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HOW MUCH DOES YOUR BOSS MAKE? THE EFFECTS OF SALARY COMPARISONS

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ABSTRACT

The vast majority of the pay inequality in an organization comes from differences in pay between employees and their bosses. But are employees aware of these pay disparities? Are employees demotivated by this inequality? To address these questions, we conducted a field experiment with a sample of 2,060 employees from a multibilliondollar corporation. We make use of the firm's administrative records alongside survey data and information-provision experiments. First, we document large misperceptions among the employees about the salaries of their managers and smaller but still significant misperceptions of the salaries of their peers. Second, we show that these perceptions have a significant causal effect on the employees' own behavior. When they find out that their managers earn more than they thought, employees work harder on average. We provide evidence that these effects are consistent with career-concerns models. In contrast, employees work less hard when they find out that their peers earn more. We conclude by discussing implications for pay inequality and pay transparency.

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A data appendix is available at http://www.nber.org/data-appendix/w24841

1 Introduction

The vast majority of the pay inequality within organizations comes from salary disparities between employees and their bosses. We often hear about this inequality from the media, politicians, and policy makers. It is unclear, however, whether employees are aware of the pay inequality in their own firms. Pay secrecy policies, for example, may prevent employees from finding out how much their managers are making. Even if employees knew that their managers earned substantially more, it is unclear how that knowledge would affect employees' behavior. On the one hand, discovering that managers earn a lot may demoralize employees as a result of social preferences; they may feel jealous or resentful, or perhaps feel the pay disparity is unfair. On the other hand, employees may find well-paid managers to be a source of motivation. According to models of career concerns, knowing their managers' salaries should give employees an extra incentive to work harder in hopes of being promoted to a managerial position with its large reward.

Are employees aware of how much their bosses get paid? And if employees believe their managers are paid handsomely, does it inspire them to greater effort or does it sap their motivation? In this study, we address these questions using a large-scale, high-stakes field experiment in collaboration with a multibillion-dollar corporation. We study these questions using a research design that combines administrative data, incentivized surveys, information-acquisition, and information-provision experiments.

The first part of the research design focuses on the employees' perceptions of the salaries in managerial positions. We define a managerial position as a higher position, typically a position the employee would like to be promoted to, and a position that grants some power over the employee, such as in the roles of assigning tasks and reviewing performance. A junior analyst might be managed by a senior analyst, for example. As a benchmark for perceptions of manager salaries, we also study employees' perceptions of the average salaries of their peers. We define peers as employees who have the same title and are from the same unit. For example, the peers of a junior analyst would be the other junior analysts in their unit.

We refer to comparisons between employees' own salaries and the salaries of their managers as vertical comparisons, and to comparisons between employees' own salaries and the salaries of their peers as horizontal comparisons. In this firm, as in most organizations, the level of vertical inequality dwarfs that of horizontal inequality. The average manager salary typically (i.e., 90% of the time) ranges between +114% and +634% of employees' salaries. In comparison, the peer average salary typically ranges between -16% and +16% of employees' salaries.

Our survey elicits the respondents' beliefs about average manager and peer salaries. It incentivizes these and other responses to ensure that it is in the best interest of participants to give thoughtful honest answers. We compare the respondents' guesses to the actual figures from the firm's administrative records to measure the respondents' misperceptions about salaries at the company. Our research design also sheds light on the nature of these misperceptions. To assess whether employees are aware of their misperceptions, we measure their level of certainty. And to assess whether they even care about others' salaries, we measure their willingness to pay for information, using an incentive-compatible method (Becker et al., 1964).

The second part of the research design explores the impact of salary perceptions on the employees' own behavior. To identify these causal effects, we created exogenous variation in salary perceptions through an information-provision experiment. After eliciting their prior beliefs about the manager's salary, we randomized whether the employee would receive a piece of information about that salary. We then re-elicited beliefs about the manager's salary to verify that the information provided affected their beliefs. Most important, we can use the rich administrative records of the firm to measure whether these exogenous shocks to perceived manager salary translate into differences in behavior in the months after the employee participated in the survey. We can test if a higher perceived manager salary translates into a lower effort, as predicted by social preferences, or into a higher effort, as predicted by the career-concerns models.

The close collaboration with the firm, along with its rich technological infrastructure, allowed us to use the company's administrative data to measure the effects of the information experiment on a number of different forms of behavior. We obtained data on two forms of effort: the number of hours an employee spent in the office (based on security data on all of the swipes in and out of the building) and the number of emails sent by the employees (based on data from the email servers). We also acquired data on one measure of performance (sales revenue, for employees who have sales roles) and a set of career outcomes, such as whether the employee left the company, was transferred internally, or received a raise.

There are some additional features of the experiment that were designed to provide evidence about mechanisms. According to the career-concerns model, the effects of manager salary should depend on whether the employee can aspire to be promoted to that position or not. To test this hypothesis, we pick a different managerial position for different subjects: for example, we ask some junior analysts in investment banking about the average salary of senior analysts (a few promotions away), while we ask other junior analysts about the average salary of the chief economist (a higher number of promotions away). To complement this data, we elicit the respondent's own perceptions of the number of promotions they would need to attain the managerial position, and the likelihood that they will be promoted to that position in the next five years.

