# Employee Responses to Compensation Changes: Evidence from a Sales Firm

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# Employee Responses to Compensation Changes: Evidence from a Sales Firm

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What are the long-term consequences of compensation changes? Using data from an inbound sales call center, we study employee responses to a compensation change that ultimately reduced take-home pay by 7% for the average affected worker. The change caused a significant increase in the turnover rate of the firm's most productive employees, but the response was relatively muted for less productive workers. On the job performance changes were minimal among workers who remained at the firm. We quantify the cost of losing highly productive employees and find that their heightened sensitivity to changes in compensation limits managers' ability to adjust incentives. Our results speak to a driver of compensation rigidity and the difficulty managers face when setting compensation.

## 1. Introduction

How will full-time employees respond to unanticipated, adverse compensation changes? Will highly productive workers respond differently than their less productive peers? Can employee responses impact firm performance? When tasked with adjusting compensation, managers balance incentive and retention effects with expense reductions, all while limiting the damage they cause to implicit relational contracts with their employees. Given this difficulty and the uncertain responses of workers, managers largely avoid imposing adverse compensation changes.

The literature has surfaced myriad negative worker responses to explain managers' reluctance to reduce compensation. Behavioral reasons focus on fairness concerns (Fehr et al. 2009, Cohn et al. 2014a) or social preferences such as 'warm glow' and social norms (DellaVigna et al. 2016). Other work documents increases in theft and antisocial behavior following unanticipated pay cuts (Greenberg 1990, Giacalone and Greenberg 1997). The research most related to ours emphasizes the importance of compensation practices that retain top talent. For example, Zenger (1992) finds that disproportionately rewarding high performers contributes to their retention, suggesting that managers' ability to revise compensation depends critically on their most productive employees' responsiveness.

The need to retain high performers clearly influences compensation structure, but experiments with compensation in real employment relationships are rare, and an open question is how the compensation-retention sensitivity varies over the distribution of worker performance. The underlying issue is that within-firm performance differences across workers can be significant (Lazear 2000, Mas and Moretti 2009, Lazear et al. 2015, Sandvik et al. 2020). If the most productive workers are also the most responsive to adverse compensation changes, then average turnover rates do not adequately capture the full impact on firm performance due to the loss of exceptional talent. This is consistent with Bewley (1998), who conducted over 300 interviews to understand why firms were reluctant to cut pay, even in the face of falling customer demand. Bewley states: "[turnover] among the better workers is especially feared, because they are more valuable and can find new jobs more easily."

We show that the cited concerns of managers are consistent with the responses of an organization's highly productive workers; they quit in response to a reduction in take-home pay. Our empirical setting is a US-based inbound sales call center. The president of one of the six divisions (henceforth Division 1) independently decided to re-balance the division's commission schedule, which led to an 18% decrease in expected commission pay and a 7% reduction in total take-home pay. The realized pay reductions closely matched these expectations. Three months after these changes, the president of a second division enacted similar changes, which led to a decrease in average take-home pay of over 14%.

To study heterogeneous responses across the employee performance distribution, we use worker-level output data, given to us by the firm, to estimate individual workers' sales productivity prior to the compensation changes. Individual productivity is widely dispersed, e.g., workers at the 75th percentile of the distribution sell about 50% more on a given call than those at the 25th percentile. This large dispersion motivates our investigation of the turnover and effort responses across the worker productivity distribution.

We use three empirical approaches to estimate worker responses. First, we begin with a traditional difference-in-differences estimation, where we compare workers in Division 1, before and after the compensation changes, to workers in untreated control divisions. Importantly, about two months before the compensation changes, Division 1 and the control divisions satisfied the difference-in-differences common-trends assumption. Our second empirical strategy mitigates concerns about long-term trend differences across divisions by focusing on heterogeneous responses for agents of different productivity levels *within* the same division. Our third approach uses survey responses to complement our main results and surface potential mechanisms.

Our main findings point to the importance of the most productive workers' heightened responsiveness to pay changes, as the turnover rate of highly productive agents in Division 1 increased significantly following the compensation changes. Specifically, workers with pre-treatment productivity that was one standard deviation above the mean had between a 40%–56% increase in attrition, relative to the baseline turnover rate. The average attrition rate of workers in Division 1 did not change, however, as less productive workers decreased their propensity to leave the firm. The loss of human capital from highly productive workers—who contribute significantly more to revenue than their colleagues—had significant consequences for overall profitability. Despite initial savings on compensation expenses, the loss of highly productive agents reduced the firm's operating performance and led to a negative estimated net present value of the changes. The second compensation change, which occurred in Division 2, validates these results. Division 2 contained only veteran, highly productive workers, and after their compensation was adjusted, the turnover rates of these extremely productive workers increased substantially relative to other divisions.

Turnover is rarely instantaneous. When examining Division 1, it took a little over four months for the cumulative loss in sales to outweigh the savings from the reduction in commission payments that resulted from the changes. Of equal importance, we observed virtually no abnormal attrition in the six weeks immediately following the compensation changes—highlighting the fact that workers did not respond to the announcement of the compensation changes by quitting *immediately*. This delay in the onset of turnover allows us to understand how job performance was impacted by the compensation changes, including for those workers who ultimately left the firm.

We find minimal evidence that agents responded to the compensation changes by adjusting their effort. If anything, Division 1 agents may have tried to increase their effort to offset some of the income lost due to the changes in their commission schedule. At first glance, this finding appears inconsistent with basic agency theory in static settings (Jensen and Meckling 1976, Hölmstrom 1979) as well as with more recent behavioral theories. However, in long-term employment relationships, workers' responses are impacted by income effects, where the desire to offset a portion of lost earnings may offset the desire to reduce effort in response to lower-powered incentives (Ashenfelter and Heckman 1974, Stafford 2015).

Our results underscore the importance of how compensation policy and performance heterogeneity interact in long-term employment relationships. We find that high performers have the greatest turnover sensitivity to compensation reductions, likely due to their superior outside options. High performers' responsiveness to compensation changes is consistent with managers' stated reasons for the rigidity of compensation contracts observed in aggregate data. Our results predict that managers will have more flexibility under labor market institutions that allow compensation policy to be tailored to individual workers.

## 2. Related Literature and Potential Mechanisms

The two most relevant strands of literature for understanding the relationship between compensation and employee effort and retention are behavioral theories and neoclassical economic foundations. Behavioral theories tend to be tested in short-term settings and focus on effort or output changes in response to compensation changes. Different theories emphasize (wage) fairness (Fehr et al. 2009), social comparisons (Larkin et al. 2012, Cohn et al. 2014b, Obloj and Zenger 2017), and negative reciprocity (Fehr and Falk 1999, Dickson and Fongoni 2018). There is some empirical support for these mechanisms in field experiments, showing that wage reductions reduce output (Kube et al. 2013) or cause attrition from short-term contractual work (Chen and Horton 2016).

By contrast, we find minimal changes in output or effort. The responses we do document, especially on the turnover margin, appear consistent with workers' optimizing their decisions according to their economic interests. We believe there are two main differences between our results and past studies. First, we focus on a long-term employment relationship where the overall change in incentives did not just alter the marginal return to effort but also affected workers' overall income levels. Despite this, past work on long-term employment relationships does show evidence of shirking or reduced production quality in response to perceived insufficient pay raises or unfair compensation (Krueger and Mas 2004, Mas 2006). A key difference in our setting is the presence of performance pay, where workers have an incentive to try and make up lost income with higher effort. Indeed, prior work has shown that piece rate contracts, or contracts with commissions, may have different incentive effects than adjustments to fixed wages (Esteves-Sorenson 2017).

Our work also contributes to a growing literature on how compensation influences employee retention. The increased turnover rate of highly productive agents in our setting aligns with findings in Krueger and Friebel (2018), who study a reduction in incentive pay at a personnel search firm. They also document changes in output, but the firm they studied increased fixed wages to offset part of the reduction in incentive pay, potentially alleviating income effects and giving rise to effort reductions. Several other studies have considered the ability to attract or retain workers through compensation policy, many of which focus on the effects of stock options (Oyer 2004, Oyer and Schaefer 2005, Aldatmaz et al. 2014). Larkin and Leider (2012) examine how different menus of incentive schemes lead to the selection of sales workers based on their confidence, suggesting convexity in pay helps select highly motivated employees. Campbell et al. (2012) show that high performing lawyers (as measured by their earnings) are less likely to turnover, and Carnahan et al. (2012) show firms that tilt compensation toward high performers show lower turnover at the top of the distribution. We complement these papers by studying employee behavior under different compensation regimes.

Our results on turnover have implications for understanding the link between changes in compensation and monopsony power. The fact that the average worker often does not respond to compensation adjustments is thought to indicate monopsony in the labor market (Manning 2003, Dube et al. 2018). Although the average turnover response to a significant compensation change is negligible in our setting, the increased turnover of highly productive employees caused the change to be NPV negative. Accordingly, average turnover rates alone are insufficient to infer whether a firm benefits from exercising labor market power. The remainder of the paper details the setting, our empirical strategy, and provides further discussion of results and limitations.

## 3. Firm Setting and the Compensation Changes

The compensation changes that we study occurred in a US-domiciled, inbound sales call center. It employed over 2,000 sales agents over the course of our sample period in two main offices and a third smaller office. The agents are organized into six divisions, based on the goods and services (henceforth, products) they sell. The presidents of two different divisions, Division 1 and Division 2, drastically changed the commission schedules of the agents in their divisions, which ultimately led to significant decreases in the average commission and take-home pay of their workers. We briefly provide context here and relegate further details to Appendix A.

### 3.1. Firm Setting

The firm contracts directly with national television, phone, and internet providers to market and sell their products. The different sales divisions are uniquely characterized by the products their agents sell. These divisions are overall similar, with the exception of Division 2, as these agents respond to inquiries from small businesses, rather than residential customers. The firm reserves space in Division 2 for its most productive and experienced agents due to the higher profitability associated with small business customers. Division 1 and Division 2 employed 20% and 7% of the firm's sales force, respectively.

An agents' task is to respond to customer needs and to upsell high profit margin products, when appropriate. Sales opportunities are randomly assigned to agents within a division through a queue that assigns agents to calls, making it possible to estimate individual agent productivity after observing a large number of calls for each agent.

### 3.2. Agent Compensation

Agent pay is made up of a fixed hourly wage, commissions, and occasional small bonuses. New agents have a base hourly wage of approximately 150% of minimum wage, which increases by about \$1 per hour annually. Commissions are a significant part of an agent's total compensation. During the eight weeks before the compensation changes occurred, the average Division 1 agent earned \$318 per week in commissions, and the average control division agent earned \$201. These amounts constituted approximately 30%–40% of agents' overall take-home pay. The president of each division has sole discretion to adjust their sales agents' commission schedules.

The mapping of products sold by an agent to the commission pay received by the agent—i.e., the commission schedule—is determined as follows. Each product has a transfer price assigned to it by the division president, which the firm refers to as "revenues." These revenue amounts approximate the actual top-line revenue generated for the firm through the sale of the product. For instance, a low-end cable TV package may be assigned a revenue of \$50, while a high-end package may be assigned a revenue of \$200. These amounts form the basis for which agents receive commissions, and division presidents set them in a way that (1) rewards agents for each sale they make and (2) provides greater rewards for selling high profit margin products.[1] Throughout the week, agents generate revenue through each sale they make. At the end of the week, these revenue amounts are summed and multiplied by the agent's commission rate, which is a function of the agent's audited call quality and selling efficiency relative to other agents.[2] The product of agents' weekly revenues and commission rate determines the weekly commission payment.

### 3.3. Changes to the Commission Schedule

In November of 2016, the president of Division 1 radically re-calibrated their agents' commission schedule by changing the transfer prices for the products sold by agents. The commission rate function was not changed, meaning that agents still made the same percentage of revenue on any sale as determined by their relative selling efficiency and call quality, but the revenue amount itself was altered. Prices for customers remained unchanged, as did the per-product top-line accounting revenues realized by the firm. Figure 1 gives an example of changes in the commission schedule for two different types of internet packages.

The Division 1 president tilted the revenue schedule toward high profit margin products—suggesting that agents could earn more by selling these products—but acknowledged that the changes would lead to an overall decrease in commissions. Though management framed the changes as an opportunity for the workers to earn more, survey evidence in Section 3.6 indicates that the agents were aware that they were likely to take home significantly less pay due to the changes. We

[1] Upstream service providers pay the firm for every sale in accordance with set contracts, which leads to the top-line revenue generated for the firm. All use of the term "revenue" in this paper refers to the transfer prices the firm uses to incentivize agents.

[2] Every agent has a fixed number of calls audited each week. If any conduct violations are identified, the agent's weekly commission rate is reduced. Selling efficiency is based on revenue-per-call (RPC) and revenue-per-hour (RPH). Being in higher quintiles of RPC and RPH increases an agent's commission rate.

estimate that the commission schedule changes would reduce the commission pay of the average Division 1 agent by 18%, holding fixed the pre-treatment period mix of products sold.

According to the firm's management, the changes to the commission schedule were intended to decrease the relatively high commission pay levels that Division 1 agents were earning in the months before the changes. These relatively high commissions were caused by the addition of new territories from which Division 1 agents fielded calls. The inclusion of these new territories (henceforth, the territory shock) significantly increased the average commissions of Division 1 agents. Figure 2a shows the evolution of average commissions by division before and after the commission schedule changes. The pre-treatment period, Week -26 to Week 0 (with Week 0 denoting the week before the commission schedule changes), is separated into three periods around the territory shock. The weeks before Week -16 constitute the pre-territory shock period. The territory shock period runs from Week -16 to Week -8, representing the period of increasing commission levels for Division 1 agents. The period from Week -8 to Week 0 makes up the post-territory shock period. Division 1 agents' average commission levels increased from $157 in the pre-territory shock period to $318 at the beginning of the post-territory shock period. The effects of the territory shock stabilized in the eight weeks before the commission schedule changes.

Because we learned of the impending commission schedule changes before they were announced, we followed the insider econometrics approach advocated by Bartel et al. (2004) and interviewed presidents and managers at the firm to assess their predictions for agent reactions. The president and managers in Division 1 believed agents' responses to the changes would be muted. Other leaders within the firm, however, expressed concern about increased turnover among affected agents. Few sales managers mentioned changes in effort, because, while the strength of incentives would fall, high-powered incentive pay would remain a significant component of agents' total compensation.

Despite managers' lack of focus on effort, agents did have discretion to meaningfully influence their sales. For example, managers reported that agents would often need to try several different approaches before successfully upselling a customer. A plausible way for agents to reduce their effort is by trying fewer approaches for selling high profit margin products and instead simply fulfilling orders for easier to sell products. Earlier attempts to increase sales with short-term incentives suggest—and subsequent academic experiments confirm—the ability for agents in this firm to adjust their effort (Sandvik et al. 2020). Another example of possible effort adjustment is reducing adherence to one's work schedule by taking more and longer breaks.[3]

Three months after the commission schedule changes occurred in Division 1, the president of Division 2 implemented similar changes.

[3] Although schedule-adherence is tracked by the firm, agents are only penalized if their adherence level dips below a threshold of 80%, and the average pre-treatment adherence level among Division 1 agents was 83%.

### 3.4. Personnel and Productivity Data

We identify the consequences of the commission schedule changes using highly detailed commission, personnel, and productivity data provided by the firm. Division 1 and the control divisions all have consistent data beginning in April of 2016. The sample is organized by agent-week and runs through June of 2017. This dataset covers 2,033 unique sales agents across 61 weeks, for a total of 39,944 agent-week observations. This dataset includes proxies of worker effort—e.g., adherence, conversion rate, phone hours, average revenue-per-call (RPC), total revenue generated per week—demographic details—e.g., age, race, tenure, gender, marital status—and commission pay data. We refer to this as the immediate sample because it has detailed productivity and commission data in the immediate period surrounding the changes in Division 1. A larger sample, used to study turnover, contains data beginning in July of 2015, but it lacks information on sales productivity. We refer to this larger sample as the extended sample. Appendix B contains additional details about variable definitions and these samples.

Table 1 displays pre- and post-treatment period summary statistics for the control divisions (Divisions 3–6) in Columns 1 and 4, Division 1 in Columns 2 and 5, and Division 2 in Columns 3 and 6. The pre-treatment period is restricted to the post-territory shock period (Weeks -8 to 0) to highlight the division-level characteristics immediately before the commission schedule changes occurred. We fail to reject the null at the 1% level that Division 1 and control division agents are similar in Tenure ($p$-value = 0.758), Age ($p$-value = 0.336), Race ($p$-value = 0.887), and Gender ($p$-value = 0.496). During the pre-treatment period, agents in the control divisions and Division 1 were predominately male, 70%–73%. The average agent was 25–26 years old and had been working at the firm for about a year. We do find that agents in the control divisions are significantly more likely to be married ($p$-value = 0.001). Division 1 agents have higher adherence ($p$-value $<$ 0.001), but both groups are at or above the firm's mandatory level of 80%. Both groups spend a similar number of hours talking to customers each week ($p$-value = 0.132), though Division 1 agents realize higher commissions and greater revenue-per-call ($p$-value $<$ 0.001), largely due to the territory shock experienced two months before the compensation changes occurred.

Agents in Division 2 earn much more in commissions ($p$-value $<$ 0.001) because they sell to small businesses, rather than residential customers. For expositional ease and because we do not have the full range of performance variables for Division 2, we focus our analysis on estimating the changes in Division 1 relative to control divisions.[4] We discuss the effects of commission changes for Division 2 in Section 5.5.

[4] We cannot separate changes in sales from changes in effort in Division 2 because we lack product-level data with revenue transfer prices before and after treatment.

### 3.5. Estimating Baseline Agent Productivity

A central test of theories around heterogeneous turnover, emphasized in the adverse selection discussions in Campbell III and Kamlani (1997) and Bewley (1998), concerns how agents of different productivity levels respond to a change in their compensation or employment contract. To identify these heterogeneous responses, we begin by estimating agents' productivity (fixed effects) prior to the commission schedule changes. To do this, we use a fixed effects regression for individual agents, controlling for tenure and division-by-time fixed effects. We extract the individual agent fixed effects and adjust them using a shrinkage procedure designed to limit the influence of measurement error, as in Lazear et al. (2015). These adjusted fixed effects are what we use for the pre-change measures of agent productivity. Additional details are provided in Appendix B.

Worker fixed effects from the per-period display wide baseline performance variability. For example, average revenue per call for Division 1 agents in the top tercile of agent fixed effects was over 50% higher than the revenue per call produced by agents in the bottom tercile. Table A.1 provides additional summary statistics for Division 1 in the pre-treatment period by splits of the sample into terciles of pre-change productivity. Agents in the top tercile have higher tenure, in line with the firm retaining highly productive workers. Demographic characteristics also vary across the adjusted worker fixed effects terciles; namely, workers in the highest tercile are older and less likely to be single.

The interpretation of our upcoming analysis would be muddled if the commission schedule changes affected high and low performers differently, due to their selling of different product mixes. We test for this possible confounding factor in Appendix B.2 and find that the expected percentage change in commissions was equal across agents in the three terciles of fixed effects in Division 1.

### 3.6. Surveys of Sentiment and Reactions to the Changes

We conducted a firm-wide survey before the announcement of the changes to gather information regarding agents' sentiment toward the firm. We asked sales agents from all divisions the following three questions: (1) "How likely are you to agree with the following statement: [the firm's] policies, for example on adherence, compensation, and promotion, are justified and fair?" (2) "Suppose your friend is looking for a job, how likely are you to recommend them to apply at [the firm]?" (3) "Do you think you will be promoted in the future?" In addition, we conducted a follow-up survey among agents in Division 1 after the announcement of the changes and before these agents received their first paycheck under the new commission schedule. We asked the same three initial questions and several additional questions related to their perceptions of the commission changes. Additional details of these surveys are provided in Appendix B.3.

