated under automatic goal setting. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. Ratcheting is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.

A.7 Testing for Heterogeneous Effects of Centralization

As discussed previously, the centralization event involved not only the switch from subjective ratcheting by supervisors to a transparent and uniform goal-setting for the salespersons in the SBUs, but also changes in many other organizational aspects, such as logistics, tasks, etc. So far, we have maintained that all these changes in the environment lead to a common shift in sales that salespersons in the SBUs experience, relative to the Sales SSU. However, it is conceivable that, although the changes are common for all in the company, their implementation varies from one supervisor to the next. In this appendix, we allow for a differential effect of centralization at the level of the supervisor by exploring the responses in a supervisor survey conducted after the centralization event.

Seven months after the centralization, the company conducted a supervisor survey, which included as one of the topics the costs of centralization. Specifically, the survey asked the supervisors from the former SBUs about their degree of agreement with a number of statements about various costs of centralization: whether centralization involved high reorganizational costs; whether the organizational culture affected the implementation of centralization; whether the centralization affected the relations with clients; whether the centralization affected the market fine-tuning; and whether the centralization affected organizational coordination. For each statement, the supervisors could express their level of agreement on a scale from 1 to 7, with 1 being "disagree completely" and 7 "agree completely".

We interact the responses of the supervisors with the dummy for the SBUs and the dummy for the period after the centralization event and add these new variables to each of the specifications reported in Table 11. We explore different ways to represent the responses of the supervisors. When we include every possible answer to every question in the regression, the precision of the estimates decreases due to multicollinearity between the extreme responses, 1 and 7, on the one hand, and the fixed effects for the supervisors who expressed these opinions. For this reason, we consider some alternative specifications. In one such specification, we group the responses 1 and 2 in the same category "disagree", the responses from 3 to 5 in the category "neither agree nor disagree", and the responses 6 and 7 in the category "agree".

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In another specification, we group the responses 1 to 3 in the same category "disagree", the responses from 4 to 7 in the category "agree". The results that we obtain under these alternative specifications are similar. Below, we discuss in some detail the results for the latter specification, also reported in Table A7.

The newly-added variables related to the reorganizational costs of the centralization, the effect of organizational culture on the centralization, the effect of the centralization on market fine-tuning, and the effect of the centralization on coordination are all not significant. The only exception is the response of the supervisors to the question about the effect of the centralization on the relations with clients. In Models (1) and (2), the coefficients associated with a disagreement with the proposition that the centralization affected the relations with clients are negative and statistically significant at the 5% significance level. When we introduce supervisor fixed effects, however, this effect disappears: The coefficient is close to zero and ceases to be statistically significant.

With respect to the variables of primary interest, we note that the introduction of the new variables does not lead to significant changes in the estimated average and heterogeneous effects of eliminating supervisor ratcheting: The AESR in specifications (1) to (3) are 0.34, 0.35, and 0.26, respectively, while the MESR in specification (1) to (3) are o.17, 0.18, and 0.13, respectively. These estimates are similar to their counterparts in Table 11.

Table A7. Effect of Centralization: Log-Level Triple Difference Regressions of Total Sales with Survey Data

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Ratcheting*SBU	-0.258	0.149	-1.73	-0.226	0.151	-1.49			
Ratcheting*Post*SBU	0.527	0.165	3.20	0.556	0.162	3.43	0.421	0.211	1.98
Ratcheting*SSU	0.034	0.095	0.36	0.016	0.097	0.16			
Ratcheting*Post*SSU	-0.004	0.070	-0.06	0.000	0.071	0	0.007	0.067	0.1
SBU*Post	-0.131	0.181	-0.72	-0.126	0.189	-0.67	0.154	0.458	0.34
(No Reorg. Cost)*SBU*Post	0.104	0.138	0.76	0.072	0.135	0.53	-0.148	0.301	-0.49
(No Cult. Cost)*SBU*Post	-0.057	0.120	-0.48	-0.080	0.117	-0.68	-0.029	0.120	-0.24
(No Rlns Cost)*SBU*Post	-0.257	0.121	-2.13	-0.244	0.120	-2.04	-0.027	0.195	-0.14
(No Adapt Cost)*SBU*Post	0.035	0.107	0.33	0.044	0.110	0.4	0.020	0.147	0.14
(No Coord Cost)*SBU*Post	-0.096	0.126	-0.76	-0.089	0.126	-0.7	0.102	0.157	0.65
Time-Varying Controls	No			Yes			Yes		
Location, Period, Salesperson FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Ratcheting									
AESR	0.339	0.118	2.80	0.354	0.116	3.04	0.264	0.133	1.97
MESR	0.172	0.060		0.179	0.059		0.134	0.067	
Obs.	8105			8105			8105		
R-sqr, within	0,27			0,28			0,43		

Note: The table reports the estimates of the log-level triple-difference regressions of total sales, defined by equation (5), that include a test for heterogeneous effects of centralization at the level of the supervisor. Workers in the SBU unit switch from supervisor ratcheting to automatic goal setting in August 2016. Workers in the SSU unit always operate under automatic goal setting. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy. It is estimated by applying the methodology in Brahm and Poblete (2018) to data for the period January-December 2015. The dummy "Post" represents the period after August 2016. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. AESR stands for average effect of supervisor ratcheting and MESR for marginal effect of supervisor ratcheting. "No Reorg. Cost" represents a supervisor's opinion that the centralization has no reorganizational costs. "No Cult. Cost" represents the opinion that organizational culture has no effect on centralization. "No Rlns Cost" represents the opinion that the centralization has not affected relations with clients. "No Adapt. Cost" represents the opinion that the centralization has not affected market fine-tuning. "No Coord. Cost" represents the opinion that the centralization has not affected coordination. The table reports robust standard errors.