We included a series of additional questions at the end of survey, after the information-provision experiments. By measuring the effects of the information provision on these survey outcomes we can generate suggestive evidence about some of the mechanisms at play. According to the career-concerns model, employees who learn that their managers are paid handsomely should become more optimistic about their own future salary prospects. To test this hypothesis, we elicit expectations about the respondent's own future salary, using an incentive-compatible method. Similarly, to examine the role of social preferences, we included questions related to employee morale (job satisfaction and pay satisfaction) and attitudes towards pay inequality.

To shed light on the potential sources of information frictions, we rolled out the survey gradually over the course of two months. The staggered nature of the survey allows us to measure not only whether the information provided to an employee affects the employee's own salary perceptions, but also whether that information diffuses to other employees connected to the participants who received information.

We conducted the field experiment with a sample of 2,060 employees from a large commercial bank (referred to hereinafter as *the firm*) with thousands of employees, millions of customers, and billions of dollars in revenues. The firm is typical in key aspects. For example, the horizontal and vertical differences in salaries line up quite well with those of similar-sized organizations in other countries (including, but not limited to, the United States). Other salient issues include the fact that employees' employment contracts explicitly prohibit them from disclosing their salaries to others, and also that employees reportedly rarely discuss salaries with their coworkers but do desire more salary transparency. Several studies show that these three facts are common in organizations from several countries, again including but not limited to the United States (Trachtman, 1999; Edwards, 2005; Hegewisch et al., 2011; Glassdoor, 2016; PayScale, 2018).

In the first set of results, we show that employees do have large misperceptions of the salaries of their managers. When comparing employees' guesses to the administrative records of the firm, we find that employees' guesses about the average manager salary have a mean absolute error of 28%. Moreover, there is a systematic bias: on average, employees underestimate the salary of their managers by 14.1%. As a benchmark, the misperceptions of peer salaries are still significant, but not as large: the mean absolute error is 11.5% and there is no systematic bias. We show that these misperceptions do not result from lack of interest: employees are aware of their misperceptions and also willing to pay days' and even weeks' worth of salary for a piece of information about the salary of their managers (or their peers).

When forming beliefs, employees put significant weight on the salary information we provided to them. For instance, a simple Bayesian learning model indicates that employees put a 69% weight on the signal about manager salary we provided and only 31% on their prior beliefs. In addition to measuring whether employees incorporate the information given to them, we can measure whether that information subsequently diffuses to other employees. We find no evidence of information diffusion. The information provided to an employee does not travel to other employees in the network, not even to the employee's closest peer. Indeed, the lack of information diffusion is also supported by non-experimental data. Contrary to the predictions of models of social learning (Mobius and Rosenblat, 2014), employees who are more central in the network and employees who report having discussed salaries do not have more accurate beliefs about the salaries of their peers or managers. We also find that employees are not more accurate at guessing peer salaries than would be expected if they were using their own salaries as their guesses, suggesting they lack any useful information beyond that.

In the second set of results, we use the exogenous variation in perceptions induced by the information experiment to estimate the causal effects of salary perceptions on the employees' own behavior. We estimate these effects using a simple Instrumental Variables model. The intuition behind this model is straightforward. Consider two respondents who, at the start of the survey, both underestimate the average manager salary by 20%. By chance, one of those respondents then receives a highly accurate signal about the average manager salary, while the other one receives no information at all. Based on the observed rates of updating, the employee who received no information continues to underestimate the average manager salary by 20%, while the employee who did receive the information underestimates the manager salary less – by 10%, let's say. As a result, the information treatment amounts to a positive shock of 10% to the perceived manager salary. We then can measure how this 10% shock to that employee's perceived manager salary affects her subsequent behavior. The Instrumental Variables model simply extends this logic to all of the respondents, not only the ones who underestimate the average salary by 20%.

We find that a higher perceived manager salary increases effort and performance. We estimate that a 10% increase in perceived manager salary increases the average hours worked in the subsequent 90 days by 1.5%, implying a behavioral elasticity of 0.15 (p-value=0.042). The corresponding effects on the other measures of effort and performance are similar in magnitude: elasticities of 0.130 in the number of emails sent (p-value=0.001) and 0.106 (p-value= 0.383) in sales performance.

The effects of the horizontal comparison, on the other hand, are the opposite of those of the vertical comparison: a 10% increase in employees' perception of their peers' salaries would decrease the number of hours they work by 9.4%, implying a behavioral elasticity of -0.94 (p-value= 0.045), with corresponding elasticities of -0.431 (p-value=0.041) in emails sent and -0.731 (p-value=0.014) in sales performance. Indeed, we can confidently reject the null hypothesis that the effects of horizontal and vertical comparisons are equal to each other: p-value=0.026 for hours worked, p-value=0.007 for emails sent, and p-value<0.001 for sales.

These results are robust to a number of checks. For example, we conduct falsification regressions in an event-study fashion. As expected, we find that the information treatments have no "effects" on pre-treatment behavior. We show that the effects are present even when using a longer time horizon (180 days after the survey) than in the baseline specification (90 days). And we provide the standard sensitivity checks, such as using binned scatterplots to detect any outliers, asymmetries, or nonlinearities.