Responses to the follow-up survey conducted after the announcement reveal that the average agent in Division 1 expected their commission pay to decrease by 13% (see Figure 3a), which approximates our estimate of an 18% decline in commissions. The average agent reported that they *would need to* work 11% harder in response to the changes to maintain their usual commission pay (see Figure 3b). Agents then reported that *they would*, on average, increase their effort by 7% in response to the changes (see Figure 3c). Agents' own responses to these last two questions suggest that effort may have actually increased, possibly due to income effects or the desire to maintain their prior earnings.

Several other questions on the follow-up survey asked agents about the motivation for the commission schedule changes. Over 75% of the agents felt the motivation for the changes was clearly communicated by management at the time of the announcement. When asked why the changes occurred, 42% responded with management thought "Sales reps were overpaid," and 40% responded with "[The firm] needs to make cutbacks to stay in business." The follow-up survey also provides evidence that the changes were unanticipated by the sales agents, as only 2% of the agents say they knew the details of the changes before they were announced.

## 4. Identifying Assumptions and Common Trends

We are interested in how the commission schedule changes impacted the turnover and effort of the affected agents. The presence of unaffected divisions motivates the use of a difference-in-differences estimation. Difference-in-differences relies on the assumption that treated and untreated groups follow a common trend in outcomes in the absence of the commission change. To assess the suitability of using other divisions as a control group, we consider trends across several variables. First, we provide evidence of common trends in the attrition rates of agents in Division 1 and the control divisions over many months leading up to the commission schedule changes. We then show common trends in several output measures, which proxy for effort. Finally, we show that agents in Division 1 and the control divisions follow common trends in commission pay after the territory shock and before the commission schedule changes.

To bolster confidence that the common trends assumption is satisfied, we show that there is no divergence in proxies for effort supply (adherence and conversion rates) or effort demand (call volume and phone hours) either before or after the commission change in Division 1. Given that these auxiliary productivity measures do not deviate across divisions suggests that the commission changes in Division 1 were not motivated by future knowledge of call volume changes or other coincident issues that would confound our analysis. That is, the smooth evolution of these measures across treated and control divisions suggests that potentially problematic trend divergence is unlikely in our setting.

### 4.1. Common Trends in Turnover

Our first outcome of interest is the turnover response of Division 1 agents. We graphically assess the pre-treatment trends in agent turnover between Division 1 and the control divisions. We use the Kaplan-Meier survival rate estimator, which plots retention rates over time, because it allows visualization of the cumulative nature of turnover. The survival rate estimator considers a starting point and then, from that time, displays the fraction of agents who remain at the firm. This allows us to detect when retention rates diverge and what fraction of the total beginning workforce is affected.

Figure 4 plots the survival rates for agents in Division 1 and the control divisions. To focus on heterogeneous turnover responses, we separately plot the survival rates of high and low performers. Figure 4a shows that highly productive workers in both Division 1 and the control divisions follow a similar trend in retention from Month -5 to Month 0. Similarly, low performers in Division 1 have survival rates that closely track those of low performers in the control divisions. The similarity of these survival rate trends suggests that agents in the control divisions provide a valid comparison group to estimate the turnover responses of agents in Division 1.

### 4.2. Common Trends in Effort

To evaluate the credibility of the assumption of common trends in effort, we estimate time-period differences between Division 1 and control divisions in an event study design and then plot the coefficients and confidence intervals (Fowlie et al. 2018, Cengiz et al. 2019). The functional form is:

$$y_{i,t} = \sum_t \delta_{i,t} \mathbb{I}(time = t) \times Div1_i + \beta_i Div1_i + \sum_t \lambda_t \mathbb{I}(time = t) + X_{i,t}\Gamma + \sum_j \gamma_j Div_j + \varepsilon_{i,t}. \qquad (1)$$

The main coefficients of interest are $\delta_{i,t}$, which capture differences in baseline time effects for agents in Division 1 relative to the common time effects for the control divisions, $\lambda_t$. The model also includes controls for location and agent characteristics in $X_{i,t}$ and division fixed effects, $Div_j$.

We find support for common trends in output-based proxies of effort. We discuss the main results here, but for formal detail on statistical tests and the graphical representations of these estimations, we refer interested readers to Figure 5. When examining adherence and conversion, two measures of effort supply, we fail to reject the null hypothesis of divergent trends in the pre-change period. We also cannot detect pre-change trend differences in call volume and phone hours, two measures of demand for worker effort.

One might worry that trends in effort demand might deviate after the commission changes if the treatment is correlated with managements' forecasts of how the environment might evolve. This does not appear to be the case. To show that call volumes and the amount of time spent working with customers do not change coincidentally with the commission schedule changes, we test that the point estimates are jointly equal to zero in the 8 weeks following the effective date of the change. We cannot detect any divergence in call-based measures of effort demand that occurred simultaneously with the commission changes in Division 1. Thus any change in worker output that we observe is not likely due to reduced call volumes or time spent talking to customers. This suggests the effects of the territory shock, that had previously shifted demand, were permanent and had stabilized in the two months preceding the commission schedule changes.

### 4.3. Common Trends in Commissions

Figure 2a shows that commission pay in Division 1 and the control divisions follows a common trend before the territory shock, despite differences in levels. The commission levels of Division 1 agents deviate from this common trend during the territory shock period, but they appear to level off in the post-territory shock period and again track the commission trends of agents in the control divisions. The implementation of the commission schedule changes again shocks the trend of Division 1 commission levels after Week 0, but the two groups appear to follow similar trends from Week 4 through at least Week 16. There is relatively little movement in the control divisions in the immediate aftermath of the Division 1 compensation changes, suggesting that the changes had limited spillover effects into other divisions. We further discuss tests that show limited spillovers to the control divisions in Appendix C.

In our setting, we expect common trends in commission pay levels after the territory shock. We focus on this eight-week period immediately prior to the commission schedule changes because we know that trends differed during the territory shock period. Figure 2b plots the coefficients, $\delta_{i,t}$, estimated using Equation (1), just as was done to assess the common trends in effort. We find evidence of common trends in commission levels when we plot the coefficients across time. The point estimates in Weeks -8 to -2 are all close to zero, and zero always exists within the 95% confidence intervals around these points. Furthermore, we fail to reject the null hypothesis that the coefficients jointly equal zero ($p = 0.82$). This result suggests that Division 1 and the control divisions followed common trends in commission pay levels in the two months before the commission schedule changes occurred. We overlay a plot of differences in brand-level call volume to show that the observed differences in commission levels after the commission schedule changes are not driven by brand-specific variations in call volume. Managers of the firm confirm that, in the absence of the commission schedule changes, agents in Division 1 would have continued to realize the high commission levels they enjoyed in the post-territory shock period.

The analysis suggests that trends in commission levels caused by the territory shock in Division 1 are not a major concern for our empirical approach. Instead, the most likely issue for interpreting estimates in light of the territory shock is the loss of "balance" between Division 1 and other divisions because the territory shock potentially changed agents' reference points or caused the job to become relatively more attractive than it had been beforehand. Because our counterfactual compares agents with better jobs and higher earnings to the control agents, our estimates of turnover and effort responses are likely lower bounds for the consequences that managers would otherwise anticipate when adjusting pay.

## 5. Results and Exploration of Mechanisms

This section details Division 1 agents' turnover and effort responses to the changes in their commission schedule. We also estimate the firm-level effects of the observed worker responses. We then consider the role of sentiment for our findings and discuss the turnover effects of Division 2 agents. Our empirical analysis is motivated by a theoretical model, which, for brevity, we discuss in Appendix D. The model provides context for our estimates by showing that whether a compensation change is profitable depends on (1) the cost changes that affect the firm's wage bill, (2) changes in workers' effort, and (3) changes in the composition of the workforce, due to asymmetric turnover based on agent productivity.

### 5.1. Turnover Responses

In Section 4.1, we introduced Figure 4a to show the common trends in attrition between agents in Division 1 and control divisions in the months before the commission schedule changes. This figure also shows that the survival rates of highly productive agents in Division 1 break from those of highly productive agents in the control divisions in the post-treatment period. This figure conditions on agents who were present at the firm several months prior to the commission schedule changes, which is useful as a diagnostic tool for pre-trends. Figure 4b, on the other hand, considers survival rates relative to the sample of agents present in each group in what is labeled Month 0 (October 2016), the calendar month immediately before the changes occurred. After the commission schedule changes, the survival rate of high performers in Division 1 decreases, relative to that of high performers in the control divisions, whereas the survival rate of low performers appears to increase. This is preliminary evidence of a heterogeneous turnover effect, wherein highly productive agents in Division 1 are more likely to leave the firm in response to the commission schedule changes.

Figure 6 presents how this differential turnover influences the composition of agents who remain by plotting the average z-score of adjusted worker fixed effects for Division 1 and the control divisions. As in many sales firms, there is positive selection by worker quality over time, captured by the upward trend in average adjusted worker fixed effects in all divisions in the pre-treatment period (represented by points to the left of the vertical line). There is then clear evidence that average worker quality begins to deteriorate in Division 1 several weeks after the commission schedule changes. By 24 weeks after the change, the average fixed effects for Division 1 fall by over 0.3 standard deviations relative to control divisions. This divergence in adjusted worker fixed effects provides graphical evidence that, in response to the commission schedule changes, agents with high pre-treatment productivity exited the firm at a higher rate than did agents with low pre-treatment productivity.

With this evidence in hand, we formally examine turnover by using a difference-in-differences estimator. These estimations use the extended sample, which includes additional data predating the immediate sample by at least a full calendar year for each division. Our goal is to identify how turnover was affected when agents' commission schedule changed and how the turnover probability differs based on agent productivity. In an analysis of turnover, it is necessary to account for how the baseline probability of leaving the firm changes with worker tenure (Bartel and Borjas 1981). We use a very flexible specification for how the usual probability of leaving the firm changes with tenure by including a flexible function, $g(Tenure)$, in the model. We specify this function as a fifth-order polynomial, providing enough flexibility to capture the possibility that workers with longer tenures are less likely to leave and that the relationship between attrition and tenure has several inflection points.[5] This function is distinct from time fixed effects, which are meant to capture calendar time shocks, like seasonality, that affect all workers. We then include combinations of division and time fixed effects to capture permanent heterogeneity across divisions and seasonal shocks that may be correlated with treatment. The model we estimate is:

$$\begin{aligned} Turnover_{i,t} = Div_j + \delta_1(Treated_i \times Post_t) + \delta_2(Treated_i \times Post_t \times Prod_i) + \beta_1(Prod_i) + \\ \beta_2(Treated_i \times Prod_i) + \beta_3(Post_t \times Prod_i) + TimeControls + g(Tenure) + \beta_4 X_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{2}$$

The dependent variable, $Turnover_{i,t}$, is an indicator that the week in question is worker $i$'s last week in the firm. After the worker leaves, he or she is no longer included in the sample. The dependent variable is thus the instantaneous turnover probability, or hazard, given that the worker was at the firm in the week in question. The parameter $\delta_1$ captures the average change in turnover probability of agents in Division 1, conditional on tenure and time controls, after the commission schedule changes occurred. This is indicated by $Post_t$, the post-treatment indicator, being interacted with $Treated_i$. We include division fixed effects, $Div_j$, to control for division-level differences in attrition. The matrix $X_{i,t}$ has a third-order polynomial in age, along with fixed effects for ethnicity, gender, call center location, and marital status. The separate tenure splines and age polynomials allow the effects of experience within the firm and total labor market experience to differ. We include baseline measures of worker productivity, captured by $Prod_i$, and its interaction

[5] Our results are little changed when using lower order polynomials, as shown in Table A.2.

with post-event indicators. To identify differences in productivity, we use the standardized $z$-score of adjusted worker fixed effects in the pre-treatment period. We use $z$-scores to standardize the adjusted fixed effects across Division 1 and the control divisions. This approach also facilitates the interpretation of the parameters, as a unit change in the $z$-score, $Prod_i$, corresponds to a standard deviation of the underlying productivity measure.

Table 2 displays the turnover responses of agents in Division 1, relative to those in the control divisions. The different columns correspond to different combinations of $TimeControls$ to account for a variety of possible temporal differences across divisions. Across all specifications, highly productive workers in Division 1 became more likely to leave after the commission schedule changes. The point estimates on *Treated x Post x Prod* across Columns 1–4 indicate that Division 1 agents with pre-treatment productivity one standard deviation above the mean had turnover rates that increased by 1.3–2.1 percentage points in a given week, compared to Division 1 agents with average pre-treatment productivity. This turnover increase is relative to an overall sample mean of about 3.7%, indicating that agents one standard deviation above the mean had between a 40%–56% increase in attrition from the sample average.

These turnover effects are precisely estimated when clustering by the identity of a worker's manager. If we instead cluster standard errors at the division level, which was the level of the treatment, our standard errors are similar. However, we do not report these standard errors, because test statistics based on them are misleading due to having few divisions and only one treated group. Instead we conduct robust statistical tests using a combined randomization inference and wild bootstrap procedure designed to estimate critical regions under clustering with few treated clusters (MacKinnon and Webb 2018).[6] The p-values from these tests are displayed in the bottom rows of Table 2 for $\delta_1$ and $\delta_2$.

Table 2 also includes placebo tests to assess whether the observed attrition patterns would have occurred at a different time. The most natural prior time to test is the exact date in the prior calendar year, and the specification in Column 1 includes placebo indicators that are dummies for the period one year prior to the announcement week. This tests whether the observed turnover effect is due to the commission schedule changes or annual patterns in turnover. The zero coefficients on *Treated x Placebo x Prod* and *Treated x Placebo* indicate that the turnover patterns overall and by agent productivity level did not diverge between Division 1 and the control divisions at the same time in the past.

[6] There is now a significant literature that addresses these issues, and applied papers have generally used some version of the wild cluster bootstrap to get valid confidence regions. See, for example, Lazear et al. (2016). This estimator has been shown to perform well in simulations and avoids problems of over-rejection that are often endemic when there are few clusters.

Columns 2 and 3 show that the estimates are robust to the inclusion of different combinations of time, week-of-year, and division fixed effects. The division by week-of-year fixed effects in Column 2 compares division-level turnover rates across calendar years, which captures possible seasonality by division and guards against the possibility that Division 1 had a similar seasonal change in turnover in the prior year. The point estimate of 0.015 suggests that workers plus or minus one standard deviation around the mean had post-treatment turnover rates of 5.2% and 2.2%, respectively. This difference provides strong evidence of a heterogeneous turnover response across the distribution of worker productivity. Column 3 includes division-by-time fixed effects, which only allows us to identify heterogeneous turnover by productivity. The advantage of these estimates is that we do not need to rely on common trends by division, only common trends in turnover by different productivity groups.[7] The similarity of the overall estimates in Columns 1 and 2 and those that do not depend on common trends at the division level (Column 3) add credibility to the identifying assumptions. This heterogeneous effect also holds when we restrict the sample to begin eight weeks before the commission schedule changes occurred (Columns 4 and 5), which removes the weeks before and during the territory shock period. The results are also robust to whether workers' productivity fixed effects include or omit their tenure. Taken together, this evidence suggests that highly productive agents in Division 1 were more likely to leave the firm in response to the commission schedule changes, as predicted by managers in the prior literature (Campbell III and Kamlani 1997, Bewley 1998).

We note that while high performers were more likely to leave the firm following the change, the average turnover rate was unchanged. The point estimates on *Treated x Post* are not precisely estimated in any of the specifications. The inability to reject that the main effects are zero for Division 1 indicates that overall turnover did not increase among these agents. Instead, only agents who were highly productive in the pre-treatment period increased their likelihood of leaving. This is consistent with the trends in survival rates, depicted in Figure 4b. This figure shows that high performers have an *increased* likelihood of quitting (i.e., a decreased survival rate), whereas low performers have a *decreased* likelihood of quitting. The departure of high performers potentially increased the placement of low performing agents in the selling efficiency quintiles used to determine commission rates. Consequently, the implicit contract improved for low performers with the attrition of high performers, increasing their incentive to stay in the firm. These offsetting effects provide intuition as to why we do not observe a significant average turnover effect among agents in Division 1.

[7] Heterogeneous responses can be estimated using division-by-time fixed effects without appealing to common trends across divisions. The maintained assumption here is common trends between different groups within each division. Figures A.1a and A.1b plot the evolution of within-division differences in performance by worker pre-treatment productivity, suggesting the validity of common trends within division.

The results from multiple additional placebo tests highlight the robustness of our turnover response estimations. Following Gubler et al. (2018), we perform fifty placebo simulations for the turnover response estimation using randomized treatment groups and treatment dates over agents. The coefficient for 47 of the 50 placebos run is smaller in size and less statistically significant than the estimated coefficient. This is approximately what one would expect from the placebo tests given the statistical significance of the estimate. We also repeat this procedure by randomizing treatment over different control divisions. In this setup, our estimate is larger than all placebo estimates. The results of these placebo estimates are displayed in Figure A.2a and Figure A.2b.

### 5.2. Effort Responses

Having found evidence of heterogeneity in the turnover of agents in Division 1, we next investigate whether these agents altered their effort in response to the commission schedule changes. The first specifications for estimating the effects of the commission schedule changes on worker effort are difference-in-differences regressions with the following form:

$$y_{i,t} = \alpha_i + Div_j + Trend_j + \delta_1(Treated_i \times Post_t) + \lambda_t + \beta_1 X_{i,t} + \varepsilon_{i,t}. \tag{3}$$

The model includes time (week) fixed effects, $\lambda_t$, and division fixed effects, $Div_j$. Some specifications include an individual fixed effect $\alpha_i$, and some include division-specific time trends, $Trend_j$. To account for the potential that different trends across divisions bias the estimates, we check the robustness of our results by using a propensity score re-weighting estimator to match control division agents who were on similar trends as those in Division 1 before the commission schedule changes occurred. This approach aims to better balance treated and control agents, based on levels of and changes in compensation over the entire pre-treatment period.[8] In addition, we verify our results by reducing the sample to a balanced panel of agents who are present in the sample before July 2016 and after April 2017. This ensures that we capture variation in agents' behavior before and after the commission schedule changes and not just changes in the composition of workers.[9]

The results of the difference-in-differences estimations using Equation (3) are contained in Columns 1–5 of Table 3. We use data from the eight weeks before and the eight weeks after the commission schedule changes to estimate agents' effort responses. In Table A.3, we show that our

[8] The details of this re-weighting procedure are provided in Appendix C.1. Figures A.3a and A.3b in the Appendix display the weighted and unweighted measures of log commissions-per-call and log commissions, respectively.

[9] Our estimation of changes in worker effort are conditional on the worker remaining at the firm. As turnover takes time to happen, however, we observe almost all treated workers with at least some sales data in the post-treatment period. The inclusion of agent fixed effects also partially addresses the concern that attrition could affect our measures of employees' effort responses. Importantly, the turnover responses that we discussed in Section 5.1 emerge several weeks (at least six) after the commission schedule changes occurred. We would expect effort responses to manifest much earlier, so it is unlikely that our estimates of effort responses are driven by abnormal attrition.

results are robust to the inclusion of all the pre- and post-treatment data. Panel A of Table 3 contains results for agents' adherence and shows that, on average, agents in Division 1 do not reduce their adherence in response to the commission schedule changes. We also find negligible differences in agents' conversion rates (Panel B). The null results in Panels A and B are robust to the inclusion of agent fixed effects (Column 2), the inclusion of division-specific time trends (Column 3), the use of a re-weighting estimator (Column 4), and the use of a balanced panel (Column 5). These findings align with the graphical evidence presented in Figures 5a and 5b in Section 4.2. They suggest that agents did not avoid calls nor did they reduce their sales conversion efforts; i.e., we find little evidence of effort adjustment after the commission schedule changes.

We further consider changes in agents' effort by considering two additional proxies of worker sales effort, log revenue-per-call if (1) the commission schedule had *not* changed (Panel C) and (2) if the commission schedule had always been at the new levels (Panel D). In these specifications, we take the revenue transfer prices as given, based on the respective commission schedule regime and apply these pseudo-revenues to the volume of products sold. We find minimal evidence of changes in log revenue-per-call at both the old and new revenue levels.[10] The positive estimates in Panel D suggest that agents might have increased effort after the commission schedule changes, potentially to compensate for income they stood to lose. However, this finding is not precisely estimated in any of the specifications.