A.8 Triple Differences Regressions with Ratcheting Quartiles

We consider a modification of the triple difference regression defined by equation (7) that replaces ratcheting by its quartiles. The goal is to investigate whether, relative to the Sales SSU, the effect of ratcheting on sales for the salesforce in the SBUs varies from one quartile to the next and whether these differences are statistically significant. The results are reported in Table A8. Apart from the replacement of ratcheting by its quartiles, the quartile specifications that we consider are the same as specifications (1), (2) and (3) in Table 11, with careful consideration of reference groups for coefficient interpretation. As discussed before in the context of equation (7), the difference between the corresponding coefficients for the SBUs and the Sales SSU identifies the effect of ratcheting on sales.

In contrast to the linear specification, the estimates of the common shift parameter β_1, are larger in magnitude and statistically significant in all specification: -0.35 in Specification (1), -0.39 in Specification (2), and -0.29 in Specification (3). As the parameter β_1 represents the reference anchor for the evaluation of the effect of eliminating ratcheting, these results imply that the salespersons who faced no or little ratcheting before August 2016 actually reduce their sales after the centralization; This is not surprising, since they actually experience an actual increase in goal ratcheting.

The estimates for the interaction of ratcheting quartiles with the indicators for the SBUs and for the change in goal setting after August 2016 are all positive and statistically significant: The coefficient for the second quartile is 0.59, for the third 0.48, and for the fourth 0.88. With respect to the estimated effect of eliminating subjective ratcheting, we obtain results similar to those in Table 11. This is not surprising because for the Sales SSU all the interactions between the ratcheting quartiles and the indicator for the change in goal setting are not statistically significant. Specifically, we find that the effect of eliminating supervisor ratcheting for the second, third and fourth quartiles are 0.53, 0.78 and 0.86, respectively. These estimates are all statistically different from their counterpart for the reference category, the first quartile. We also note that the effect of eliminating supervisor ratcheting monotonically increases, but at a decreasing rate across the quartiles.

These results imply that the causal response decreases from 0.53 to 0.25 and 0.08, where the first two are statistically different from zero. Thus, the greatest impact occurs when salespeople move from no ratcheting to some ratcheting, while moving from average ratcheting to high ratcheting has little or no effect. On average, moving from one quartile to the next leads to an output reduction by 0.28 or, in percentage terms, by 34%.

Table A8. Log-Level Triple Differences Regressions for Log(Total Sales) with Ratcheting Quartiles

Panel I	Model (1)			Model (2)			Model (3)		
	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
(Q2 Ratcheting)*SBU	-0.448	0.149	-3	-0.420	0.145	-2.9			
(Q3 Ratcheting)*SBU	-0.360	0.162	-2.22	-0.340	0.156	-2.17			
(Q4 Ratcheting) *SBU	-0.630	0.197	-3.2	-0.574	0.195	-2.94			
(Q2 Ratcheting)*SBU*Post	0.579	0.166	3.5	0.586	0.159	3.7	0.515	0.123	4.2
(Q3 Ratcheting)*SBU*Post	0.455	0.165	2.77	0.486	0.155	3.13	0.481	0.108	4.46
(Q4 Ratcheting)*SBU*Post	0.863	0.181	4.77	0.877	0.176	4.99	0.685	0.137	4.99
SBU*Post	-0.354	0.148	-2.38	-0.389	0.142	-2.73	-0.291	0.096	-3.01
(Q2 Ratcheting)*SSU	-0.079	0.134	-0.59	-0.111	0.142	-0.78			
(Q3 Ratcheting)*SSU	0.205	0.093	2.22	0.213	0.094	2.26			
(Q4 Ratcheting)*SSU	0.006	0.126	0.04	-0.015	0.129	-0.11			
(Q2 Ratcheting)*SSU*Post	0.068	0.083	0.82	0.053	0.083	0.65	0.038	0.086	0.45
(Q3 Ratcheting)*SSU*Post	-0.296	0.095	-3.12	-0.297	0.095	-3.13	-0.314	0.102	-3.07
(Q4 Ratcheting)*SSU*Post	0.014	0.085	0.16	0.017	0.086	0.19	0.021	0.082	0.26
Time-Varying Controls	No			Yes			Yes		
Salesperson, Period, Location FE	Yes			Yes			Yes		
Supervisor FE	No			No			Yes		

Panel II	Coef.	Std. Err.	t	Coef.	Std. Err.	t	Coef.	Std. Err.	t
Effect of Eliminating Subjective Ratcheting (Ref: Quartile 1, SBU*Post)									
Quartile 2	0.511	0.181	2.83	0.533	0.173	3.09	0.477	0.140	3.40
Quartile 3	0.751	0.190	3.95	0.783	0.182	4.30	0.795	0.149	5.35
Quartile 4	0.850	0.200	4.25	0.860	0.196	4.38	0.664	0.158	4.20
Obs.	8107			8107			8107		
R-sqr, within	0.28			0.29			0.46		

Note: The table reports the estimates of log-level triple-differences regressions of total sales that uses the quartiles of supervisor ratcheting. Workers in the SBUs switch from ratcheting to automatic goal setting in August 2016. Workers in the Sales SSU operate all the time under automatic goal setting. Ratcheting represents the subjective ratcheting of the supervisor prior to the change in compensation policy, and it is estimated following the methodology in Brahm and Poblete (2018) using data for January-December 2015. It is estimated using data for the period January-December 2015. The dummy "Post" represents the change in compensation policy that takes place in August 2016. Qx Ratcheting represents the x quartile of the distribution of ratcheting for the SBUs. The first quartile of ratcheting is the reference category. Time-varying controls include salesperson tenure, supervisor tenure, unionization status of salespersons, classification of employment contract, and family status. The table reports robust standard errors.