We provide evidence about the mechanisms underlying the effects of salary comparisons. The positive effect of perceived manager salary on effort suggests that the career-concerns mechanism, which predicts positive effects, dominates over the social preferences channel, which predicts negative effects. Moreover, we provide more direct evidence of the mechanisms at play using two different strategies.

The first strategy consists of measuring the effects of the salary information on survey outcomes. We show that, as is consistent with the career-concerns model, when employees learn that their managers earn more, they become more optimistic about what their own salaries will be five years in the future. On the other hand, and contrary to what the social preferences channel would predict, we do not find any evidence that perceptions of manager salary have any effect on measures of employee morale (pay satisfaction and job satisfaction) or tolerance for pay inequality. In contrast, these same survey outcomes suggest that social preferences may be at play in peer comparisons: a higher perceived peer salary does have negative effects on employee morale (pay and job satisfaction) as well as on tolerance for pay inequality.

A second strategy exploits heterogeneity in the distance between the employee's own position and the managerial position. As is consistent with the career-concerns model, we find that the effects of the perceived manager salary are stronger for managerial positions that the employee can aspire to attain. When employees find out that managers who are a few promotions away earn more, they expect higher salaries in five years and they work harder. In contrast, when employees find about the high salaries of managerial positions they cannot aspire to, the effects are close to zero and statistically insignificant.

Our findings have implications for organizations. First, they show that, due to salary comparisons, changing the salary of one employee can affect the behavior of other employees in the same firm. These externalities should be taken into account when designing compensation incentives inside organizations. Indeed, our finding that horizontal and vertical salary comparisons have markedly different effects may help explain the somewhat puzzling fact that firms tend to load incentives vertically, in the form of promotions, rather than horizontally, such as through pay-for-performance (Baker et al., 1988).

Our findings also have implications for policy makers. Our evidence runs counter to the widespread view that social preferences tend to compress pay inequality within the firm (Frank, 1984). For example, policy makers promote transparency policies, such as disclosure of CEO pay (Faleye et al., 2013; Mas, 2016; Mueller et al., 2017), with the hope of putting pressure on firms to reduce pay inequality. Our findings suggest that these policies are unlikely to generate pressure from employees to reduce vertical inequality, which accounts for the vast majority of pay inequality. Pay transparency can still be useful to induce horizontal pay compression, such as paying employees the same within a given position, or paying men and women equally for the same title. Since horizontal inequality accounts for a small fraction of overall inequality, the potential effects of horizontal compression are probably minor.

This study is related to a recent but growing body of literature that looks at the effects of pay transparency and pay inequality. In a seminal study, Card, Mas, Moretti, and Saez (2012) conducted a field experiment to explore the effects of salary transparency at the University of California. A random subsample of employees was sent an email with a link to a public website that listed the salaries of all university employees. Three to ten days later, the researchers sent another email, this time to the entire sample, with a link to an online survey. The authors found evidence of horizontal comparisons having negative effects: for workers who had salaries below the peer average, receiving the link to the salary website decreased their job satisfaction and increased the likelihood of stating the intention of finding a different job. These findings proved consistent with models of social preferences among peers (Frank, 1984; Romer, 1984; Summers, 1988; Lazear, 1989; Akerlof and Yellen, 1990). More recently, other studies have documented effects of pay inequality and pay transparency using natural experiments (Mas, 2017; Perez-Truglia, 2019; Dube, Giuliano, and Leonard, 2019), field experiments (Cohn, Fehr, Herrmann, and Schneider, 2014; Cullen and Pakzad-Hurson, 2016; Breza, Kaur, and Shamdasani, 2018), and laboratory experiments (Bracha, Gneezy, and Loewenstein, 2015; Huet-Vaughn, 2017).

Our study advances the existing literature in two ways. First, whereas the previous work focused exclusively on horizontal comparisons, our study investigates both vertical and horizontal comparisons. Horizontal pay inequality accounts for a small share of the overall inequality within firms (Baker et al., 1988). In the firm we studied, for example, less than 5% of the salary inequality is horizontal. This distinction between vertical and horizontal comparisons is important, as we find that the two types of comparisons have effects in opposite directions, through different mechanisms, and thus have very different implications.

The second way we contribute to this literature is by providing direct evidence of salary misperceptions. Our close collaboration with the firm we studied allowed us to provide measurements that had proved elusive in previous work. We are able to match incentivized survey responses to administrative records, which allows us to measure salary misperceptions directly. Additionally, our unique experimental design allows us to disentangle the sources of the misperceptions by measuring employees' willingness to pay for information and the diffusion of salary information. With the exception of a few studies from Psychology, such as Lawler (1965) and Kiatpongsan and Norton (2014), the question of how employees learn about salaries remains largely unexplored in economics.

Our study also contributes to literature about career concerns and salary dynamics (Lazear and Rosen, 1981; Harris and Holmstrom, 1982; Rosen, 1986; Gibbons and Murphy, 1992; Holmstrom, 1999; Dewatripont et al., 1999; Gibbons and Waldman, 1999a,b). Although there is a rich body of theoretical work on this topic, there is little direct evidence about these mechanisms. We contribute by testing some of the central predictions of these models. We show, for example, that employees form their expectations about future salaries based on what they think their bosses are making. We also show that employees work harder when they find out that a position that they aspire to get promoted to offers high salaries.