We find limited evidence that agents were able to substantially shift from low- to high-margin products. If workers were substituting to higher margin products under the new commission structure, we would have expected to see substantial divergence between the results using the old and new revenue schedules in Panels C and D. Instead, both sets of estimates include 0 in the confidence intervals, suggesting minimal ability to substitute to higher margin products.[11] In some specifications, however, we are able to reject the null that the coefficients using new or old prices are the same. Specifically, in Columns 1 and 2 we reject equality at the 1% level, and in Column 3 we reject equality at the 10% level. However, in Columns 4 and 5, which use a re-weighting procedure and a balanced sample, respectively, we cannot reject equality at the 10% level. Comparing

[10] Our results are qualitatively unchanged when we measure productivity as revenue-per-hour, rather than revenue-per-call. The call based metric is the firm's focal measure and is more salient to sales agents and their direct supervisors, which is why we focus on it. The time based metric provides an interesting complement to this measure. Panels A and B of Table A.4 use the logarithm of revenue-per-hour as the dependent variable and report similar estimates as those in Table 3, suggesting a limited time-spent-per-call response to the compensation changes. This evidence aligns with the fact that agents have a limited capacity to control the total number of calls received each week. Panels C and D of Table A.4 use level RPC as the dependent variable.

[11] Similarly, we may expect that productivity would immediately decline as people learn to allocate effort and game new incentive plans (Obloj and Sengul 2012). In our setting, however, it is difficult to disentangle learning and other time trends because the compensation changes impacted all eligible agents simultaneously.

these differences, their estimated magnitudes are generally small.[12] Substitution to higher margin products would have implicitly reduced the magnitude of the compensation changes experienced by the agents. As a result, the relationship between the compensation changes and turnover that we estimate is likely a lower bound for the turnover that would have materialized absent product substitution.

A second specification identifies heterogeneous effort responses across agents, based on their pre-treatment productivity.[13] This specification includes interactions of productivity pre-treatment, $Prod_i$ with variables in the model in Equation (3):

$$\begin{aligned} y_{i,t} = \alpha_i + (Div_j \times \lambda_t) + \delta_1(Treated_i \times Post_t) + \delta_2(Treated_i \times Post_t \times Prod_i) + \\ \beta_1 Prod_i + \beta_2(Post_t \times Prod_i) + \beta_3(Treated_i \times Prod_i) + \beta_4 X_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{4}$$

Column 6 of Table 3 reports both $\delta_1$ and $\delta_2$ from Equation (4), capturing the fact that highly productive agents may have different effort responses on some dimensions. Column 7 identifies only the parameter $\delta_2$ by including division-by-time fixed effects in the model. We do not find a heterogeneous reduction in adherence across agents of varying productivity levels (Panel A), which suggests that neither high performers nor low performers responded to the commission schedule changes by avoiding calls or disregarding their schedules. In Panel B, we find that the conversion rates of highly productive agents decreased, relative to those of less productive agents. Similarly, the negative coefficients on *Treated x Post x Prod* in Panels C and D suggest that high performers may have reduced their revenue generation per call, relative to the average agent, but the effects are not precisely estimated.

A potential concern with the estimations of productivity changes is the possibility of mean reversion, which may be amplified in short time periods. Several empirical facts suggest a limited role for mean reversion in the productivity data we present, but we acknowledge the possibility. First, it is unlikely that mean reversion drives these results because Equation (4) accounts for this through $\beta_2(Post_t \times Prod_i)$. The parameter $\delta_2$ on $(Treated_i \times Post_t \times Prod_i)$ thus captures any deviation from natural agent productivity mean reversion in the post-period.[14] In addition, mean

[12] So the commission adjustments likely did not alter the firm's per-call unit economics, due to a substantial change in the composition of products sold. Additionally, these results are not driven by spillovers or reactions by agents in the control division, as discussed in the Appendix C.2.

[13] The heterogeneous treatment effects are based on standardized measures, so the average worker will have an effect that is captured by "Treated x Post" because the productivity average is zero. The interpretation for other workers requires multiplying by their productivity, which has a mean of zero and standard deviation 1, so the coefficients on these interactions reflect the effect of a standard deviation change around the mean.

[14] The relative reduction in the conversion of high performers is unlikely to be driven by mean reversion. Adjusted worker fixed effects—used to distinguish between high and low performers—are established using pre-treatment data up to four weeks before the changes occurred. The average conversion of agents in each of the three terciles of adjusted worker fixed effects increased from the weeks before this cutoff to the weeks after, suggesting mean reversion is not a

reversion would likely bias us toward finding substantial changes in effort because sales in Division 1 began at a higher level than in the control divisions, and we would thus expect Division 1 sales to fall under mean reversion. Given the modest size of the estimated sales reductions, we expect that mean reversion is unlikely to be driving these results. Taken together, the results in Table 3 imply that agents of all productivity levels had rather muted effort responses to the commission changes.

Finally, note that none of these results on effort report the wild cluster bootstrap randomization p-values, due to the general insignificance of the findings. Figure A.4 in the Appendix shows placebo tests where we repeat our effort estimation procedure using different control divisions as the chosen "treated" division. As the figure shows, we cannot reject the null of zero changes in effort when control divisions proxy for the treated division.

### 5.3. Implications For Profitability

Having estimated both the turnover and effort responses of agents in Division 1, we next estimate the overall return on investment stemming from the compensation changes. While the firm initially saved money as the result of paying fewer commissions in Division 1, over time the lost revenue from high performing agents who left the firm outweighed the initial compensation savings.

At the outset, the cost savings from the commission schedule changes looked attractive, saving the firm about \$0.68 in compensation expense per-call. Because turnover was minimal in the first few weeks after the changes, there was no offsetting reduction in revenue. However, about two months after the changes (eight weeks), the workforce composition effect reduced average revenue-per-call by \$0.58, compared to Week 8 labor cost savings of \$0.71. Over time, the decrease in the average revenue-per-call grew more quickly than the cost savings. About four months post-treatment (18 weeks) is the is inflection point where the change became unprofitable. Six months after the changes, the firm's gross margin per-call fell by more than 1.7 percentage points.

To put these numbers into context, we estimate the total net present value of the commission schedule change by multiplying the per-call numbers by the actual number of calls per week. Using just a six-month horizon, the present value of the commission schedule changes totaled negative \$75,500. We emphasize that this estimate likely understates the impact for the firm because we do not include the costs of training new hires. Additionally, our analysis does not consider the spillover effects associated with losing high performers. Previous work has shown that high-performing

likely cause of our findings. For example, top tercile agents in Division 1 had average conversion in September 2016 of 35%. In October, after the adjusted worker fixed effects had already been measured, this average increased to 37%. Across this same time horizon, bottom tercile agents maintained an average conversion of 29% and middle tercile agents increased their conversion from 32% to 33%. We cannot, however, disentangle whether changes in RPC and conversion rates are due to decreased effort or due to highly productive agents losing sales, as the result of aggressively trying to upsell.

employees are an important resource for raising the productivity of others (Sandvik et al. 2020), so the loss of highly productive workers likely had a deleterious effect on long-term productivity, beyond the six month horizon. We provide details behind these NPV calculations in Appendix B.4.

### 5.4. Commission Schedule Changes, Worker Sentiment, and Mechanisms

We now turn to additional evidence on the mechanism behind our results. We investigate three different questions regarding whether agents' sentiment toward the firm or perceptions of fairness can explain our findings. Prior to the announcement of the commission changes in Division 1, we surveyed agents from all six divisions about their perceptions of firm fairness, their willingness to give referrals, and their future promotion prospects. The exact wording of these questions is provided in Section 3.6. Shortly after the commission changes, we again surveyed agents in Division 1 to see how their answers changed.

How do these responses vary over the performance distribution? Do the highest performers (who eventually leave the firm) also have the most negative responses regarding whether the firm became less fair? Whether they would be less likely to refer others to work at the firm? The first row of Table 4 shows no significant changes in either high or low performers' perceptions of the firm's fairness. Instead, the second row reports that, across all terciles of pre-treatment productivity, agents reduced their reported willingness to refer others to work at the firm, but the decline was greatest among high performers. High-performers' 19.8 percentage point decline indicates a substantial reduction in perceived firm quality and is much larger than the 5.3 percentage point decline among the lowest tercile of agent productivity. The third row shows relatively small changes in agents' perceptions of their own promotions prospects. Based on these changes in survey responses, fairness channels have less support as a mechanism because perceived fairness reductions do not load differentially for the high-performing agents who ultimately leave the firm. Instead, high-performing agents have the largest reduced perception of the quality of their current job, presumably relative to other employment options.

We also investigate whether agents' turnover and effort responses vary with differences in pre-change survey responses. We use agents' pre-announcement responses, as we lack data on changes over time for control divisions. Among Division 1 agents where we can measure changes, Table A.5 shows that those who had the most positive responses prior to the change generally had the largest reductions after the change for each of the survey questions. We separately interact the treatment indicator with indicators for high firm fairness perceptions, high referral likelihood, and a belief that promotion is likely. We continue to find that highly productive workers in Division 1 increased their turnover rates after the commission schedule changes, relative to the average worker in Division 1. We do not, however, find any significant heterogeneity in treatment effects across responses to the three survey questions. The estimates are close to zero and are not statistically significant, as reported in Table A.6. A similar analysis for agent effort in Table A.7 reveals no evidence of statistically significant heterogeneity. Taken together, these results fail to find differential turnover and effort effects that function through ex-ante proxies for sentiment.

We caution, however, that these latter tests may be under-powered for ruling out fairness channels. While the survey evidence finds a limited role for the fairness explanation, there are several key limitations. First, agents may not have internalized the impact of the change at the time of the follow-up survey, as this survey occurred before the agents' first post-treatment paycheck. Second, the response rate of the follow-up survey is 30%, possibly inducing selection bias. Third, we cannot use changes in sentiment as the interactive variable of interest, as only Division 1 agents took the follow-up survey. Finally, the survey questions about fairness encompass many aspects of the job, not just pay considerations. Thus, while workers' fairness concerns do not appear to be driving our results, they cannot be definitively ruled out.

### 5.5. Effects of the Commission Schedule Changes in Division 2

For expositional ease, we deferred the discussion of the commission schedule changes in Division 2 until now. Division 2 changes allow us to test whether our main findings generalize. We begin our analysis of Division 2 by discussing the trends in commission pay levels for Division 2 agents, relative to control division agents, before and after their commission schedule changes. We then report the results from our difference-in-differences analysis of turnover responses. Finally, we corroborate these findings with graphical evidence that Division 2 workers increased their propensity to quit after the commission schedule changes. We do not consider the effort responses of Division 2 workers, as data limitations prevent us from measuring changes in revenue-per-call for this division.

Figure A.5a shows the evolution of average commission levels in Division 2 and the control divisions before and after the changes. As Division 2 agents sell products to small businesses, their sales and commissions are highly cyclical. Taking the average of Division 2 commissions prior to and after the change, commission pay fell from \$392 per week to \$309. In Figure A.5b, we again overlay a plot of call volume to show that the observed drop in commissions is not driven by a decrease in the number of calls.

The commissions change in Division 2 caused the average turnover rate to double, from a baseline of 0.83% per week to 2% per week (see Table A.8 with a coefficient of 0.013). Figures A.6a and A.6b corroborate the estimates by showing the Kaplan-Meier survival rates for agents in Division 2, relative to the control divisions. We do not detect within-division heterogeneous turnover responses in Division 2, but note that these agents are highly productive and highly compensated relative to the rest of the firm. Division 2 agents come from the right tail of the firm-wide productivity distribution, resembling the best agents in Division 1. Highly productive agents in Division 1 and all agents in Division 2 would have had similar outside options and similar incentives to search for other employment after their commission schedules changed, effectively reducing their take-home pay. This may explain why we observe a heterogeneous turnover effect in Division 1 and an overall increase in turnover in Division 2.

Our relatively limited data for Division 2 also prevents us from performing the same net present value calculation for Division 2 as we performed for Division 1 in Section 5.3. The Division 2 compensation changes were also likely NPV-negative in the long-term given the results from Division 1 and the large increase in attrition of Division 2 workers.

## 6. Concluding Discussion

The strategy and management literature has long studied the importance of incentive compensation in attracting and retaining workers, with a focus on top-talent (Zenger 1992, Campbell et al. 2012, Carnahan et al. 2012). We extend this literature by showing how workers of varying ability respond differently to incentive compensation *changes*. Specifically, we study effort and retention effects associated with a compensation change that reduced overall pay by 7% in an inbound sales call center.

We find that the most productive workers, those with pre-change productivity one standard deviation above the mean, increased their turnover likelihood by between 40%–56%. By contrast, other employees had minimal responses to the compensation changes. Apart from the attrition of high performing workers, we find limited changes in effort, sales revenue, and other on-the-job performance measures. The increased attrition of the most productive employees took several weeks to manifest, and we find no evidence that ex-ante worker sentiment about their jobs or the firm drove the increase in turnover. Instead, highly productive workers likely left the firm upon securing more attractive outside options, whereas their less productive peers were unable, or unwilling, to do so.

We find that the compensation change was ultimately unprofitable over the long-term. The change allowed the firm to reduce their payroll expenses but revenues decreased due to the subsequent departure of highly-skilled workers who were replaced by less experienced and less productive workers. Given that measured effort and productivity for retained workers did not change, while separation of highly-skilled workers occurred with a lag, analysis of average turnover rates alone, or looking at a relatively narrow time window, would have incorrectly concluded that the changes were profitable. Combined, these results highlight the importance of (patiently) measuring responses across the productivity distribution when one evaluates the net effects associated with incentive changes and—more generally—human resource management changes.

While our study firm provides an ideal setting to estimate heterogeneous worker responses to a compensation change, there remain important questions that we are unable to answer. First, our data is only from one firm, which limits our ability to determine if employees who leave are making optimal decisions. This is especially important in light of the firm-specific comparative advantage that employees would lose by leaving (Groysberg et al. 2008). Second, there are open questions about the role of performance pay in limiting employee effort responses. The extant literature on responses to compensation changes has largely focused on fixed or hourly wages (Fehr and Falk 1999, Dickson and Fongoni 2018), and generally finds that workes reduce output following compensation reductions. By contrast, workers in our setting incurred an adverse change to their performance-pay, which potentially explains the limited effort response in our setting. We conjecture that the lack of observed effort changes stems from a combination of income effects, income targeting, or reference points (Mas 2006). While we are unable to precisely isolate these mechanisms with the variation available in our setting, better understanding these mechanisms is important due to the growing share of workers receiving performance pay (Lemieux et al. 2009).

Our work also addresses the micro-foundations of downward nominal wage rigidity. By linking compensation changes with heterogeneous worker responses, our findings validate earlier surveys and interviews wherein managers report that the fear of losing top talent constrains them from adjusting compensation downward (Campbell III and Kamlani 1997, Bewley 1998, Kahn 1997).

Prior research has shown that contextual framing and communication matters for how individuals respond to changes in their environment (Kahneman et al. 1986, Chen and Horton 2016, Englmaier et al. 2017). Future studies might consider how workers' responses to externally motivated events might vary compared to the within-firm motivations examined here (e.g., pandemics or business cycle shocks, as examined in Mascarenhas and Aaker (1989)). A second area of future study is to apply our results more generally to the study of monopsony in the labor market. Existing studies tend to take the existence of a limited labor supply elasticity facing an individual firm as evidence that the firm can exercise labor market power. Given our results on worker heterogeneity, more work is needed to understand the conditions under which firms may exercise labor market power, or the contracts they would need to write with different workers in order to do so.

Our findings have two direct implications for managers. First, high performing employees are the most sensitive subgroup to adverse compensation changes. Therefore, while compensation changes may reduce payroll costs across all impacted employees, retention risks—and the subsequent costs—are greatest among top performers. This finding suggests insulating the most productive employees from adverse pay changes may be beneficial, albeit more research is required to understand the potential adverse effects of workplace inequality. Second, the presence of performance-pay may limit negative, on-the-job responses to adverse compensation changes via income effects, suggesting a potential pathway to avoid previously observed forms of behavioral and sentiment-driven reactions to adverse compensation changes.

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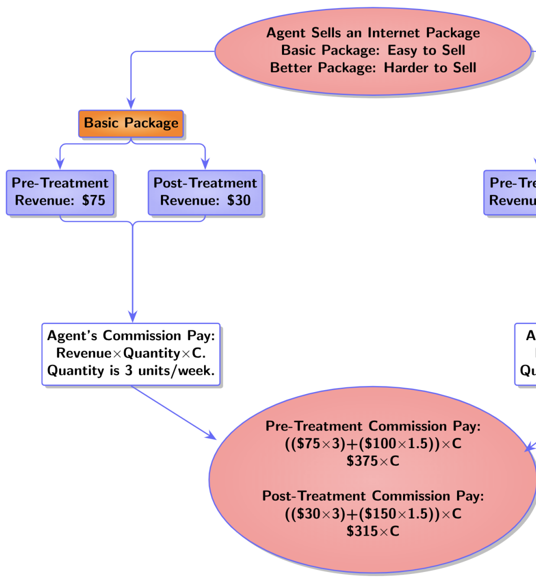
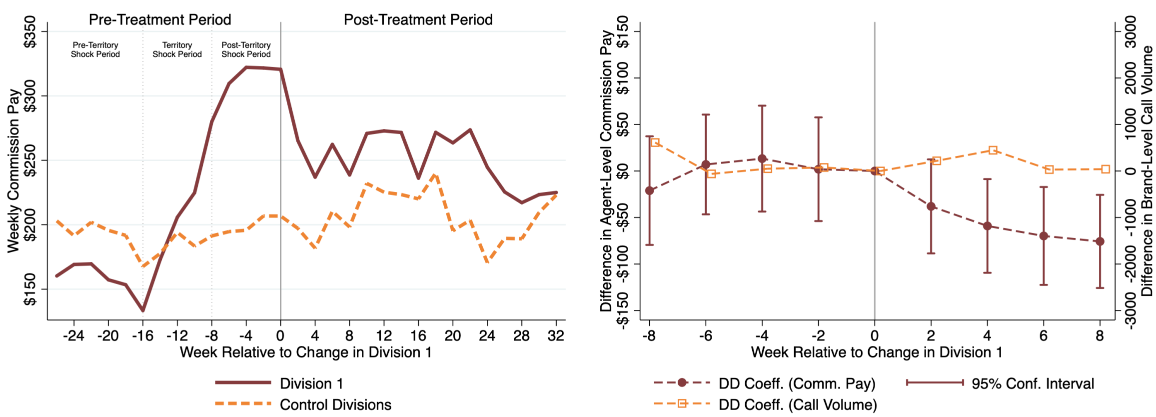
# Figures and Tables

**Figure 1 Revenue Transfer Price Changes Within the Commission Schedule**




*Notes:* This figure provides an example of changes in the commission schedule for two different types of internet packages. The pre- and post-treatment revenue transfer prices for the basic package are displayed in the left branch. The pre- and post-treatment revenue transfer prices for the better package are displayed in the right branch. A basic package is easier to sell than a better package, captured by the higher quantity of sales per agent-week, 3 vs. 1.5. The agent's commission rate, $C$, is multiplied by the product of the revenue transfer price and quantity sold to determine the amount of commission pay the agent receives for selling a particular package.

**Figure 2 Commissions in Division 1 and the Control Divisions**

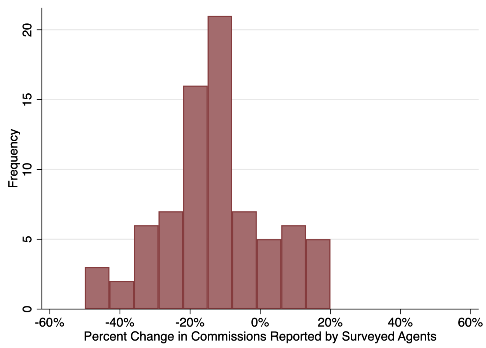



(a) Commission Level Time Series (b) Trends in Commissions and Call Volumes

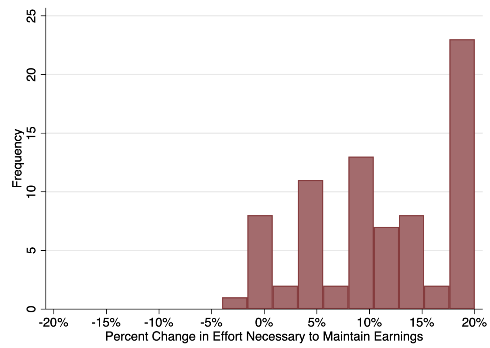
*Notes:* Figure (a) plots the average weekly commission pay levels for agents in Division 1 and the control divisions. The solid vertical line corresponds to the week immediately before the week of the commission schedule changes in Division 1. Figure (b) plots the difference-in-differences coefficients that capture differential trends in commission pay levels and total call volume between Division 1 and the control divisions in the post-territory shock period. We fail to reject the null hypothesis that the coefficients in Weeks -8 to 0 jointly equal zero (p = 0.82).

**Figure 3 Reported Changes in Commissions and Effort**

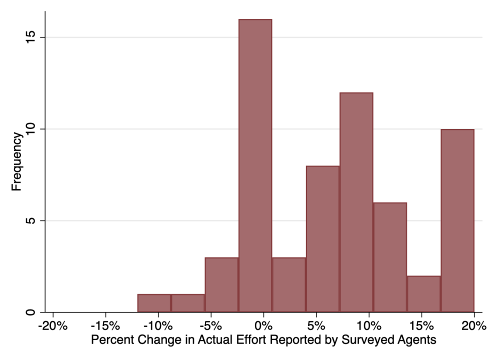
(a) **Expected Change in Commissions**




(b) **Effort to Maintain Earnings**

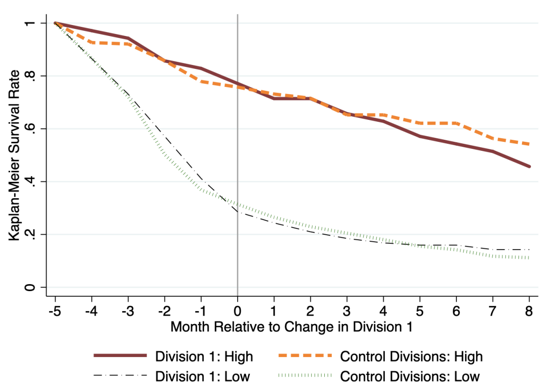


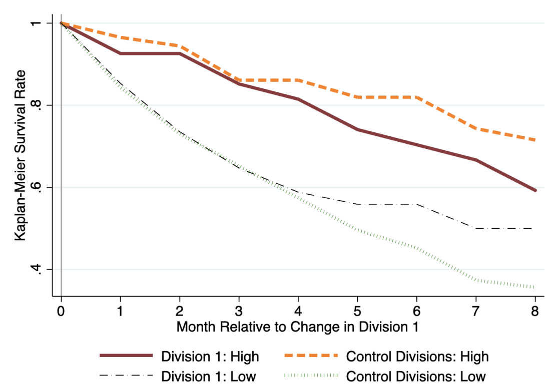

(c) **Actual Change in Effort**




*Notes:* Histogram (a) displays survey responses to a question asking agents what their expected change in commissions would be due to the commission schedule changes. Histogram (b) displays responses to a question asking how much agents' effort would need to change to maintain their normal level of earnings. Histogram (c) displays responses to a question of what changes in effort workers actually planned to make. See Section 3.6 for more details.

**Figure 4 Survival Rates By Productivity**

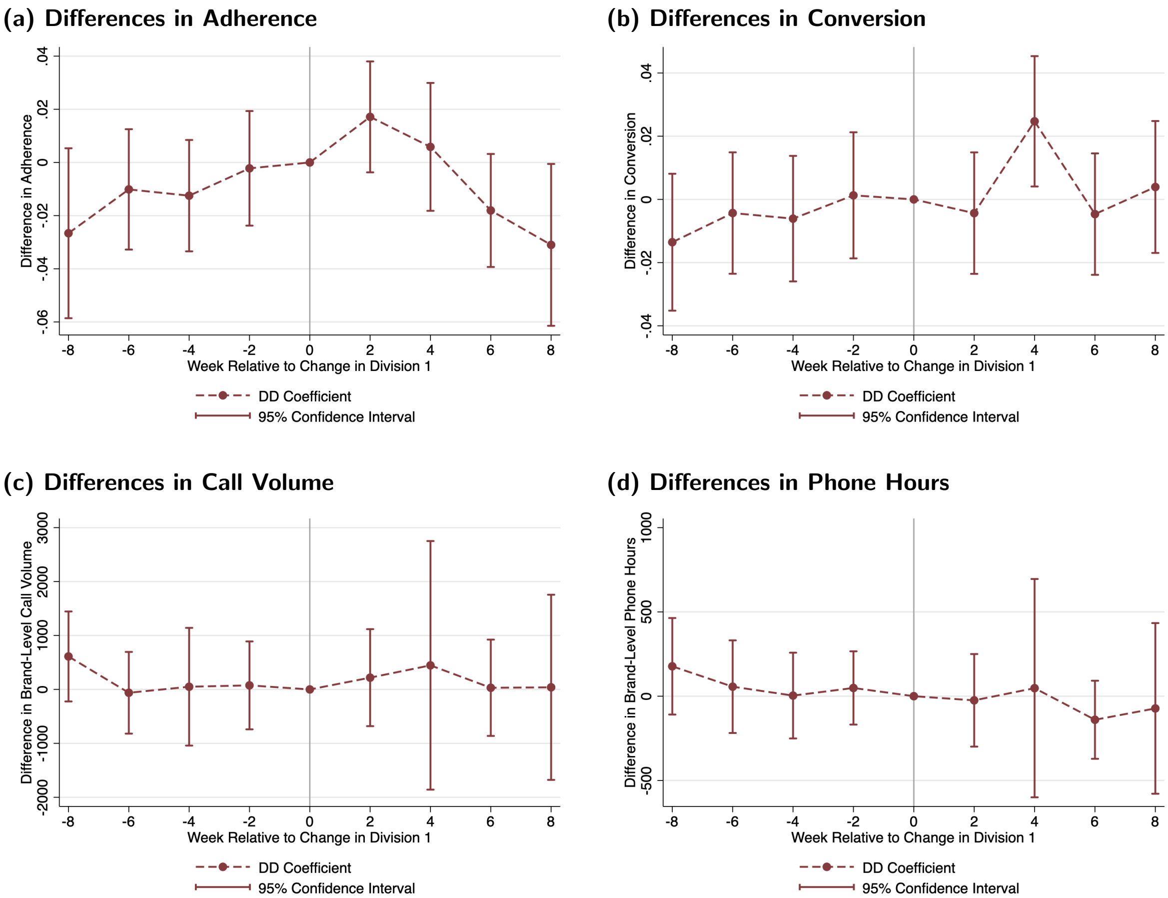



(a) Survival Rates Relative to Month -5 (b) Survival Rates Relative to Month 0

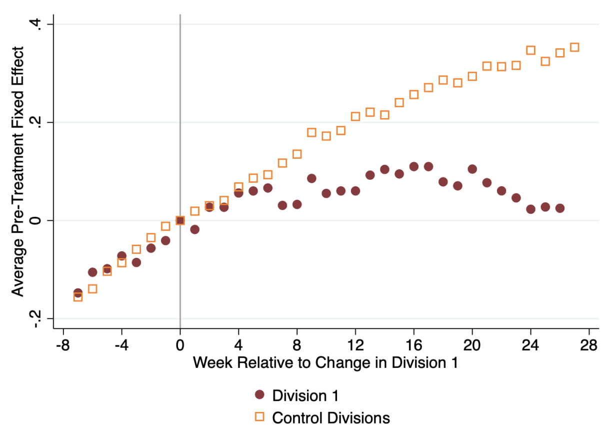
*Notes:* These figures plot Kaplan-Meier survival rates over time. The survival rate estimator considers a starting point and then, from that time, displays the fraction of agents that remain at the firm. Because turnover can be lumpy, with multiple exits in some weeks and no exits in others, we aggregate survival rates to the monthly level. The sample is split by high and low performers based on whether agents' adjusted worker fixed effects are above or below the median within their division.

**Figure 5 Trends in Proxies for Effort Supply and Effort Demand**




*Notes:* The coefficients in these figures are estimates of $\delta_{i,t}$ from Equation (1), using different outcome variables of interest. Adherence and conversion are the two proxies for an agent's supply of effort. Call volume and phone hours are the proxies for customers' demand for worker effort. To improve the readability of these figures, we aggregate data into bi-weekly bins. The p-values of tests that the Week -8 to Week 0 point estimates are jointly equal to zero are 0.45, 0.70, 0.36, and 0.76 for adherence, conversion, call volume, and phone hours, respectively. The p-values of tests that the Week 0 to Week 8 point estimates are jointly zero are 0.98 and 0.76 for call volume and phone hours, respectively.

**Figure 6 Adjusted Worker Fixed Effects Before and After the Compensation Changes**




*Notes:* This figure plots the average adjusted worker fixed effects for Division 1 and the control divisions after taking a z-score transformation. Because the fixed effects are calculated prior to the commission changes, the data are limited to agents who were at the firm prior to the commission changes. Adjusted worker fixed effects are calculated from a regression of log commissions on worker dummy variables, division-by-week dummy variables, and a cubic spline in tenure. We then correct for sampling variation using the method in Lazear et al. (2015). The series are normalized to correspond at the announcement date, which is depicted by the vertical line.

**Table 1 Summary Statistics Pre- and Post-Treatment**

| | Eight Weeks Pre-Treatment | | | All Weeks Post-Treatment | | |
|---|---|---|---|---|---|---|
| | Control Divisions | Treated Division 1 | Treated Division 2 | Control Divisions | Treated Division 1 | Treated Division 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Commission | 200.91 | 318.39 | 502.68 | 206.92 | 249.71 | 308.56 |
| | (184.79) | (283.59) | (333.35) | (189.57) | (230.30) | (226.25) |
| Adherence | 0.80 | 0.83 | 0.79 | 0.81 | 0.82 | 0.80 |
| | (0.11) | (0.11) | (0.15) | (0.12) | (0.12) | (0.14) |
| Conversion | 0.26 | 0.33 | 0.29 | 0.25 | 0.32 | 0.29 |
| | (0.10) | (0.09) | (0.12) | (0.09) | (0.10) | (0.13) |
| Log $RPC_{Old}$ | 4.11 | 4.19 | | 4.14 | 4.09 | |
| | (0.47) | (0.51) | | (0.49) | (0.52) | |
| Log $RPC_{New}$ | 4.11 | 3.96 | | 4.14 | 3.90 | |
| | (0.47) | (0.50) | | (0.49) | (0.50) | |
| Phone Hours | 19.89 | 20.71 | 17.41 | 20.41 | 20.18 | 15.92 |
| | (7.59) | (7.32) | (6.25) | (8.38) | (7.88) | (7.26) |
| Total Calls | 62.35 | 71.13 | 49.19 | 64.58 | 69.88 | 45.30 |
| | (26.79) | (27.76) | (19.52) | (28.99) | (29.50) | (21.31) |
| Tenure (days) | 356.56 | 369.02 | 672.98 | 450.36 | 399.81 | 608.06 |
| | (419.52) | (389.51) | (558.98) | (505.66) | (411.39) | (594.50) |
| Age | 25.84 | 25.18 | 29.71 | 26.20 | 25.99 | 28.33 |
| | (7.15) | (6.50) | (8.64) | (7.32) | (7.75) | (7.98) |
| Single | 0.52 | 0.68 | 0.44 | 0.38 | 0.44 | 0.33 |
| | (0.50) | (0.47) | (0.50) | (0.49) | (0.50) | (0.47) |
| White | 0.71 | 0.72 | 0.60 | 0.68 | 0.66 | 0.62 |
| | (0.45) | (0.45) | (0.49) | (0.47) | (0.48) | (0.49) |
| Male | 0.70 | 0.73 | 0.70 | 0.73 | 0.73 | 0.64 |
| | (0.46) | (0.44) | (0.46) | (0.44) | (0.44) | (0.48) |
| Agent-Weeks | 4,024 | 867 | 357 | 13,817 | 3,474 | 950 |
| Agents | 632 | 138 | 51 | 874 | 234 | 89 |

*Notes:* This table presents summary statistics for the control divisions, Division 1, and Division 2. The *Commission* measure is average weekly commissions; *Adherence* is a measure of schedule adherence, which captures the amount of time an agent is available to take calls; *Conversion* is the probability of having positive sales revenue on a given call; *Log* $RPC_{Old}$ measures an agent's revenue-per-call (RPC) if the commission schedule had *not* changed; *Log* $RPC_{New}$ measures an agent's revenue-per-call (RPC) if the commission schedule had always been at the new levels. *Phone Hours* capture the amount a time an agent spends talking with customers; and *Total Calls* is the number of calls fielded by an agent each week. Data limitations prevent us from measuring *Log* $RPC_{Old}$ and *Log* $RPC_{New}$ for Division 2. Standard deviations are reported in parentheses.

**Table 2 Linear Probability Model Estimates of Turnover Responses**

| | Last Week in Firm | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated x Post x Prod | 0.021** | 0.015** | 0.016* | 0.013** | 0.012* |
| | (0.007) | (0.005) | (0.007) | (0.005) | (0.005) |
| Treated x Post | -0.006 | -0.006 | | -0.002 | |
| | (0.004) | (0.007) | | (0.010) | |
| Treated x Placebo x Prod | -0.006 | | -0.002 | | |
| | (0.004) | | (0.004) | | |
| Treated x Placebo | 0.000 | | | | |
| | (0.004) | | | | |
| Week Fixed Effects | ✓ | ✓ | | ✓ | |
| Division x Week-of-Year Fixed Effects | | ✓ | | | |
| Week x Division Fixed Effects | | | ✓ | | ✓ |
| Post-Territory Shock Period | | | | ✓ | ✓ |
| Observations | 51,497 | 51,497 | 51,497 | 19,689 | 19,689 |
| Mean Turnover Probability in Division 1 | | | 0.037 | | |
| $p$-value on Treated x Post x Prod | 0.017 | 0.081 | 0.096 | 0.036 | 0.040 |
| $p$-value on Treated x Post | 0.482 | 0.316 | | 0.133 | |

*Notes:* The dependent variable is an indicator that equals one if it is the worker's last week at the firm. The sample includes all current employees in Division 1 and the control divisions with non-missing data. Estimates come from a linear probability model that captures changes in the turnover probability for the existing workforce. Each model includes a 5th order polynomial for workers' tenure to account for a potentially arbitrary baseline relationship between tenure and turnover. *Prod* refers an agent's sales $z$-score, which is the standardized measure of an agent's pre-treatment productivity estimated as their adjusted worker fixed effect according to the procedure in Lazear et al. (2015). For additional details, see Section 3.5 and Appendix B. The specification in Column 2 includes division by week-of-year fixed effects to account for seasonality. The specification in Column 3 includes week by division fixed effects. Columns 4 and 5 use a shortened pre-treatment period that only includes the weeks of data after the territory shock period. *Placebo* is an indicator for the date 52 weeks prior to the treatment date. Standard errors are clustered by manager (in parentheses). The p-values in the bottom two lines are computed after clustering by division and applying the wild cluster bootstrap randomization inference procedure in MacKinnon and Webb (2018). We use the t-statistic version of the procedure that imposes the null hypothesis.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

**Table 3 Estimates of Effort Responses**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Panel A: Adherence to Schedule | | | | | | | |
| Treated x Post | 0.003 | 0.006 | 0.025 | 0.022 | 0.018 | 0.029* | |
| | (0.009) | (0.010) | (0.014) | (0.019) | (0.019) | (0.013) | |
| Treated x Post x Prod | | | | | | -0.005 | -0.004 |
| | | | | | | (0.005) | (0.006) |
| Observations | 8,647 | 8,647 | 8,647 | 7,570 | 3,979 | 8,647 | 8,647 |
| Panel B: Conversion Rate | | | | | | | |
| Treated x Post | 0.004 | 0.002 | 0.010 | 0.006 | 0.008 | 0.016* | |
| | (0.007) | (0.005) | (0.007) | (0.008) | (0.007) | (0.007) | |
| Treated x Post x Prod | | | | | | -0.020*** | -0.020*** |
| | | | | | | (0.005) | (0.005) |
| Observations | 8,283 | 8,283 | 8,283 | 6,903 | 3,743 | 8,283 | 8,283 |
| Panel C: Log RPC at Old Prices | | | | | | | |
| Treated x Post | -0.025 | -0.039 | 0.005 | -0.016 | 0.005 | 0.022 | |
| | (0.031) | (0.025) | (0.033) | (0.036) | (0.048) | (0.032) | |
| Treated x Post x Prod | | | | | | -0.041 | -0.042 |
| | | | | | | (0.024) | (0.024) |
| Observations | 9,229 | 9,229 | 9,229 | 7,840 | 4,126 | 9,229 | 9,229 |
| Panel D: Log RPC at New Prices | | | | | | | |
| Treated x Post | 0.027 | 0.007 | 0.027 | 0.002 | 0.016 | 0.046 | |
| | (0.032) | (0.025) | (0.036) | (0.039) | (0.054) | (0.035) | |
| Treated x Post x Prod | | | | | | -0.048 | -0.049 |
| | | | | | | (0.024) | (0.025) |
| Observations | 9,229 | 9,229 | 9,229 | 7,840 | 4,126 | 9,229 | 9,229 |
| Week Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Agent Fixed Effects | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Division Trend Controls | | | ✓ | ✓ | ✓ | ✓ | |
| Week x Division Fixed Effects | | | | | | | ✓ |
| Re-Weighted | | | | ✓ | | | |
| Balanced Sample | | | | | ✓ | | |

*Notes:* The sample includes all current employees in Division 1 and the control divisions with non-missing data. The models in Columns 1–6 include fixed effects for week, division, and office location. All models include cubic splines for tenure and a cubic polynomial in age. The OLS regression in Column 1 includes dummies for ethnicity, gender, and marital status. The specifications in Columns 2–7 include individual fixed effects. Columns 3–6 include division-specific trend controls. The specification in Column 4 uses a re-weighting estimator based on the propensity score for being in Division 1 (see Appendix C.1). The balanced panel in Column 5 restricts to workers who are present prior to July, 2016 and after April, 2017. Columns 6 and 7 consider heterogeneous responses based on worker productivity, and Column 7 omits week fixed effects and division-specific trend controls and instead includes week by division fixed effects. Differing numbers of observations across panels reflect differences in data availability. The sample used restricts to eight weeks of pre-treatment data and eight weeks of post-treatment data. The results are similar when all available pre- and post-treatment data is used (See Table A.3). Reported standard errors are clustered by manager.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

**Table 4 Sentiment Descriptive Statistics**

| | | Pre-Treatment Productivity ($Z$-Score) | | | |
|---|---|---|---|---|---|
| | All | 0%–33% | 33%–66% | 66%–100% | Diff. |
| | (1) | (2) | (3) | (4) | (4)–(2) |
| ΔFairness Perceptions | -1.43 | -2.74 | -3.92 | 2.48 | 5.22 |
| | (2.74) | (4.35) | (5.12) | (4.80) | (6.48) |
| ΔReferral Likelihood | -12.51*** | -5.30 | -11.52** | -19.77*** | -14.46** |
| | (2.90) | (3.32) | (4.85) | (5.93) | (7.04) |
| ΔPromotion Prospects | -0.17** | -0.04 | -0.33** | -0.14 | -0.10 |
| | (0.07) | (0.10) | (0.16) | (0.12) | (0.16) |
| Agents | 70 | 23 | 24 | 23 | |

*Notes:* This table documents the changes in the self-reported sentiment levels of Division 1 agents from before to after the commission schedule changes. We split the data based on terciles of pre-treatment agent productivity. The results of difference-in-means tests between Columns 4 and 2 are reported in the far right column. Standard errors of means are reported in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

## Appendix A: Firm Setting and Compensation Changes

Approximately 600–850 agents were employed at a given point in time during the sample period.