More broadly, our study also relates to a literature on the determinants of employee morale (Dellavigna, List, Malmendier, and Rao, 2019) and on the effects of relative income on job satisfaction (Godechot and Senik, 2015; Clark, Frijters, and Shields, 2008) and happiness (Senik, 2004).

The rest of the paper proceeds as follows. Section 2 describes the survey design. Section 3 discusses the institutional context and data sources. Section 4 presents the results on salary misperceptions. Section 5 discusses the effects of perceived salaries on behavior. The last section concludes.

2 Survey Design

In this section, we discuss the most important aspects of the survey design.

2.1 Training

A sample of the full online survey is included in Appendix A.[1] The first module of the survey was designed to teach the subjects some basic concepts that would be useful for the rest of

[1]To protect the identity of the firm, we removed all identifying information from this survey instrument, including the formatting.

the survey. It begins with an explanation of how the incentivized questions work and why responding honestly would be in the respondent's best interest. To cement this knowledge, we included some practice questions on topics unrelated to salaries.

All of the accuracy rewards in the survey were set up using the traditional quadratic loss function calibrated to award up to $2.61 per question – this amount, as well as all other monetary amounts discussed in the paper, have been transformed to United States dollars using PPP-adjusted exchange rates from April 2017. We did not inform subjects whether they got any of the specific questions right or wrong, to prevent them from learning anything about the questions they were being asked. Furthermore, a participation fee whose amount was determined at random was added to each reward. A few weeks after the survey, the participant received a direct deposit in the full amount for survey participation. Due to all of these measures, it would be almost impossible for the individual to use that amount to infer anything about whether he was right or wrong on a specific question (e.g., guessing the average manager's salary).

This module also provides the definition of salary used in the rest of the survey. We focus on monthly base salary, that is, the salary before any additions or deductions, such as taxes, allowances, commissions, or bonuses. According to interviews with administrators from the human resources department and employees who were not participating in the experiment, base salary is the feature of compensation that is most salient and most important to employees. To illustrate its centrality, when a new employee joins the firm, the monthly base salary is the key figure written in the contract. Moreover, the base salary accounts for nearly all of the total compensation for the subjects in our sample.[2]

To confirm that respondents understood the definition of salary, we asked them to guess their own salaries for the month of March 2017. We incentivized the question by offering a reward for accuracy. On the next screen, we showed the participant's guess as well as the true salary. If the respondent's guess was not within 5% of the true salary, we showed them an additional screen re-explaining the definition. This question was also intended to convey that the surveyor already knows the salary of the respondent, thus undermining any inclination on the part of the respondents to misreport their salaries in order to avoid revealing them to the researchers.

[2]Because of the sensitive nature of the data, we cannot share specifics of the compensation scheme. After base salary, the second source of compensation for individuals who have some form of sales role is sales commissions, but they tend to be small relative to the base salary. Other forms of performance pay can be substantial for employees in the highest positions, but those employees were excluded from participating in our survey.

2.2 Definitions of Managers and Peers

The survey revolves around the average salaries of two groups: managers and peers. To identify a managerial position for each individual, we used multiple sources of administrative data. The criteria for who we considered a manager can be summarized as follows: 1) The managerial position had to be occupied by someone in the respondent's unit; 2) The managerial position had to be higher than the respondent's position; 3) The managerial position had to have an oversight role over the respondent, such as conducting performance evaluations or approving leaves of absence.

According to the career-concerns model, the effects of manager salary should depend on the distance between the managerial position and the respondent's own position. To test this hypothesis, we pick a different managerial position for different respondents. For example, a junior analyst in investment banking could be asked about the average salary of senior analysts (a position that is a few promotions above them) or about the salary of the chief economist (a higher number of promotions away). And to complement this data, we included two questions in the survey to elicit the respondents' own perceptions of the distance between their own positions and the managerial position: the number of promotions needed to attain the managerial position and the likelihood of being promoted to that position within five years.

We use a definition of peer group that is close to the definition used in other studies (Card et al., 2012; Cohn et al., 2014; Dube et al., 2019; Cullen and Pakzad-Hurson, 2016; Breza et al., 2018): employees with the same position title who work in the same unit. The peers of a junior analyst in investment banking, for example, would be the other junior analysts in investment banking. Because they have the same title, these employees should have the same powers and the same responsibilities. Employees typically work in close physical proximity to their peers and on some occasions they may even need to collaborate with each other. In the survey, we provide specific instructions about the definition of each group. In the case of peer group, for example, we state the full position title, the full name of the unit, and the number of employees currently working in that peer group.

2.3 Salary Perceptions Modules

The two main modules, on manager salaries and peer salaries, follow the structure below:

- **Step 1 (Elicit Prior Belief):** We asked respondents about the average monthly base salary among peers/managers. To elicit truthful responses, we offered a reward for accuracy. To get a sense of how certain respondents felt about their guesses, we also elicited the probability beliefs over a series of bins around the respondent's guess – this question was also incentivized.