### A.1. Firm Setting

The firm contracts directly with national television, phone, and internet providers to market and sell their products. Prospective customers respond to the firm's marketing promotions by calling an 800-number that corresponds to a particular service provider's products. The sales agents respond to these inquiries and, when appropriate, try to upsell customers on high profit margin products (e.g., larger bundles of TV channels or faster internet speeds).

Agents in Division 1, for instance, handle calls from residential customers, inquiring about products from a particular service provider. Agents in the control divisions respond to residential inquiries about products from different, albeit similar, service providers. Agents in Division 2 respond to inquiries from small businesses, but the products offered resemble those of the other divisions. To accommodate the lucrative opportunities of interacting with small businesses, the firm reserves space in Division 2 for its most productive and experienced agents. Inbound calls are routed to a particular division based on the phone number dialed by the prospective customer. Within a division, calls are allocated to the next available agent in the queue (i.e., based on who been waiting the longest). This means sales opportunities are allocated to agents randomly. The firm is almost exclusively an *inbound* call center, meaning that agents answer calls from interested customers. Less than 3% of calls are outbound—most of which are agents following up on earlier inbound calls (e.g., returning a dropped call).

Agents rely on designated sales protocols and their understanding of the caller's needs to sell the products. Success depends on an agent's understanding of the products, his or her ability to master the sales protocol, and his or her ability to upsell customers onto high profit margin products. In most cases, the highest margins are earned on the most expensive products (e.g., a satellite subscription with all possible channels) or bundles of products (e.g., a service contract covering internet, telephone, and television), which are more difficult to sell. Agents generally earn more in commissions for selling high profit margin products than they do for selling low profit margin products. Agents spend about 80% of their workday either on calls or waiting for another call to arrive, and they have little scope to change the number of calls they receive.

All the agents in our sample have an employment contract requiring their presence during scheduled hours. Approximately 85% of the agents work at least 30 hours a week, and they have limited scope to adjust their hours.

### A.2. Agent Compensation

Agent pay is made up of a fixed hourly wage, commissions, and occasional small bonuses. Agents start at an hourly wage of approximately 150% of minimum wage and receive small raises for every three months of tenure. Hourly wage rates are capped at approximately 200% of the minimum wage. Agents who stay with the company beyond a waiting period are eligible for health benefits. In addition to fixed wages, commissions are a significant part of an agent's total compensation. During the eight weeks before the compensation changes occurred, the average Division 1 agent earned \$318 per week in commissions, and the average control division agent earned \$201.

### A.3. Changes to the Commission Schedule

Table A.10 provides a more detailed example of the commission schedule changes for a set of products that can either be sold individually, in a bundle of two products, or in a bundle of three products. The example suggests that an agent's revenue would drop from $375 to $315 if the agent was unable to sell a greater proportion of higher quality internet packages.

Based on the productivity data provided by the firm, we estimated that the commission schedule changes would reduce the commission pay of the average Division 1 agent by 18%, holding fixed the pre-treatment period mix of products sold. When we compare average commission levels in the pre-territory shock period ($318.39 from Weeks -8 to 0) to those in the post-treatment period ($249.71 after Week 0), the implementation of the new commission schedule led to a 21.5% decrease in commission pay for the average Division 1 agent.

## Appendix B: Empirical Details

Our immediate sample includes proxies of worker effort—e.g., adherence, conversion rate, phone hours, revenue generated per call, total revenue generated per week—demographic details—e.g., age, race, tenure, gender, marital status—and commission pay data. Adherence equals the fraction of time an agent is available to answer calls over the total time an agent is scheduled to be available; conversion rate equals the ratio of calls received where a sale is made to total calls received; and phone hours equals the number of hours in a week an agent spends talking to a customer.

To estimate turnover responses, we extend our data. This extended data has limited performance data on sales revenue, but it does contain commissions, allowing us to identify and control for seasonal (year-to-year) patterns in compensation and attrition. We use this extended dataset, called the extended sample, for our turnover analysis.

### B.1. Estimating Baseline Agent Productivity

We estimate productivity by using a fixed effects regression analysis of log commissions, which is an omnibus measure of sales productivity that is available in both the immediate sample and the extended sample. We calculate an agent's adjusted worker fixed effect using a regression of log commissions on the worker's tenure profile, division-by-week fixed effects, and agent fixed effects. We use data leading up to four weeks before the changes. Log commissions are used, rather than commissions-per-call/hour, revenue-per-call/hour, or log revenue, because we do not have data on the number of calls received, the number of hours worked, or the revenue generated in the extended sample for 2015. In addition, accounting for the tenure profile makes this measure one about underlying talent, rather than productivity improvements that may come from learning or on-the-job experience. We include agent tenure in the main specification because those with higher productivity are less likely to leave the firm and may have greater tenure. To minimize the impact of measurement and sampling error, we adjust the raw worker fixed effects, according to best practices in the literature. The adjusted worker fixed effects are interpretable up to a division-level average that is removed through the division-by-week fixed effects. To account for sampling variation in the estimated fixed effects, we run the main regression and collect the residuals plus the estimated worker fixed effects. We then follow the procedure of Lazear et al. (2015) and fit a restricted maximum likelihood random effects estimator and recover each worker's expected best linear unbiased predictor of their latent fixed effect. The estimator resembles an empirical Bayes procedure, where noisier sequences of data on individual workers receive less weight; less noisy data moves the estimated fixed effect away from 0 (a normalization). We call the resulting output the adjusted worker fixed effects. The adjusted worker fixed effects guard against mean reversion or classification being driven by sampling error from a short panel. We use the resulting adjusted worker fixed effects as a measure of agents' baseline productivity, allowing us to identify high and low performers in each division before the commission schedule changes occurred in Division 1.

### B.2. Test of Heterogeneous Commission Change Exposure by Agent Productivity

To test whether agents with different fixed effects were more or less affected by the commission change, we calculate the expected percentage change in commissions based on the sales mix in the pre-treatment period. The variable *Predicted Pct* $\Delta$ *Commission Post-Treatment* in Table A.1 reports this measure. The predicted percentage change in commissions, due to the pre-treatment period sales mix, is 17%–18% across all tercile groups of workers. Although the top tercile of workers has average weekly commissions that are more than 2.5 times greater than the bottom tercile, the product mix of sales does not impact percentage changes in commissions significantly more for any one group based on pre-treatment sales behavior. Put differently, the commission schedule changes affected the expected percentage change in commissions equally across all terciles of agents in Division 1.

### B.3. Surveys of Sentiment and Reactions to the Changes

We conducted a firm-wide survey before the announcement of the changes to gather information regarding agents' sentiment toward the firm. We asked sales agents from all divisions the following three questions: (1) "How likely are you to agree with the following statement, [the firm's] policies, for example on adherence, compensation, and promotion, are justified and fair?" (2) "Suppose your friend is looking for a job, how likely are you to recommend them to apply at [the firm]?" (3) "Do you think you will be promoted in the future?" The possible answers to these questions are as follows: (1) Unit increment slider from 0 (Strongly Disagree) to 100 (Strongly Agree). (2) Unit increment slider from 0 (Not Likely) to 100 (Very Likely). (3) "Yes, within 0–3 months," "Yes, within 3–6 months," "Yes, within 6–12 months," "Yes, in over 12 months," "I don't want a promotion," and "No, promotion is not likely."

In addition, we conducted a follow-up survey among agents in Division 1 after the announcement of the changes and before these agents received their first paycheck reflecting the new commission schedule. In this survey, we again asked the three sentiment questions (which are discussed in detail in Section 5.4), and we also asked agents several questions related to the effects of and motivation for the commission schedule changes. The first prompt of both surveys told respondents "This project was specifically outsourced to increase privacy, so you can answer these questions truthfully." We believe agents responded truthfully to the survey questions, though we can never be sure how agents will interpret this statement.

In the follow-up survey we asked:

"By how much will the changes affect your commission pay (assuming you work just as many hours and just as hard as before)?" Agents responded using a slider from -50% to 50%, indicating a 50% decrease to 50% increase.

"How would your level of effort have to change in order to maintain your usual commission pay?" Agents responded using a slider from -20% to 20%, indicating a 20% decrease to 20% increase in effort exertion.

"How will the revenue change affect your choice of effort relative to before the change?" Agents responded using a slider from -20% to 20%, indicating a 20% decrease to 20% increase in effort exertion.

### B.4. The Firm-Level Effects of the Compensation Changes

Because the loss of high performers occurred with a significant lag, as shown in Figures 4b and 6, the short-term return to the firm was positive, due to the compensation savings, negligible short-run workforce changes, and minimal overall effort change. We attempt to quantify the inflection point when the unit profit change to the firm, on a per-call (or per-transaction) basis, might turn negative, due to changes in workforce composition.

We calculate the unit profit change to the firm by combining changes in revenue-per-call and commission-per-call. We calculate these changes in revenues and costs using several estimated statistics while making several plausible assumptions. All that is required for this calculation is a measure of the agent's average RPC in the pre-treatment period, adjusted worker fixed effects, and an estimate of the change in the turnover probability as a function of adjusted worker fixed effects. We make five further simplifying assumptions. First, as supported by the lack of empirical evidence of changes in worker effort and revenue generation, we assume these effects are zero. Put simply, we assume the brands' actual payments to the firm remained fixed. Second, we assume that calls are re-routed to an average agent (based on the pre-treatment sales distribution) in the face of turnover. (This is a conservative assumption. New workers who join are likely to have expected sales that are below the average sales of the cross-section of agents.) Third, we assume that the per-call commission expense (CPC) for replaced agents reflects the median agent's commissions, which yields slightly greater cost savings compared to using the average. Fourth, we assume replacement agents earn hourly wages that are \$1.00 lower than departing agents; at two calls per hour, this translates into a per-call savings of about \$0.50. Fifth, to match the timing of turnover empirically, we assume that week-to-week turnover differences begin to accumulate with a five-week lag after the implementation of the commission schedule changes.

Using these assumptions, we first calculate the change in per-call revenue to the firm $t$ weeks after the compensation adjustment as:

$$\overline{\Delta RPC_t} = \frac{1}{N}\sum_i \left[(RPC_i^{Pre} - \overline{RPC^{Pre}}) \times [(1-\tau_i^{Pre})^t - (1-\tau_i^{Post})^t]\right], \tag{5}$$

where $\tau_i^{Pre}$ is the per-week turnover probability for each agent prior to the changes and $\tau_i^{Post}$ is the per-week turnover probability for each agent after the changes, which is estimated from agent productivity and the parameter $\delta_2$ in Equation (2). The expression $[(1-\tau_i^{Pre})^t - (1-\tau_i^{Post})^t]$ captures the change in the probability of retaining worker $i$ through week $t$ as a result of the compensation changes. The expression inside the summation operator captures the change in average revenue-per-call as the product of agent $i$'s baseline RPC, relative to the mean RPC, multiplied by the change in retention probability through week $t$ for that agent.

Second, we calculate the change in commission-per-call and the fixed-wage bill using a similar expression, shown below. These calculations reflect the baseline cost savings, absent agent turnover, where $\Delta CPC_i$ is the change in commission-per-call as a result of the compensation changes for each agent. We take $\overline{\Delta CPC}$ as the average change in commission-per-call in the absence of turnover and then adjust the compensation savings due to agent composition as follows:

$$\overline{\Delta CostPerCall_t} = \overline{\Delta CPC} + \frac{1}{N}\sum_i \left[(\Delta CPC_i - \Delta CPC_{Med.} - 0.5) \times [(1-\tau_i^{Pre})^t - (1-\tau_i^{Post})^t]\right], \quad (6)$$

where the term in the summation is the per-call change in commissions less the change in per-call fixed wages ($0.50) weighted by the difference in retention probability.

Finally, we compute the unit profit change to the firm through week $t$ as any revenue change plus net cost savings: $\overline{\Delta RPC_t} - \overline{\Delta CostPerCall_t}$. To get a rough estimate of the present value of these weekly changes, we use a 12.5% annual interest rate and project forward for six months. We stop the data at six months, because this firm has seasonal hiring that begins in the summer, often a period of substantial workforce changes.

Initially, the cost savings from the commission schedule changes look attractive, saving the firm about $0.68 in compensation expense per-call. Because turnover is minimal in the first few weeks after the changes, there is no offsetting reduction in revenue. However, about two months after the changes (eight weeks), the workforce composition effect reduces average revenue-per-call by $0.58, whereas the labor cost savings in Week 8 equals $0.71. Over time, the decrease in the average revenue-per-call grows more quickly than does the cost savings, and we find that Week 18 is the inflection point (a bit more than four months post-treatment), after which the net present value of the commission schedule changes is negative. Six months after the changes, the firm's gross margin per-call fell by more than 1.7 percentage points.

To put these numbers into context, we estimate the total net present value of the commission schedule change by multiplying the per-call numbers by the actual number of calls per week. At the six-month horizon, the net present value of the commission schedule changes totaled -$75,500.

## Appendix C: Robustness

### C.1. Re-weighting Estimators

This section provides details about the implementation of the re-weighting estimators that attempt to match individuals in control divisions with individuals in Division 1. The purpose is to match individuals based on their sales trajectories. The first step is to estimate the probability of being in Division 1. We use the data from the pre-treatment period for this purpose but hold out the data one month prior to the commission schedule changes. The second step is to use the propensity score from this estimation procedure to form weights which will be used in later regressions. The third step is to assess how well the re-weighting estimates fit, using a "hold out" sample of data one month prior to the commission schedule changes.

In the first step, we estimate logit models where the dependent variable is being in Division 1. Each worker present in the pre-treatment period for Division 1 and the control divisions enters the sample once. The first month of available data includes the $X$ variables and demographic characteristics in levels. The regressors in $X$ are an indicator for male, the agent's age, and the agent's monthly averages of log commissions, log commission per call, log revenue, log total calls, tenure, and adherence. For each of the regressors on productivity, we also include one and two month differences over future months to capture trends in these measures. We then estimate the logit model and form $\hat{P}$, the predicted probability of being in Division 1.

The weights in the second step are formed as $W_i = Treated_i + (1 - Treated_i)\frac{\hat{P}}{1-\hat{P}}$ where $\hat{P}$ is the treatment probability estimated from the logistic regression on pre-treatment data and $Treated_i$ indicates the worker is in Division 1. Figures A.3a and A.3b assess fit, making it clear that per-call fit works reasonably well. Fit for overall revenue is not as good, suggesting that the territory shock yielded an up-tick in sales success among Division 1 agents. As a result, we prefer specifications at the per-call level to remove potential demand confounders when interpreting changes in effort supply. These per-call measures of productivity allow us to measure output while controlling for demand. Given that the divergence between the re-weighted control group trend and the trend for Division 1 occurs before the commission schedule changes, we suspect demand changes are responsible for divergence in the levels measures.

### C.2. Spillovers to the Control Group

To test another identifying assumption, the lack of spillovers to control divisions (Obloj and Zenger 2017), we conduct structural break tests for the control group. Figure A.7 plots the parameter estimates from various specifications of these break tests. Structural break tests come from regressions using the control sample. The figure reports the post-treatment indicator parameter estimates and confidence intervals. We consider several different dependent variables, and each regression includes a post-treatment indicator for Division 1, the matrix of agent characteristics $X_{it}$, division fixed effects, and trends for each division. These results suggest that there are minimal spillovers to the control group.

Figure A.8 plots the time series process for the control groups around the event date for Division 1. Within a month of the event date, there is minimal movement in the control group averages. Conversion rates and RPC do show some mild deterioration after the first month, which is likely due to seasonality based on the time of the year.

### C.3. Substitution to Different Products

Whether agents could reduce the impact of the commission schedule changes because of substitution to other products is an empirical question. The approach is to estimate whether sales revenue becomes more heavily weighted to items with more favorable relative prices under the new commission schedule. Although there were some relative price changes that may have given rise to agent substitution, we find that agents could not offset the adverse effects of the commission schedule changes by changing their mix of products sold. That is, the overall change in commissions-per-call that we estimate closely follow the predicted reductions given the pre-treatment mix of products sold.

## Appendix D: Motivating Framework

We motivate the analysis with a simple model of heterogeneous agent responses to commission changes as a function of productivity differences. Heterogeneous responses are difficult to sign without assumptions, making them empirical objects of analysis. We then consider how turnover changes across the productivity distribution affect profitability.

Let $e_i$ denote agent $i$'s sales effort and assume further that his sales revenue, $y_i$ is given by $y_i = \theta_i e_i + \epsilon$ where $\theta_i > 0$ is the agent's productivity or type, and $\epsilon$ is mean-zero noise. To simplify the exposition, all agents are assumed to be risk-neutral and collect a linear share of their revenues, $R$, in addition to a common fixed wage, $\alpha$, such that we can represent agent $i$'s expected utility by $U(\alpha, R, \theta_i, e_i) = \alpha + R\theta_i e_i - c(e_i)$.

The cost of effort function $c(\cdot)$ is strictly increasing and convex, with $c(0) = c'(0) = 0$. Let $e^*$ denote the unique solution to the agent's problem:

$$e^* = \underset{e}{\operatorname{argmax}} R\theta e - c(e)$$

such that agent $i$'s value function evaluated at $e^*$ can be expressed as $V(\alpha, R; \theta_i)$.

The optimal effort, $e_i^*$, is strictly positive, as $c'(0) = c(0) = 0 < R$. Accordingly, the function $U$ has strictly increasing differences in $e_i$ and $R$, as well as in $e_i$ and $\theta_i$. By application of Topkis's Theorem, both $\frac{\partial e_i^*}{\partial R}$ and $\frac{\partial e_i^*}{\partial \theta_i}$ are themselves strictly positive. However the heterogeneous effort responses across agents of different pre-treatment productivity are captured by $\frac{\partial^2 e_i^*}{\partial R \partial \theta_i}$, which we cannot sign without additional assumptions.

Proposition 1. *An agent's change in effort with respect to commissions is increasing in agent productivity, $\theta$, as long as $c'''$ is sufficiently small.*

*Proof of Proposition 1.* The goal is to show that the marginal effect of productivity, $\theta$, on agent $i$'s effort response to a change in commissions is directly proportional to the curvature of the agents' cost function. Specifically:

$$\frac{\partial^2 e_i^*}{\partial R \partial \theta_i} \propto \left(c''\left(e_i^*\right)\right)^2 - c'''\left(e_i^*\right) R\theta_i. \tag{7}$$

To prove Equation (7), we begin with the first order condition $R\theta_i = c'\left(e_i^*\right)$. Differentiating both sides with respect to $R$ yields $\theta_i = c''\left(e_i^*\right)\frac{\partial e_i^*}{\partial R}$. Differentiating again by $\theta_i$ yields: $1 = c'''\left(e_i^*\right)\frac{\partial e_i^*}{\partial R}\frac{\partial e_i^*}{\partial \theta_i} + c''\left(e_i^*\right)\frac{\partial^2 e_i^*}{\partial R \partial \theta_i}$, substituting the earlier terms and rearranging yields:

$$\frac{\partial^2 e_i^*}{\partial R \partial \theta_i} = \frac{\left(c''\left(e_i^*\right)\right)^2 - c'''\left(e_i^*\right) R\theta_i}{\left(c''\left(e_i^*\right)\right)^3},$$

which completes the proof as $c'' > 0$ by assumption. □

When the agent's costs follow a standard power function, e.g. $c(e) = e^n/n$, the expression characterizing $\frac{\partial^2 e_i^*}{\partial R \partial \theta_i}$ is strictly positive. We conclude that in most standard settings, agents have weakly larger effort responses to commission changes as their type increases. Accordingly, we treat effort changes by agent type as an empirical question, and instead turn our attention to turnover effects.

Beginning with the seminal work of Burdett and Mortensen (1998), the job ladder model has been used extensively to capture worker mobility. The standard model maintains an attrition (quit) rate of $Q(w) = \delta + \lambda[1 - F(w)]$, where $\delta > 0$ captures exogenous job destruction, $\lambda \in [0, 1]$ captures search frictions via an arrival rate of outside job opportunities, and $w$ is a random variable with density $f(\cdot)$ and associated CDF $F(\cdot)$ capturing the distribution of *fixed* wage offers to the agent from outside firms. We define the agent's reservation wage, $w(\theta_i)^*$, as the lowest fixed-wage yielding an expected utility of $V(\alpha, R; \theta_i)$. (Without loss of generality, we assume that the fixed-wage offers require the agent to exert a fixed level of (un-modeled) effort with known dis-utility equal to 0.) To simplify the ensuing analysis, we assume that the agent's type, $\theta_i$, does not influence his expected utility outside of the firm—that is, we assume that agent productivity is entirely firm-specific. As the following proposition shows, however, the agent's type will influence his reservation fixed-wage.

PROPOSITION 2. *First, low-productivity agents are more likely to leave the firm than high-productivity agents. Second, the marginal attrition associated with a commission reduction is greatest for high-productivity agents. Third, the distribution of incoming offers ultimately determines if the change in turnover rate is increasing in agent productivity.*

*Proof of Proposition 2.* The optimal effort $e_i^*$ is increasing in type (see proof to Proposition 1), therefore revealed preference implies that the agents' expected utility $V(\alpha, R; \theta_i)$ is itself increasing in $\theta_i$. Because the agents have a (strictly) positive utility for wages, the unique fixed-wage, $w(\theta_i)^*$, which makes an agent indifferent between the outside offer and his internal utility, $V(\alpha, R; \theta_i)$, must itself be increasing in $\theta_i$. Consider two agents, with productivity levels $\theta_j > \theta_i > 0$. Since $w(\theta_j)^* > w(\theta_i)^*$, all offers $\overline{w} \geq w(\theta_j)^*$ are sufficient to lure both types of agents away from the firm. Offers $\underline{w} \in [w(\theta_i)^*, w(\theta_j)^*)$, on the other hand, will lure the agent with type $\theta_i$ but are insufficient to lure the agent with type $\theta_j$. Accordingly, an agent with productivity $\theta_i$ will leave the firm while an agent with productivity $w(\theta_j)^*$ will remain with probability $F(w(\theta_j)^*) - F(w(\theta_i)^*) > 0$.

To prove the second statement, we must establish that $\frac{\partial^2 w(\theta_i)^*}{\partial R \partial \theta_i} > 0$, which suffices as the distribution of outside offers is independent of internal compensation contracts. By definition, $w(\theta_i)^*$ is the lowest external, fixed-wage offer that yields utility $V(\alpha, R; \theta_i)$ to agent $i$. Revealed preference guarantees that an agent's expected utility $V(\alpha, R; \theta_i)$ is strictly increasing in $R$. Accordingly, the minimum external wage $w(\theta_i)^*$ increases (decreases) for all types as the commission rate $R$ increases (decreases). To see this formally, note that the envelope theorem yields $U'(e_i^*) = 0$, hence:

$$\frac{dV}{dR} = \frac{\partial U}{\partial R} + U'(e_i^*)\frac{\partial e_i^*}{\partial R} = \frac{\partial U}{\partial R} = \theta_i e_i^* > 0,$$

where the final inequality holds by the strict convexity of $c(\cdot)$ and the fact that both $c(0)$ and $c'(0)$ are equal to zero.

We must next prove that the marginal effect increases concomitantly with agent productivity:

$$\begin{aligned} \frac{d^2 V(\alpha, R, \theta_i)}{dR d\theta_i} &= \frac{\partial^2 U}{\partial R \partial \theta_i} + \frac{\partial U}{\partial R} U'(e_i^*) \frac{\partial e_i^*}{\partial \theta_i} \\ &= \frac{\partial^2 U}{\partial R \partial \theta_i} = 2e_i^* > 0. \end{aligned}$$

We have thus established that: (1) decreasing the commission rate $R$ makes all agents more vulnerable to poaching, and (2) following a reduction of $R$, a highly productive agent, say an agent with productivity $\theta_j$, decreases their external reservation rate, $w(\theta_j)^*$ by more than a less productive agent reduces their own external reservation wage $w(\theta_i)^*$, where $\theta_i < \theta_j$. This does not, however, establish that high-productivity agents are more likely to leave the firm following a wage reduction, because separation nonetheless requires an external offer. To see this, consider a discrete change in $R$ from $\overline{R}$ to $\underline{R}$ with $\overline{R} > \underline{R}$. Abusing notation, let $\underline{w}(\theta_i)^* = V(\alpha, \underline{R}, \theta_i)$ and $\overline{w}(\theta_i)^* = V(\alpha, \overline{R}, \theta_i)$. Accordingly, we can define $W(\theta_i)^* = [\underline{w}(\theta_i)^*, \overline{w}(\theta_i)^*)$ as the set of external wages which would suffice to lure an agent with productivity $\theta_i$ under the commission rate $\underline{R}$ but not under the commission rate $\overline{R}$. For $\theta_i < \theta_j$, we have shown that $\|W(\theta_i)^*\| < \|W(\theta_j)^*\|$, however $\int_{W(\theta_i)^*} f(w)dw$ may exceed $\int_{W(\theta_j)^*} f(w)dw$. In other words, the distribution of incoming external offers ultimately determines whether high- or low-productivity workers are more likely to separate from the firm following a reduction in the commission rate, $R$. $\square$

The intuition behind the first statement in Proposition 2 is relatively straight-forward: because all agents face the same distribution of outside offers, those with the lowest reservation utility are more likely to accept a relatively low outside offer, and hence are the most likely to leave. The second finding is slightly more nuanced; while all agents are more likely to accept an outside offer once their (internal) commission rate, $R$, decreases, a commission reduction will decrease a high-productivity agent's reservation wage $w(\theta_j)^*$ by more than the same commission rate change affects a low-productivity agent's reservation wage $w(\theta_i)^*$—the difference in reservation wage adjustment is determined by the agents' (common) effort cost function, $c(e)$. Despite the larger reservation wage adjustment, the theory is unable to predict how a change in the commission rate, $R$, will effect relative attrition rates because we have not imposed restrictions on the distribution, $f(\cdot)$ of external offers $w$. If, however, internal productivity did influence external offers; e.g. if agents can project their productivity to external employers, then highly-productive agents will be that much more likely to separate from the firm. Even had we modeled such a mechanism, without very strict assumptions now on the conditional distribution of external opportunities, whether or not highly-productive agents are more likely to separate from the firm following an adverse commission change, would remain an empirical question. The answer to this question influences how compensation changes map into firm profits.

PROPOSITION 3. *The sensitivity of changes in profits with respect to sales commissions depends on the turnover propensity of high-productivity agents relative to low-productivity agents. The turnover of highly productive agents mitigates any cost savings from reducing R.*

*Proof of Proposition 3.* We consider a representative sales opportunity allocated to a random agent. Let $g(\theta|R)$ denote the density of agent types at the firm under the commission structure $R$. The expected profits from the sales opportunity are

$$(1-R)\int \theta e^*(\theta,R)\,dG(\theta|R).$$

Differentiation with respect to R yields

$$\frac{\partial \pi}{\partial R} = -\int \theta e^*(\theta,R)\,dG(\theta|R) + (1-R)\int \left\{\theta \frac{\partial e^*}{\partial R} g(\theta|R) + \theta e^* \frac{\partial g(\theta|R)}{\partial R}\right\} d\theta.$$

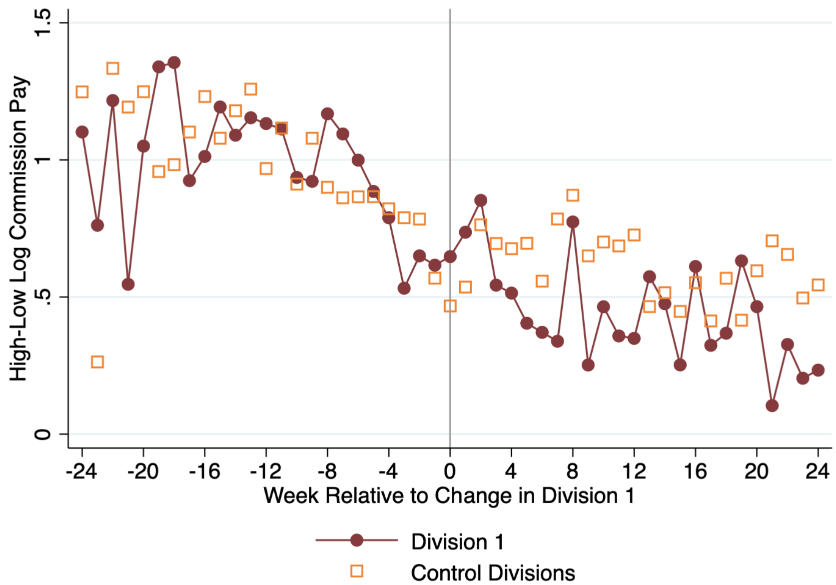
The first term, $-\int \theta e^*(\theta,R)\,dG(\theta|R)$, is negative, as raising commissions while holding sales fixed provides the agent with a transfer. When $\frac{\partial g(\theta|R)}{\partial R} = 0$, such that there is no sorting, the sign of the second term is positive, meaning the agent's positive effort response may offset the firm's decreased profits from the transfer made to the agent. When $\frac{\partial g(\theta|R)}{\partial R} > 0$, the average quality of the workforce increases with $R$, further offsetting the firm's decreased profits stemming from marginal transfers to the agent. $\square$

A reduction in commissions has two different effects: profits increase because of cost savings, while effort reductions offset some of these savings. When the change in the composition of the workforce is greatest for highly productive workers, that is $\frac{\partial g(\theta|R)}{\partial R}$ is increasing in $\theta$, the loss of highly productive workers further offsets the cost savings from the commission changes. The magnitude of the composition and effort changes is the empirical question that we examine.

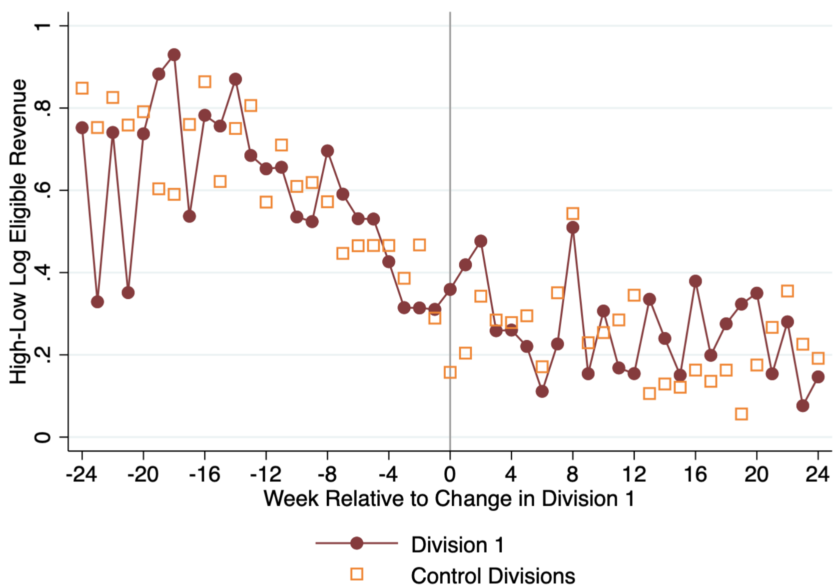
## Appendix Figures and Tables

**Figure A.1 Common Trends by Worker Type within Division**

(a) **Log Commissions by Median Ability**



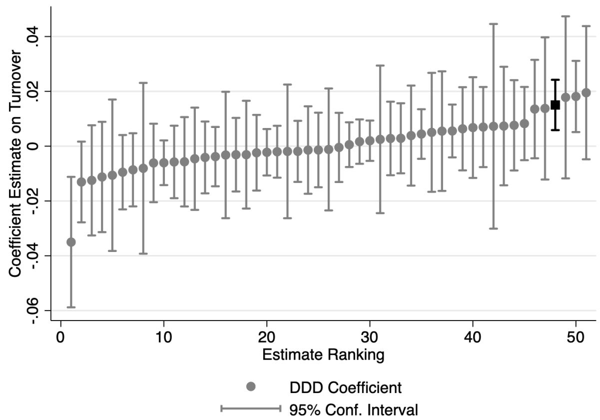

(b) **Log Revenue by Median Ability**



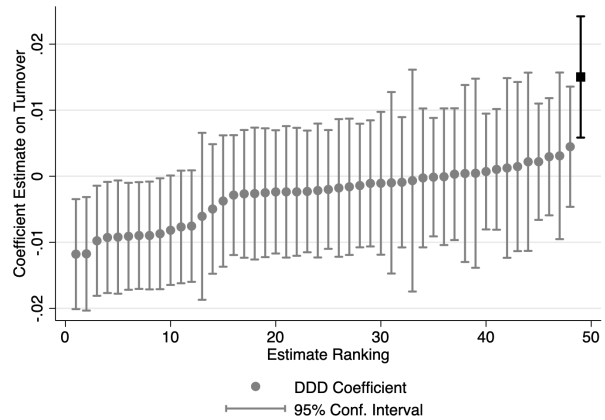

*Notes:* These figures plot the evolution of within-division differences in performance by worker pre-treatment productivity. Figure (a) considers trends in log commissions, whereas Figure (b) considers trends in log revenue. Week 0 on the x-axis denotes the week immediately before the commission schedule changes occurred. The y-axis in each figure captures the differences in output between high and low performers.

**Figure A.2 Placebo Treatment Tests**

(a) **Agents and Treatment Date Randomized**



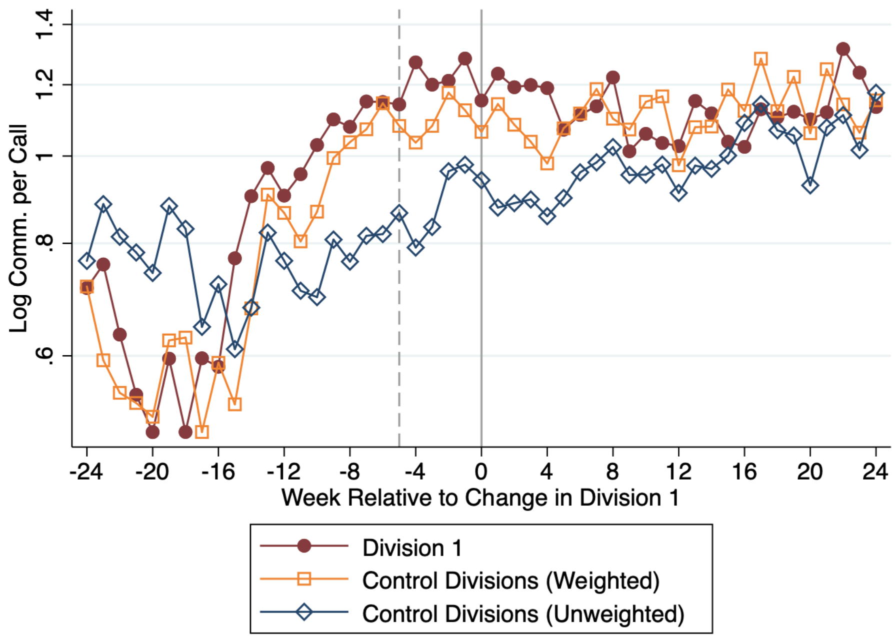

(b) **Divisions and Treatment Date Randomized**



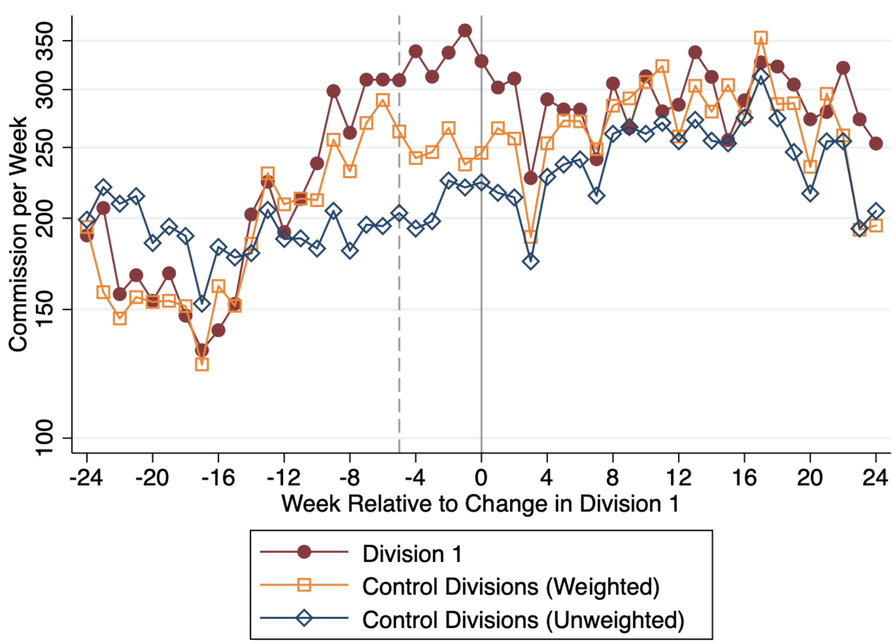

*Notes:* Figure (a) plots fifty placebo simulations (gray dots) for the turnover response estimation using randomized treatment groups and treatment dates. The black square shows the actual result from Column 2 of Table 2. For each placebo simulation, we randomly select 180 agents to constitute the treated division, with the other agents making up the control group. We then randomly choose an intervention week between September 1st, 2016 and January 31st, 2017. This process is similar to that used in Gubler et al. (2018). Figure (b) plots similar placebo simulations for the turnover response estimation using different divisions as the "treated" division, while the other divisions (including the actual treated division) make up the control group.

**Figure A.3** **Re-weighted Commissions and Commissions-per-Call for Division 1**

(a) **Log Commissions per Call (log scale)**

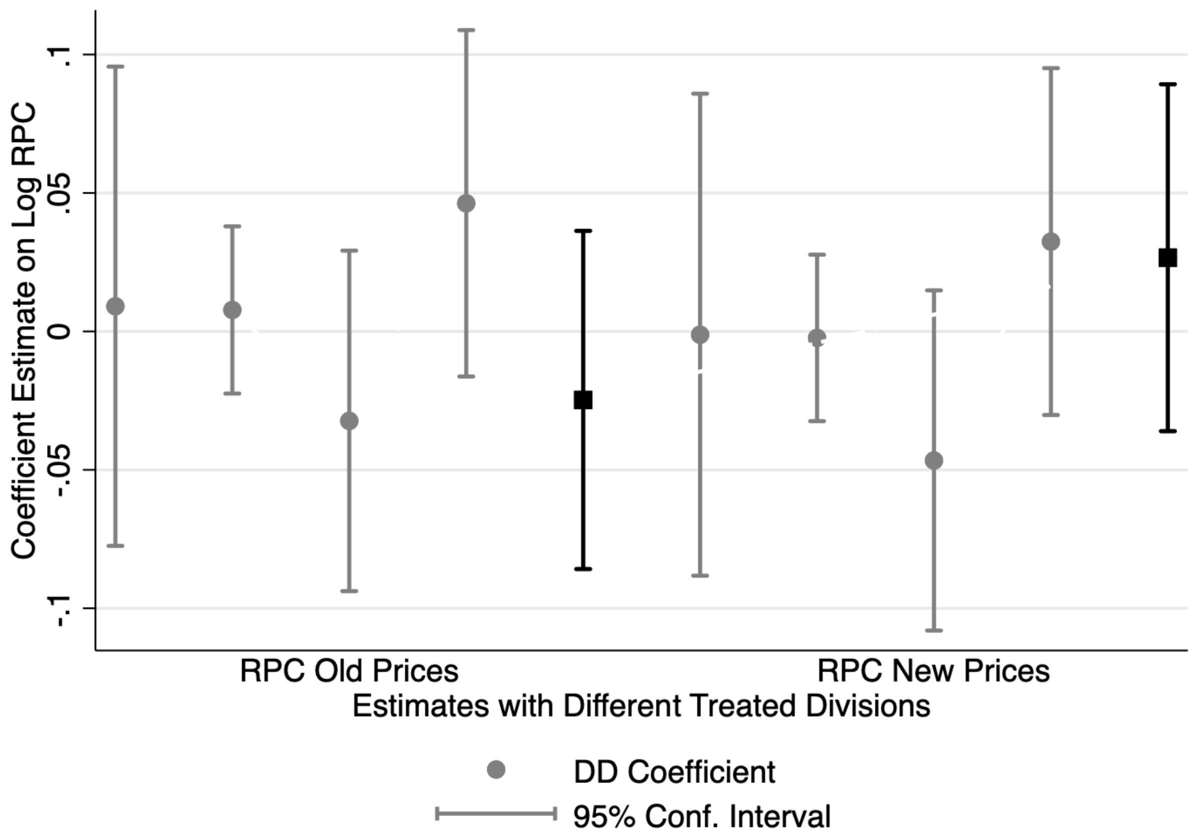



(b) **Commissions per Week (log scale)**




*Notes:* These figures display unweighted and propensity score weighted comparisons of agents in the control divisions and Division 1. The dashed line represents the end of the period used for estimating the propensity score weights. Output measures are displayed on a log scale.