- **Step 2 (Elicit Willingness to Pay)**: We offered respondents the opportunity to acquire the following piece of information: the average salary over a random sample of five managers/peers. To elicit this information in an incentive-compatible way, we employed the multiple price list variation of the Becker-DeGroot-Marschak (BDM) method (Becker et al., 1964). This method consists of having respondents make choices in five hypothetical scenarios. In each scenario, the respondent can choose to either see the piece of information or add a certain amount to their survey rewards (i.e., the "price" of the information). Since all employees must have accounts in the bank where they work, the monetary rewards could be deposited directly into a respondent's bank account. The five scenarios differ in the price of the information: $1.3, $6.5, $26.1, $130.5, and $652.3.[3] We explained to subjects that making truthful choices was in their best interest because there was a 1% probability that one of the five scenarios would be randomly selected to be executed. For the 2% of respondents who had their scenarios executed (1% for the manager salary and 1% for the peer salary), the survey was automatically terminated; thus, they are excluded from the subject pool. The other 98% of respondents continued with the rest of the survey.

- **Step 3 (Information-Provision Experiment)**: For each subject, we calculated the two signals described in the previous step: the average salary over a random sample of five managers, and the average salary over a random sample of five peers. We then cross-randomized whether the subject would get to see each of the signals. Each subject faces a 50% probability of seeing each signal. To avoid respondents making inferences from the act of receiving information, we made the randomization explicit. In a first screen, we let the respondents know that a group of individuals participating in this survey would be randomly chosen to receive the signal about manager/peer salary for free. In the following screen, we let the subjects know whether they were chosen to receive the signal or not.

- **Step 4 (Elicit Posterior Belief)**: We gave the subjects the opportunity to revise their guess about the average salary of their managers/peers. To avoid subjects making inferences based on the opportunity to re-elicit their guesses, we explicitly noted that all survey participants automatically had this opportunity, regardless of their initial guesses.

[3] We calibrated this scale using a small pilot survey that elicited willingness to pay for information with an open-ended and non-incentivized question.

This module appears twice, first for peer salary and then for manager salary. With respect to the information-provision experiment, we cross-randomized the two pieces of information, which resulted in four treatment groups: one group received a signal about the average salary of their peers but no salary information about their manager; one group received a signal about the salary of their manager but not those of their peers; one group received information about both their peers' and manager's salaries; and one group received no salary information.

Note that our survey elicited beliefs about the average salaries in those specific groups (managers and peers). In practice, employees may be interested in other moments of the distribution, such as the median, minimum, or maximum. This design choice was based on interviews with employees who were not invited to the survey and also managers from the human resources division, all of whom indicated that the information about averages was most relevant for them. If anything, to the extent that our choice of specification missed other important characteristics of the salary distribution, our baseline model would underestimate the effects of salary comparisons.

2.4 Survey Outcomes

The main outcomes of interest consist of actual behavior based on administrative data. Additionally, we included a series of questions at the end of survey (i.e., after the information-provision experiments) to serve as survey outcomes and provide evidence of the underlying mechanisms at work.

The first two survey questions investigate the career-concerns model. According to this model, if employees learn that their managers get paid more, and if they expect to reach that same position eventually, then they should become more optimistic about their own future salaries. To test this hypothesis, we elicit expectations about respondents' own future salaries one year and five years in the future, using an incentive-compatible method. Incentivizing truthful responses is a bit harder for this question. It was not practical to offer rewards by comparing the guesses to the actual future salaries, because to do that we would need to wait one and five years to pay the rewards. Instead, we told respondents that we would compare their guesses to our own predictions of their future salaries (based on our administrative data and models) and told them that we would pay them higher rewards the closer their predictions were to our own predictions.

We included three questions meant to gauge the role of social preferences. First, we follow a body of literature that uses self-reported employee satisfaction as a proxy for employee morale (Clark and Oswald, 1996; Card et al., 2012). We included the standard question on pay satisfaction: "How satisfied are you with your current salary?" Responses to this question used a 5-point scale from very dissatisfied (1) to very satisfied (5). And we also included the standard question on job satisfaction: "Taking all aspects of your job into account, how satisfied are you with your current job?" This question used the same 5-point response scale used for pay satisfaction. The third question, on tolerance for pay inequality in the firm, is an adaptation of a traditional question from the literature on preferences for redistribution (Cruces et al., 2013): "Across the thousands of [Bank Name] employees, salaries vary with the nature of the work, education, experience, responsibilities, etc. What do you think of wage differentials in the company today?" The possible answers were (1) They are too large, (2) They are adequate, and (3) They are too small. Higher values of this outcome thus indicate higher tolerance for pay inequality.

2.5 Background Questions

Towards the end of the survey, we included a few additional questions meant to provide some useful descriptive statistics about income transparency. One question elicited how often the respondent discusses salaries with coworkers, on a standard scale ranging from "once a week" to "never." We asked employees to rank how interested they would be in learning the average salaries in different positions, such as their same position or positions above theirs. Last, we asked employees about their preferences for pay transparency at the firm.

3 Institutional Context, Data, and Subject Pool

3.1 Institutional Context

We conducted the experiment in collaboration with a private commercial bank in Asia. To keep the identity of the firm secret, we refrain from being specific about the firm's characteristics. This firm has millions of customers, billions of dollars in assets and in revenues, and thousands of employees. These employees are based in two headquarters and in hundreds of geographically dispersed branches.

This firm is comparable to other large firms around the world in two key respects: pay inequality and pay transparency.