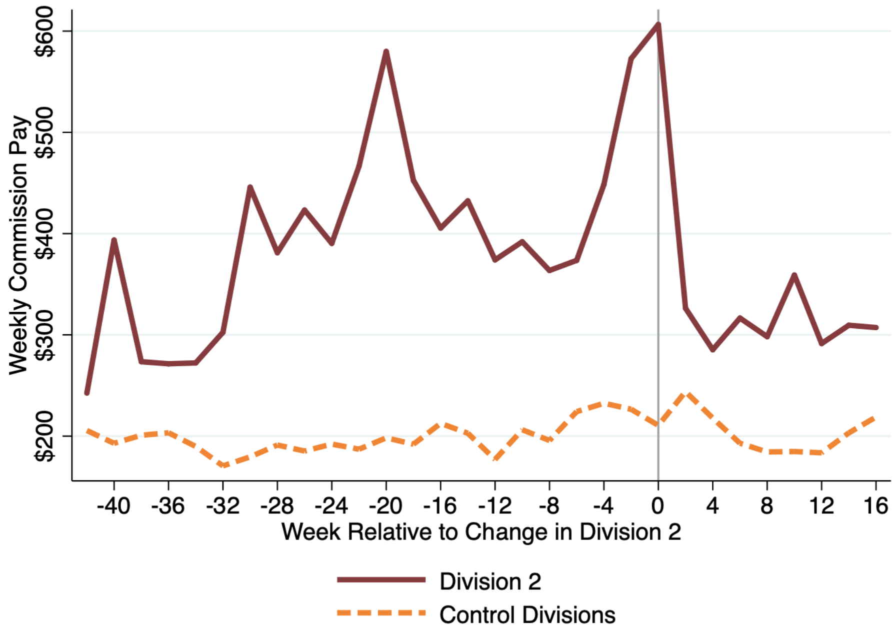
**Figure A.4 Placebo Tests for Effort Estimations**



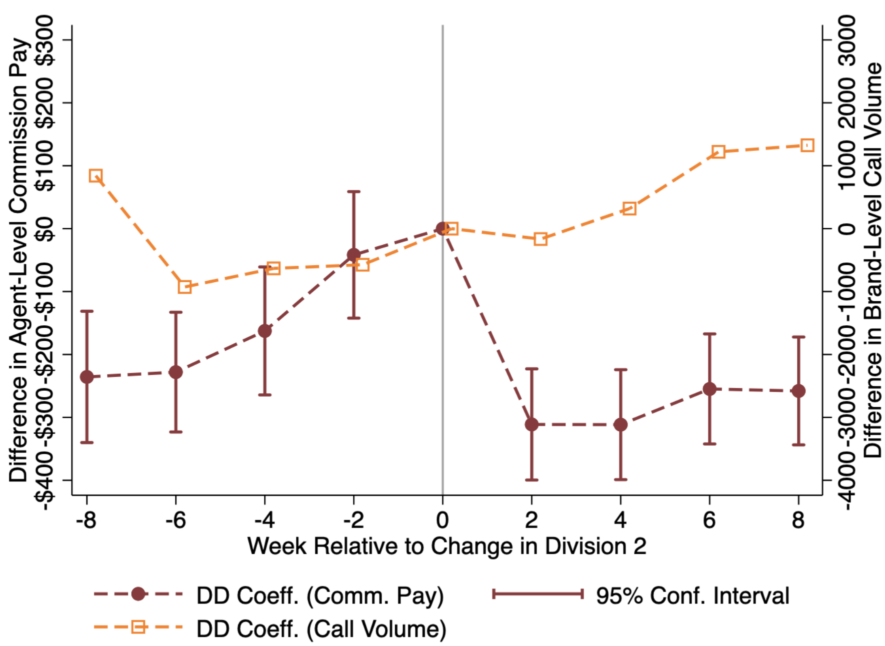

*Notes:* This figure plots placebo simulations for the effort response estimation using different divisions as the "treated" division, while the other divisions (including the actual treated division) make up the control group. These are marked by the gray dots. The actual results from Column 1 of Table 3 are depicted by the black squares. The left five estimations use log RPC based on the old prices as the dependent variable, whereas the right five estimations use log RPC based on the new prices as the dependent variable.

**Figure A.5 Commission Trends in Division 2 and the Control Divisions**

(a) **Trends in Commission Levels**



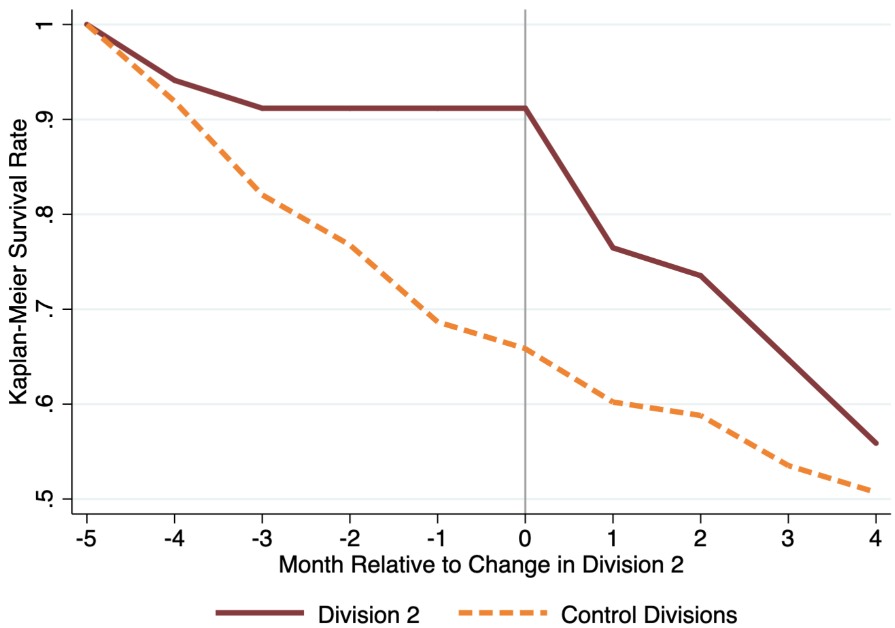

(b) **Differences in Commission Levels and Total Call Volume**



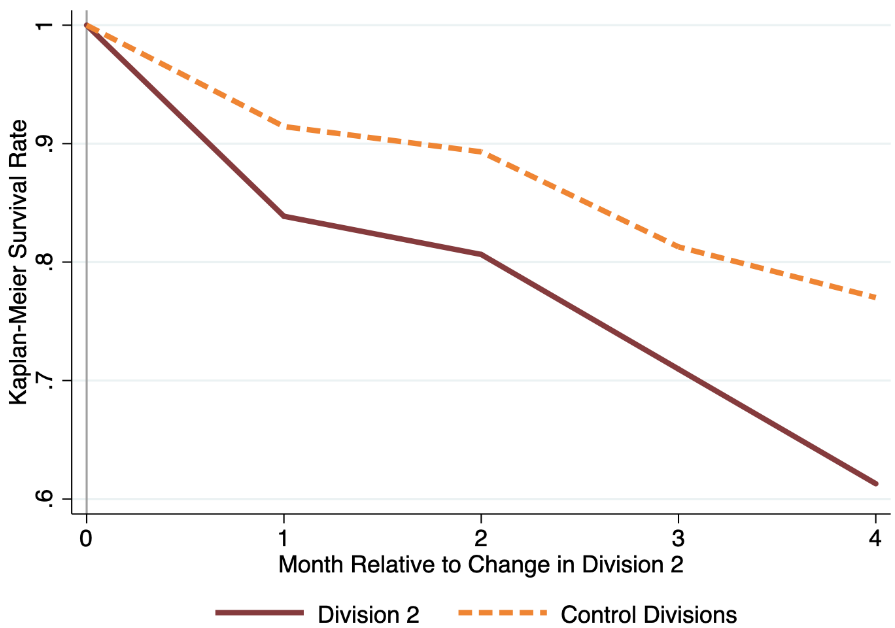

*Notes:* Figure (a) plots the average weekly commission pay levels for agents in Division 2 and the control divisions. The solid vertical line corresponds to the week immediately before the week of the commission schedule changes in Division 2. Figure (b) plots the difference-in-differences coefficients that capture differential trends in commission pay levels and total call volume between Division 2 and the control divisions.

**Figure A.6 Survival Rates in Division 2 and the Control Divisions**

(a) **Survival Rates Relative to Month -5**

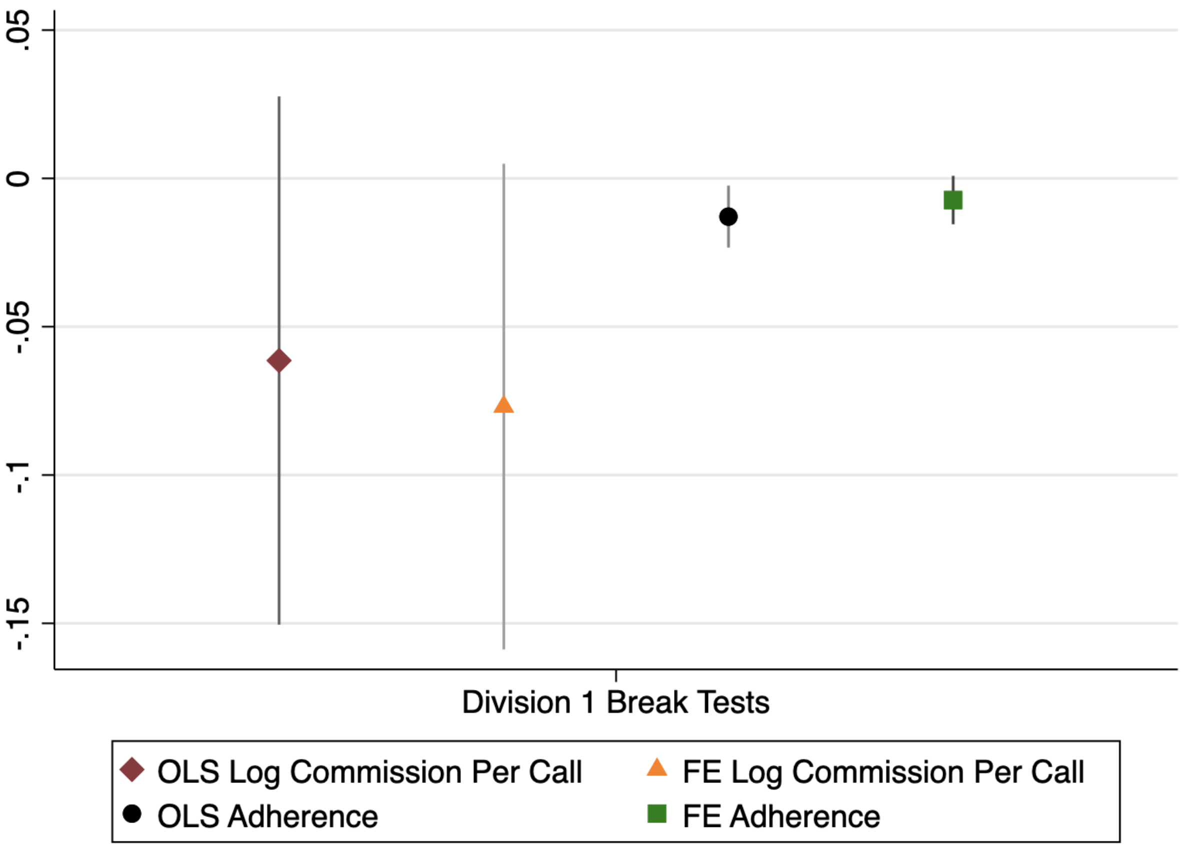



(b) **Survival Rates Relative to Month 0**




*Notes:* These figures plot Kaplan-Meier survival rates over time. The survival rate estimator considers a starting point and then, from that time, displays the fraction of agents that remain at the firm. The graphical properties of the cumulative survival rate allow an assessment of when retention diverges over time and what fraction of the total beginning workforce is affected. Because turnover can be lumpy, with multiple exits in some weeks and no exits in others, we aggregate survival rates to the monthly level.

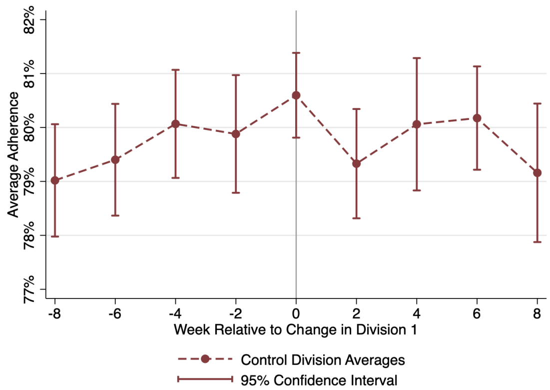
**Figure A.7 Structural Break Tests in the Control Divisions**



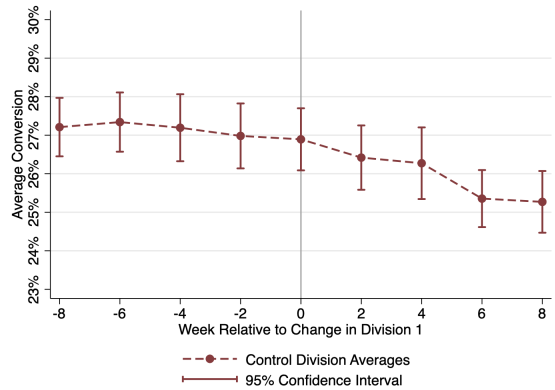

*Notes:* These structural break tests come from regressions using the control sample. The figure reports the post-treatment indicator parameter estimates and the corresponding confidence intervals. The dependent variable is in the legend, and each regression includes a post-treatment indicator for Division 1, the matrix of agent characteristics $X_{it}$, division fixed effects, and trends for each division. Specifications with "FE" add individual fixed effects.

**Figure A.8 Trends in Observable Outcomes for Control Divisions**

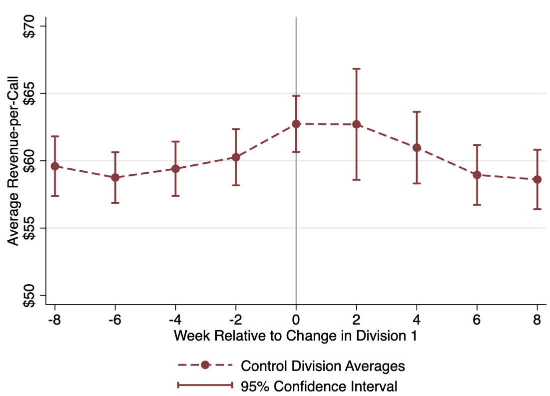
**(a) Trends in Adherence**



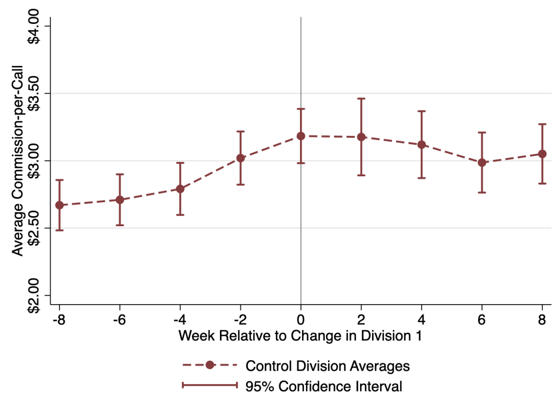

**(b) Trends in Conversion**




**(c) Trends in Revenue-per-Call**




**(d) Trends in Commission-per-Call**




*Notes:* These figures show raw averages in different outcome variables for the control divisions. Adherence and conversion are the two proxies for an agent's supply of effort. Revenue-per-call and commission-per-call are two additional measure of output. To improve the readability of these figures, we aggregate data into bi-weekly clusters. Confidence intervals are based on the standard errors of the means.

**Table A.1 Summary Statistics for Division 1 By Productivity Level**

| | Tercile of Adjusted Worker Fixed Effects | | |
|---|---|---|---|
| | Bottom Third | Middle Third | Top Third |
| | (1) | (2) | (3) |
| Commission | 170.88 | 298.02 | 476.63 |
| | (167.92) | (210.59) | (342.00) |
| Predicted Pct $\Delta$ Commission Post-Treatment | -0.18 | -0.18 | -0.17 |
| | (0.04) | (0.03) | (0.03) |
| Adherence | 0.81 | 0.84 | 0.84 |
| | (0.15) | (0.10) | (0.09) |
| Conversion | 0.29 | 0.34 | 0.37 |
| | (0.09) | (0.08) | (0.09) |
| Log $\text{RPC}_{Old}$ | 3.93 | 4.24 | 4.37 |
| | (0.59) | (0.41) | (0.41) |
| Log $\text{RPC}_{New}$ | 3.69 | 4.00 | 4.15 |
| | (0.59) | (0.40) | (0.40) |
| Phone Hours | 18.41 | 21.26 | 22.99 |
| | (7.32) | (7.02) | (6.26) |
| Total Calls | 64.95 | 72.42 | 77.55 |
| | (25.86) | (25.75) | (25.00) |
| Tenure (days) | 149.69 | 215.07 | 691.07 |
| | (67.70) | (110.89) | (485.51) |
| Age | 23.12 | 23.67 | 28.14 |
| | (4.78) | (3.77) | (8.48) |
| Single | 0.79 | 0.76 | 0.58 |
| | (0.41) | (0.43) | (0.50) |
| White | 0.73 | 0.70 | 0.73 |
| | (0.44) | (0.46) | (0.44) |
| Male | 0.68 | 0.78 | 0.73 |
| | (0.47) | (0.41) | (0.45) |
| Survey Response to Firm Fairness | 0.57 | 0.48 | 0.35 |
| | (0.50) | (0.50) | (0.48) |
| Survey Response to Referral Likelihood | 0.73 | 0.58 | 0.52 |
| | (0.45) | (0.49) | (0.50) |
| Survey Response to Promotion Likelihood | 0.55 | 0.83 | 0.59 |
| | (0.50) | (0.38) | (0.49) |
| Agent-Weeks | 249 | 292 | 297 |
| Agents | 40 | 40 | 40 |

*Notes:* This table presents summary statistics for Division 1 using data eight weeks prior to the commission schedule changes. Each column represents an approximate tercile of the distribution of adjusted worker fixed effects in the pre-treatment period. Adjusted worker fixed effects are calculated from a regression of log commissions on worker dummy variables, division-by-week dummy variables, and a cubic spline in tenure. We then correct for sampling variation using the method in Lazear et al. (2015). We are not able to estimate adjusted worker fixed effects for every agent, resulting in slightly smaller agent and agent-week counts compared to those in Table 1. The *Predicted Percentage $\Delta$ Commission Post-Treatment* is a calculation of how total commissions would decline for each agent due to the commission schedule changes as a function of the pre-treatment sales mix of products observed for that agent. For Division 2, see Table A.9 in the Appendix.

**Table A.2 Linear Probability Model Estimates of Turnover Responses (Low-Ordered Polynomials)**

| | Last Week in Firm | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated x Post x Prod | 0.021** | 0.015** | 0.016* | 0.012** | 0.012* |
| | (0.007) | (0.005) | (0.006) | (0.005) | (0.005) |
| Treated x Post | -0.006 | -0.006 | | -0.002 | |
| | (0.004) | (0.007) | | (0.010) | |
| Treated x Placebo x Prod | -0.006 | | -0.002 | | |
| | (0.004) | | (0.004) | | |
| Treated x Placebo | 0.000 | | | | |
| | (0.004) | | | | |
| Week Fixed Effects | ✓ | ✓ | | ✓ | |
| Division x Week-of-Year Fixed Effects | | ✓ | | | |
| Week x Division Fixed Effects | | | ✓ | | ✓ |
| Post-Territory Shock Period | | | | ✓ | ✓ |
| Observations | 51,497 | 51,497 | 51,497 | 19,689 | 19,689 |
| Mean Turnover Probability in Division 1 | | | 0.037 | | |

*Notes:* The dependent variable is an indicator that equals one if it is the worker's last week at the firm. The sample includes all current employees in Division 1 and the control divisions with non-missing data. Estimates come from a linear probability model that captures changes in the turnover probability for the existing workforce. These models include only $Age$, $Age^2$, $Tenure$, and $Tenure^2$, removing the higher-ordered polynomial terms on *Age* and *Tenure*. *Prod* refers an agent's sales $z$-score, which is the standardized measure of an agent's pre-treatment productivity estimated as their adjusted worker fixed effect according to the procedure in Lazear et al. (2015). For additional details, see Section 3.5. The specification in Column 2 includes division by week-of-year fixed effects to account for seasonality. The specification in Column 3 includes week by division fixed effects. Columns 4 and 5 use a shortened pre-treatment period that only includes the weeks of data after the territory shock period. *Placebo* is an indicator for the date 52 weeks prior to the treatment date.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

**Table A.3 Estimates of Effort Responses Using the Full Pre-Treatment Sample**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Panel A: Adherence to Schedule | | | | | | | |
| Treated x Post | -0.002 | 0.001 | 0.007 | 0.014 | 0.005 | 0.008 | |
| | (0.006) | (0.004) | (0.008) | (0.012) | (0.009) | (0.007) | |
| Treated x Post x Prod | | | | | | -0.003 | -0.003 |
| | | | | | | (0.004) | (0.004) |
| Observations | 33,068 | 33,068 | 33,068 | 14,775 | 14,491 | 33,068 | 33,068 |
| Panel B: Conversion Rate | | | | | | | |
| Treated x Post | 0.005 | 0.001 | 0.006 | 0.001 | 0.005 | 0.011 | |
| | (0.006) | (0.005) | (0.005) | (0.005) | (0.006) | (0.005) | |
| Treated x Post x Prod | | | | | | -0.017*** | -0.018*** |
| | | | | | | (0.004) | (0.003) |
| Observations | 33,044 | 33,044 | 33,044 | 13,469 | 13,981 | 33,044 | 33,044 |
| Panel C: Log RPC at Old Prices | | | | | | | |
| Treated x Post | 0.076 | 0.053 | -0.006 | -0.006 | -0.003 | 0.005 | |
| | (0.046) | (0.033) | (0.033) | (0.034) | (0.041) | (0.034) | |
| Treated x Post x Prod | | | | | | -0.054* | -0.044 |
| | | | | | | (0.027) | (0.023) |
| Observations | 35,366 | 35,366 | 35,366 | 15,077 | 15,071 | 35,366 | 35,366 |
| Panel D: Log RPC at New Prices | | | | | | | |
| Treated x Post | 0.069 | 0.061* | 0.063 | 0.037 | 0.062 | 0.077* | |
| | (0.043) | (0.030) | (0.032) | (0.032) | (0.041) | (0.033) | |
| Treated x Post x Prod | | | | | | -0.061* | -0.049* |
| | | | | | | (0.026) | (0.022) |
| Observations | 35,366 | 35,366 | 35,366 | 15,077 | 15,071 | 35,366 | 35,366 |
| Week Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Agent Fixed Effects | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Division Trend Controls | | | ✓ | ✓ | ✓ | ✓ | |
| Week x Division Fixed Effects | | | | | | | ✓ |
| Re-Weighted | | | | ✓ | | | |
| Balanced Sample | | | | | ✓ | | |

*Notes:* This table is an analog of Table 3. The sample includes all current employees in Division 1 and the control divisions with non-missing data. The models in Columns 1–6 include fixed effects for week, division, and office location. All models include cubic splines for tenure and a cubic polynomial in age. The OLS regression in Column 1 includes dummies for ethnicity, gender, and marital status. The specifications in Columns 2–7 include individual fixed effects. Columns 3–6 include division-specific trend controls. The specification in Column 4 uses a re-weighting estimator based on the propensity score for being in Division 1 (see Appendix C.1). The balanced panel in Column 5 restricts to workers who are present prior to July, 2016 and after April, 2017. Columns 6 and 7 consider heterogeneous responses based on worker productivity, and Column 7 omits week fixed effects and division-specific trend controls and instead includes week by division fixed effects. Differing numbers of observations across panels reflect differences in data availability. The sample include all pre- and post-treatment period data in the immediate sample. Reported standard errors are clustered by manager.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

**Table A.4 Estimates of Effort Responses (Log RPH and RPC Levels)**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Panel A: Log RPH at Old Prices | | | | | | | |
| Treated x Post | 0.051 | 0.041 | 0.035 | 0.007 | 0.053 | 0.036 | |
| | (0.046) | (0.035) | (0.046) | (0.044) | (0.059) | (0.046) | |
| Treated x Post x Prod | | | | | | -0.016 | -0.017 |
| | | | | | | (0.032) | (0.032) |
| Observations | 9,145 | 9,145 | 9,145 | 7,761 | 4,075 | 9,145 | 9,145 |
| Panel B: Log RPH at New Prices | | | | | | | |
| Treated x Post | 0.102* | 0.086* | 0.057 | 0.026 | 0.065 | 0.061 | |
| | (0.047) | (0.035) | (0.045) | (0.044) | (0.062) | (0.045) | |
| Treated x Post x Prod | | | | | | -0.024 | -0.024 |
| | | | | | | (0.032) | (0.032) |
| Observations | 9,145 | 9,145 | 9,145 | 7,761 | 4,075 | 9,145 | 9,145 |
| Panel C: Level RPC at Old Prices | | | | | | | |
| Treated x Post | -5.012 | -3.381 | -1.155 | -2.117 | -1.108 | 0.885 | |
| | (2.614) | (2.015) | (2.593) | (3.091) | (3.596) | (2.491) | |
| Treated x Post x Prod | | | | | | -4.362** | -4.377** |
| | | | | | | (1.554) | (1.599) |
| Observations | 9,229 | 9,229 | 9,229 | 7,840 | 4,126 | 9,229 | 9,229 |
| Panel D: Level RPC at New Prices | | | | | | | |
| Treated x Post | -1.426 | 0.057 | -0.068 | -1.407 | -0.781 | 1.813 | |
| | (2.179) | (1.775) | (2.258) | (2.907) | (3.544) | (2.211) | |
| Treated x Post x Prod | | | | | | -4.199** | -4.181** |
| | | | | | | (1.457) | (1.493) |
| Observations | 9,229 | 9,229 | 9,229 | 7,840 | 4,126 | 9,229 | 9,229 |
| Week Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | |
| Agent Fixed Effects | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Division Trend Controls | | | ✓ | ✓ | ✓ | ✓ | |
| Week x Division Fixed Effects | | | | | | | ✓ |
| Re-Weighted | | | | ✓ | | | |
| Balanced Sample | | | | | ✓ | | |

*Notes:* The sample includes all current employees in Division 1 and the control divisions with non-missing data. The models in Columns 1–6 include fixed effects for week, division, and office location. All models include cubic splines for tenure and a cubic polynomial in age. The OLS regression in Column 1 includes dummies for ethnicity, gender, and marital status. The specifications in Columns 2–7 include individual fixed effects. Columns 3–6 include division-specific trend controls. The specification in Column 4 uses a re-weighting estimator based on the propensity score for being in Division 1 (see Appendix C.1). The balanced panel in Column 5 restricts to workers who are present prior to July, 2016 and after April, 2017. Columns 6 and 7 consider heterogeneous responses based on worker productivity, and Column 7 omits week fixed effects and division-specific trend controls and instead includes week by division fixed effects. The sample used restricts to eight weeks of pre-treatment data and eight weeks of post-treatment data. Reported standard errors are clustered by manager.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

**Table A.5 Sentiment Descriptive Statistics**

| | Pre-Treatment Sentiment | | | | |
|---|---|---|---|---|---|
| | All | 0%–33% | 33%–66% | 66%–100% | Diff. |
| | (1) | (2) | (3) | (4) | (4)–(2) |
| ΔFairness Perceptions | -1.43 | 14.17** | -12.25** | -5.74** | -19.91*** |
| | (2.74) | (5.14) | (4.51) | (2.37) | (5.66) |
| ΔReferral Likelihood | -12.51*** | -9.09* | -11.14** | -16.36*** | -7.27 |
| | (2.90) | (4.87) | (4.44) | (5.38) | (7.40) |
| ΔPromotion Prospects | -0.17** | 0.09 | 0.04 | -0.57*** | -0.66*** |
| | (0.07) | (0.06) | (0.11) | (0.14) | (0.17) |
| Agents | 70 | 23 | 24 | 23 | |

*Notes:* This table documents the changes in the self-reported sentiment levels of Division 1 agents from before to after the commission schedule changes. The data is split across terciles of pre-treatment sentiment where Column 2 contains agents with the lowest sentiment and Column 4 contains agents with the highest sentiment. The results of difference-in-means tests between Columns 4 and 2 are reported in the far right column. Standard errors of means are reported in parentheses.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

**Table A.6 Heterogeneous Turnover Responses Based on Worker Sentiment**

| | Last Week in Firm | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated x Post x Prod | 0.012 | 0.014** | 0.010 | 0.013* | 0.013* |
| | (0.006) | (0.005) | (0.007) | (0.005) | (0.005) |
| Treated x Post x Firm Fair | 0.009 | 0.009 | 0.009 | 0.013 | 0.012 |
| | (0.009) | (0.009) | (0.009) | (0.008) | (0.008) |
| Treated x Post x High Refer | 0.005 | 0.004 | 0.004 | 0.005 | 0.004 |
| | (0.007) | (0.006) | (0.007) | (0.006) | (0.007) |
| Treated x Post x Promotion | -0.011 | -0.011 | -0.011 | -0.011 | -0.010 |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| Week Fixed Effects | ✓ | ✓ | | ✓ | |
| Division x Week-of-Year Fixed Effects | | ✓ | | | |
| Week x Division Fixed Effects | | | ✓ | | ✓ |
| Shortened Pre-Period | | | | ✓ | ✓ |
| Observations | 51,497 | 51,497 | 51,497 | 19,689 | 19,689 |
| Mean Turnover Probability in Division 1 | | | 0.037 | | |
| $p$-value on Treated x Post x Prod | 0.039 | 0.040 | 0.164 | 0.050 | 0.060 |

*Notes:* The dependent variable is an indicator that equals one if it is the worker's last week at the firm. The sample includes all current employees in Division 1 and the control divisions with non-missing data. Estimates come from a linear probability model that captures changes in the turnover probability for the existing workforce. Each model includes a 5th order polynomial for workers' tenure to account for a potentially arbitrary baseline relationship between tenure and turnover. *Prod* refers an agent's sales $z$-score, which is the standardized measure of an agent's pre-treatment productivity estimated as their adjusted worker fixed effect according to the procedure in Lazear et al. (2015). For additional details, see Section 3.5. We separately interact the treatment indicator with indicators for high firm fairness perceptions, high referral likelihood, and a belief that promotion is likely. An agent's firm fairness perception is marked as high if it is above the median value. Referral likelihood is marked is high if it is above the median value. If an agent says they are likely to be promoted in the future, their promotion likelihood indicator equals one. The specification in Column 2 includes division by week-of-year fixed effects to account for seasonality. The specification in Column 3 includes week by division fixed effects. Columns 4 and 5 use a shortened pre-treatment period that only includes the weeks of data after the territory shock period. Two forms of inference are presented, one using standard errors clustered by manager (see parentheses) and the second using p-values with division-level clusters (see the final two lines) computed using the wild cluster bootstrap randomization inference procedure in MacKinnon and Webb (2018). We use the t-statistic version of the procedure that imposes the null hypothesis.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

**Table A.7 Heterogeneous Effort Responses Based on Worker Sentiment**

| | Adherence to Schedule | Conversion Rate | Log RPC at Old Prices | Log RPC at New Prices |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated x Post x Prod | -0.004 | -0.021*** | -0.046* | -0.046* |
| | (0.006) | (0.005) | (0.019) | (0.018) |
| Treated x Post x Firm Fair | -0.007 | 0.004 | -0.001 | 0.005 |
| | (0.016) | (0.010) | (0.064) | (0.060) |
| Treated x Post x High Refer | -0.001 | -0.013 | -0.067 | -0.062 |
| | (0.013) | (0.008) | (0.049) | (0.044) |
| Treated x Post x Promotion | 0.001 | 0.005 | 0.070 | 0.085 |
| | (0.014) | (0.011) | (0.062) | (0.051) |
| Agent Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Week x Division Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Observations | 8,647 | 8,283 | 9,229 | 9,229 |

*Notes:* The sample includes all current employees in Division 1 and the control divisions with non-missing data. All models include agent fixed effects and fixed effects for division and office location. To account for experience effects, all models include cubic splines for tenure with the firm and a cubic polynomials in age. Each specification also includes week by division fixed effects. We separately interact the treatment indicator with indicators for high firm fairness perceptions, high referral likelihood, and a belief that promotion is likely. An agent's firm fairness perception is marked as high if it is above the median value. Referral likelihood is marked is high if it is above the median value. If an agent says they are likely to be promoted in the future, their promotion likelihood indicator equals one. Differing numbers of observations across columns reflect differences in data availability. The sample used restricts to eight weeks of pre-treatment data and eight weeks of post-treatment data. The results are similar when all available pre- and post-treatment data is used. Reported standard errors are clustered by manager.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

**Table A.8 Linear Probability Model Estimates of Turnover Responses in Division 2**

| | Last Week in Firm | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated x Post x Prod | -0.011 | 0.002 | -0.008 |
| | (0.007) | (0.007) | (0.007) |
| Treated x Post | 0.013** | 0.023* | |
| | (0.004) | (0.010) | |
| Time Fixed Effects | ✓ | ✓ | |
| Division x Week-of-Year Fixed Effects | | ✓ | |
| Time x Division Fixed Effects | | | ✓ |
| Observations | 45,328 | 45,328 | 45,328 |
| Mean Turnover Prob in Treated Division | | 0.008 | |
| $p$-value on Treated x Post x Prod | 0.236 | 0.682 | 0.436 |
| $p$-value on Treated x Post | 0.131 | 0.377 | |

*Notes:* The dependent variable is an indicator that equals one if it is the worker's last week at the firm. The sample includes all current employees in Division 2 and the control divisions with non-missing data. Estimates come from a linear probability model that captures changes in the turnover probability for the existing workforce. Each model includes a 5th order polynomial for workers' tenure to account for a potentially arbitrary baseline relationship between tenure and turnover. *Prod* refers to an agent's sales $z$-score, which is the standardized measure of an agent's pre-treatment productivity estimated as their adjusted worker fixed effect according to the procedure in Lazear et al. (2015). For additional details, see Section 3.5. The specification in Column 2 includes division by week-of-year fixed effects to account for seasonality. The specification in Column 3 includes week by division fixed effects. Two forms of inference are presented, one using standard errors clustered by manager (in parentheses) and the second using p-values with division-level clusters (see the final two lines) computed using the wild cluster bootstrap randomization inference procedure in MacKinnon and Webb (2018). We use the t-statistic version of the procedure that imposes the null hypothesis.

$^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

**Table A.9 Summary Statistics for Division 2 By Productivity Level**

| | Adjusted Worker Fixed Effects | | |
|---|---|---|---|
| | Bottom Third | Middle Third | Top Third |
| | (1) | (2) | (3) |
| Commission | 306.13 | 496.85 | 717.00 |
| | (206.50) | (316.55) | (345.83) |
| RPC | 73.85 | 101.41 | 129.99 |
| | (36.80) | (44.14) | (46.00) |
| Adherence | 0.75 | 0.78 | 0.81 |
| | (0.21) | (0.15) | (0.10) |
| Conversion | 0.23 | 0.29 | 0.34 |
| | (0.11) | (0.11) | (0.12) |
| Phone Hours | 16.96 | 17.70 | 17.45 |
| | (6.68) | (6.61) | (5.39) |
| Total Calls | 52.46 | 45.53 | 47.69 |
| | (22.64) | (17.86) | (14.23) |
| Tenure (days) | 324.13 | 679.67 | 1386.26 |
| | (93.80) | (356.30) | (414.86) |
| Age | 26.16 | 31.14 | 32.51 |
| | (4.00) | (8.52) | (9.94) |
| Single | 0.77 | 0.51 | 0.30 |
| | (0.42) | (0.50) | (0.46) |
| White | 0.92 | 0.25 | 0.74 |
| | (0.28) | (0.43) | (0.44) |
| Male | 0.75 | 0.75 | 0.61 |
| | (0.44) | (0.43) | (0.49) |
| Survey Response to Firm Fairness | 0.22 | 0.45 | 0.10 |
| | (0.42) | (0.50) | (0.30) |
| Survey Response to Referral Likelihood | 0.63 | 0.63 | 0.54 |
| | (0.49) | (0.49) | (0.50) |
| Survey Response to Promotion Likelihood | 0.66 | 0.57 | 0.46 |
| | (0.48) | (0.50) | (0.50) |
| Agent-Weeks | 95 | 97 | 90 |
| Agents | 13 | 13 | 12 |

*Notes:* This table presents cross-sectional summary statistics for Division 2 using data eight weeks prior to the Division 2 commission schedule changes. Each column represents an approximate tercile of the distribution of adjusted worker fixed effects in the pre-treatment period. Adjusted worker fixed effects are calculated from a regression of log commissions on worker dummy variables, division-by-week dummy variables, and a cubic spline in tenure. We then correct for sampling variation using the method in Lazear et al. (2015).

**Table A.10 Illustration of Commission Changes**

| | Pre-Change | Post-Change | Difference (2)−(1) | Commissions (3) × 10% |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| One of Three Products | | | | |
| Transfer Price per Sale | \$15 | \$10 | -\$5 | |
| Avg. Sales per Agent-Week | 39.9 | 38.5 | -1.40 | |
| Avg. Revenue per Agent-Week | \$598.50 | \$385.00 | -\$213.50 | -\$21.35 |
| Bundle of Two Products | | | | |
| Transfer Price per Sale | \$50 | \$25 | -\$25 | |
| Avg. Sales per Agent-Week | 7.56 | 4.97 | -2.59 | |
| Avg. Revenue per Agent-Week | \$378.00 | \$124.25 | -\$253.75 | -\$25.38 |
| Bundle of Three Products | | | | |
| Transfer Price per Sale | \$100 | \$125 | \$25 | |
| Avg. Sales per Agent-Week | 5.33 | 5.99 | 0.66 | |
| Avg. Revenue per Agent-Week | \$533.00 | \$748.75 | \$215.75 | \$21.58 |
| Total per Agent-Week | \$1,509.50 | \$1,258.00 | -\$251.50 | -\$25.15 |

*Notes:* The purpose of this table is to better highlight some of the details of the commission schedule changes. We display agent-week level sales averages and transfer prices for different bundles of three separate products. While we have a partial record of the products sold, we do not have any way of knowing what products customers initially sought out when they called. As a result, we are unable to measure product-level conversion rates. Columns (1) and (2) show revenue transfer prices, average sales per agent-week, and average revenue per agent-week for different product bundles in the pre- and post-treatment periods, respectively. Column (3) displays the differences in transfer prices, average sales per agent-week, and average revenue per agent-week between between these two periods. Column (4) multiplies this difference by a hypothetical commission rate of 10% (which is at the top end of the commission rate distribution). As mentioned in Section A.3, agents' commission rates were not mechanically changed, so an agent with a commission rate of 10% in the pre-treatment period likely maintained this commission rate in the post-treatment period.