Regarding pay inequality, the ratio between the 10th and 90th percentile of salaries is 0.21 in this firm, whereas it is 0.19 for the average medium-sized U.S. firm (Song et al., 2019). The inequality in this firm is also typical in that only a small part of it is horizontal (Baker et al., 1988). A simple inequality decomposition suggests that less than 5% of the pay inequality is horizontal – the details for this calculation are reported in Appendix D.1, where we also show that the share of horizontal inequality is in the same order of magnitude for other organizations that have been studied in the literature. Moreover, we find salary differentials comparable to those in the U.S., even when we look at specific pairs of employees and their managers. For instance, the ratio between the salary of a senior relationship manager and their subordinate, a personal retail banker, was 1.5 in this firm, while according to 2017 data from Glassdoor the corresponding ratio for Bank of America was also 1.5.

Regarding transparency, the firm does not have an open salary policy.[4] Several surveys corroborate this pattern of pay secrecy around the world. For example, a 2003 survey of Fortune-1,000 firms shows that only 3.5% of the surveyed firms had open salary policies (Lawler, 2003); a survey of about 1,000 companies indicates that only 3% have open salary policies and less than a quarter disclose data on salary ranges (Scott, 2003). A survey of 7,100 managers from the United States and other countries indicates that only 6% have open salary policies (PayScale, 2018). Indeed, the standard employment contract at this firm explicitly prohibits employees from sharing salary information. Many organizations around the world have similar policies (Day, 2007). In a survey of private sector employees from the United States, for example, more than 60% report that their employer discourages or prohibits employees from discussing salaries with coworkers (Hegewisch et al., 2011).

According to our survey data, 45% of employees never discuss salaries with coworkers.[5] Similar patterns have been documented around the world. For instance, a survey of 1,022 employees from the United Kingdom found that 48% discuss salaries with their peers (Burchell and Yagil, 1997); and a 2017 survey of Americans aged 18-36 years show that 70% report to never discuss salaries with coworkers (Gee, 2017).

Finally, employees at the firm seem to desire more income transparency. According to our own survey data, 65% of respondents report that they would be better off if the bank disclosed information on average salaries by position, and only 20.5% think that they would be worse off. This is also a common finding in other firms and countries. A survey of employees from eight developed countries, for example, shows that most employees wish their employers were more transparent about pay (Glassdoor, 2016).

3.2 Survey Implementation

We started with a universe of employees numbering in the thousands. We were asked by the firm to exclude some specific groups of employees from participating in the survey. We

[4]The firm discloses some information about pay, but this information is too vague to form a decent guess about the average salaries of peers and managers. For instance, the firm discloses the existence of a 10-point pay band scale, but the minimum and maximum salaries in these boundaries are not disclosed, and they even overlap quite a bit.

[5]More precisely, 45% of employees reported never talking about salaries; 16%, once a year; 31%, a few times a year; 6%, once a month; and the remaining 2%, once a week or more often. Note that to the extent that it is frowned upon by the employer, this type of behavior is probably underreported in surveys.

excluded employees from the highest pay bands (i.e., the highest executives), employees who had joined the firm in the past 6 months, and employees from smaller divisions. Next, we had to exclude a few employees due to data limitations: a small minority of employees who belonged to small peer groups, and a minority of employees who could not be matched to managerial positions. After accounting for employees who were ineligible to participate, we were left with a sample of 3,841 employees we invited to participate in the survey.

The survey invitations were distributed by email. A sample of the invitation email (stripped of formatting and identifying information) is presented in Appendix B. The invitation email stated that the survey typically takes less than 30 minutes, and that survey participants would receive, on average, $30 as rewards for participating in the survey. The invitation email did not include any specific information about the content of the survey. The email mentioned that participation was not compulsory, but that employees were encouraged to take part. The invitation email listed the endorsements of three of the firm's high-level executives. The heads of each unit also reached out in separate emails to encourage participation.

The email invitations were sent out gradually over the course of two months. The dates of survey responses span from the first week of April 2017 to the first week of June 2017. This staggered timing was designed to measure the diffusion of the salary information. Of the 3,841 invitations sent out, 2,060 individuals completed the main module of the survey, corresponding to a 53.6% response rate.[6]

3.3 Descriptive Statistics and Randomization Balance

The subject pool comprises employees from different pay bands, with all types of roles (e.g., analysts, technicians, tellers, sellers, clerks, receptionists). Table 1 presents some descriptive statistics. Column (1) corresponds to the entire subject pool. On average, subjects are 29 years old and have been working at the firm for five years. 73% of them are female and 86% have a college or higher degree. The median number of employees in a peer group is 19, and the 25th and 75th percentiles are 10 and 32. In Appendix D.2, we provide more details about the subject pool – for instance, we show that the subject pool is representative of the universe of employees in all of these observable characteristics.

We can also check whether there is balance in observables across treatment groups. Subjects were cross-randomized to receive information about manager and peer salaries, which

[6]This sample already excluded the 2% of participants who were randomly assigned to have their choices in the information-shopping scenarios executed, for whom the survey was automatically terminated. This final sample also excluded 15 subjects (0.7% of the sample) with extreme prior beliefs about the average manager or peer salary (most likely due to typos).

resulted in four treatment groups. In columns (2) through (5) of Table 1, we break down the average characteristics by each of the four treatment groups. The last column reports p-values for the null hypothesis that each average characteristic is constant across the four treatment groups. The results show that, as is consistent with successful random assignment, the observable characteristics are balanced across treatments.

The comparison between the subjects' own salaries and the salaries of the managers and peers indicate that, as discussed in Section 3.1, the vertical inequality is much larger than the horizontal inequality. The mean absolute difference between the subjects' own salaries and the average peer salary is 11.7% of the subjects' own salaries. In comparison, the mean absolute difference between the subjects' own salaries and the average manager salary is 315% of their own salaries. The degree of vertical inequality that we study is not even the entire vertical inequality in the firm. For instance, we did not ask employees about managerial positions that were well above their pay grade (e.g., asking tellers about CEO pay). Had we included a broader set of managers, differences between the subjects' own salaries and their managers' salaries would have been even larger.

One metric that plays an important role in this study is the perceived distance between the subjects' current positions and the managerial positions we asked about. The average subject expects thinks 3.65 promotions are needed to reach the managerial position and thinks there is a 55.8% probability of being promoted to that position within the next five years. There is also significant variation in this perceived distance to the manager. For example, 12% of employees report that they need just one promotion to reach the managerial position, while 7% of respondents report needing five or more promotions to reach the managerial position. It is also important to note that there is significant scope for upward mobility in the organization. In a different study, about a different topic, we have data from this same firm spanning four years. We show that the annual promotion rate is 16.5%, and that over a period of four years, over half of employees are promoted at least once (Cullen and Perez-Truglia, 2019).

Our objective data suggest that these subjective perceptions are reasonably calibrated. For example, we can take advantage of the fact that increases in pay grade typically, although not always, indicate a promotion. Thus, one reasonable proxy for the number of promotions required to reach the managerial position is the difference in pay grades between the employee and his or her manager. On average, employees were 4.32 pay grades away from the managerial position. This distance seems consistent with the subjects' perceived need of 3.65 promotions to reach the managerial position. Our objective proxy also validates the cross-sectional variation in these perceptions: the perceived number of promotions needed to reach the managerial position is significantly correlated to the actual number of pay grades separating the employee from the managerial position (correlation coefficient of 0.403, p-value<0.001).

3.4 Behavioral Outcomes

We collaborated with the different units of the organization to create a centralized and anonymous database collecting many aspects of employee behavior.

The main behavioral outcomes of interest are effort and performance. We have two proxies for the effort of the employee. The first proxy is observed for employees who work in the headquarters (29% of the sample). Employees there must clock in and out from the office using an electronic card-swipe system, which is strictly enforced by security personnel. We use these time stamps to calculate the hours, minutes, and seconds that each employee spends at work on a daily basis.

The second measure of effort is observed for every employee in the sample. We scraped the email servers of the company in real time, collecting data on the emails sent and received by all employees.[7] Our measure of effort is defined as the total number of emails sent by the employee on a daily basis. The advantage of this measure over the alternative, of hours worked, is that it is available for the entire subject pool. While the number of emails may not be a great measure of effort in other contexts, it seems to be a good proxy in our context and possibly even better than the numbers of hours worked. For security reasons, employees can only access their work email account from their office computers, implying that they can only send emails while at the office. Employees are strongly discouraged from using their work email account for matters unrelated to work. Employees need to send emails to clients or coworkers for most of their duties, such as reaching out to new clients, or obtaining internal approvals for loans or credit cards. Last but not least, due to company policy, most of the work that employees do is supposed to leave an email trail. For example, after calling a client to offer a product, employees are required to follow up with an email containing the information shared over the phone. Consistent with the above anecdotal evidence, the number of emails is positively and significantly correlated to the alternative measure of effort, which is the number of hours spent in the office (correlation coefficient of 0.24, p-value<0.001).

As discussed in Dellavigna et al. (2019), there are different margins of effort, and while some of those margins may be elastic to experimental interventions, some other margins may be quite inelastic. For example, in the experiment conducted by Dellavigna et al. (2019), subjects were much more elastic to incentives when deciding to stay on the job after hours. We believe our measures of effort – hours in the office and emails sent – are likely to be

[7] Due to the sensitive nature of the data, we did not retrieve any information on the content of the emails.

elastic, too. Employees seem to have quite a bit of discretion in how long to work (leaving a bit earlier or staying after hours), and in how many emails to send and respond to. The hours worked and emails sent are not explicitly monitored by the company and employees are not rewarded or punished for them. As a result, these forms of effort may be more reactive to factors such as career concerns and social preferences.

We have one measure of performance, for employees who have a sales role (38% of the sample). The firm has detailed data on the sales revenue of each employee at the monthly level. We use the firm's standard formula to aggregate sales across the different products offered by the firm (e.g., credit cards, loans, mortgages). Our measure of performance is defined as the employee's rank in the monthly distribution of normalized sales revenues, ranging from 0 (lowest) to 1 (highest).

The information about manager and peer salaries may affect career outcomes – an employee may react by leaving the company or by renegotiating her salary, for instance. To explore these channels, we track four specific outcomes: 1) whether the employee leaves the firm; 2) whether the employee transfers to another unit inside the firm; 3) changes in the employee's salary; 4) changes in position title.

We began collecting data on these behavioral outcomes three months before launching the survey. As a result, in addition to post-treatment outcomes, we can compute the corresponding pre-treatment outcomes, which can be used as control variables to improve precision as well as for falsification tests.

4 Results: Beliefs about Manager and Peer Salaries

In this section, we document the accuracy of perceptions of manager and peer salaries and provide evidence about potential sources of misperceptions.

4.1 Accuracy of Prior Beliefs

We measure misperceptions by comparing the employees' salary guesses against the true figures from the administrative records of the firm. Figure 1.a shows misperceptions of average manager salary. Only a small share (12%) of respondents guess the average manager salary within ± 5% of the truth. The rest of the respondents miss the mark, often by a large margin: the mean absolute error is 28%. Moreover, there is a systematic bias in the perceptions: on average, employees underestimate the average salary of managers by 14.1%.

As a benchmark, Figure 1.b shows misperceptions of average peer salary. While still significant, the misperceptions of peer salary are smaller than the misperceptions of manager salary. The fraction of employees who can guess their peers' salaries within 5% (32% of respondents) is 2.6 times the fraction of employees who can guess the managers' salaries (12% of respondents). The mean absolute error for manager salaries (28%) is 2.4 times the mean absolute error for peer salaries (11.5%). Moreover, while there is a systematic negative bias of 14.1% in perceptions of manager salaries, there is an average overestimation of peer salary but of only 2.5% (p-value<0.01).

The size of misperceptions, or their direction, seem largely unpredictable. In Appendix D.4 we show that the direction and misperceptions are largely unrelated to a host of employee characteristics such as gender, pay grade, or occupation. The misperceptions of peer salary are unrelated to the size of the peer group: the correlation between peer group size and mean absolute error is small (-0.025) and statistically insignificant (p-value=0.257). And, as shown in Figure 1.c, the misperceptions of manager and peer salaries are largely unrelated to each other: their correlation coefficient is statistically significant (p-value=0.007) but small in magnitude (0.059).

We can also assess whether employees are aware of their misperceptions. To do that, we elicit the probability that the true salaries fall within certain bins around the respondent's guess. We find that individuals are largely aware that they do not know the manager and peer salaries perfectly. For example, respondents on average think that there is a 32.2% probability that the true manager salary falls within 2.5% (i.e., +/-2.5%) of their guesses, while the corresponding probability for peer salary is 33.8%. Despite being aware that they are far from perfectly accurate, employees are still overconfident about their accuracy: while on average they expect a 32.2% probability of guessing the manager salary within 2.5%, only 8% of the guesses are that accurate; and they are similarly overconfident about peer salaries.[8]

Our favorite interpretation of the large misperceptions reported above is that employees have little information about salaries beyond knowing their own salaries. In the case of perceptions of peer salary, the subjects' own salaries are significantly informative about the average peer salary. Indeed, we find that a significant fraction of respondents seem to be reporting their own salary: 35% report a guess for average peer salary within 5% of their own salaries. The majority of employees, however, seem to be using information beyond their own salaries. Whatever additional information they are using, however, it does not seem to be helping them be more accurate on average. If anything, the additional information makes them slightly less accurate. If individuals reported their own salaries as a guess for the

[8] For peer salaries, respondents expect on average a 33.8% probability of guessing within 2.5%, but the fraction of guesses that are that accurate is only 16.1%. The results are robust if we use wider bins. For example, the average individual thinks that there is a 75% probability that their guess for average peer salary falls within 10% of the truth (i.e., +/-10%), while the actual share of guesses falling that close to the truth is 55%.

average peer salary, the mean absolute error would be 11.4% (vs. 11.5% in reality), and the bias would be -0.4% (vs. 2.5% in reality). While reporting one's own salary is a reasonable idea when guessing the peer salary, one's own salary would be a poor guess for the average manager salary. If employees can only see their own salaries, that would explain why they fare so much worse at guessing manager salaries than at guessing peer salaries. Yet employees could still extrapolate from their own past salary dynamics to form a guess about the manager salary. However, there are a host of documented biases in that type of extrapolation. Some of these biases may explain not only the extent of the misperceptions, but also the systematic negative bias. For example, employees may be projecting their own salaries forward using their past salary growth linearly instead of exponentially, which could generate a systematic underestimation (Stango and Zinman, 2009).

As a robustness check, we can check whether subjects understood the definition of base salary that we provided to them. The results from the training module, discussed in detail in Appendix D.3, suggest this is the case. The vast majority of employees were able to guess their own salaries almost exactly on their first try. This finding confirms the anecdotal evidence that base salary is the most salient aspect of compensation in this firm. Moreover, we took additional steps to make sure that before we elicited manager and peer perceptions, all respondents understood and agreed with the definition of base salary given to them.

As an additional robustness check, we can take advantage of the fact that we measured misperceptions in a follow-up study (Cullen and Perez-Truglia, 2018), with a similar sample but with some methodological differences. We provided different incentives (rewarding responses within 5% of the truth instead of using the quadratic scoring rule), we used larger reward amounts (earning up to $63, instead of up to $2.61), and we elicited a different belief (the average salary among a specific sample of five peers, instead of the average among all peers). Despite these methodological differences, the results from the two experiments are quite consistent in magnitude: the mean absolute error for peer salaries is 11.5% in this survey versus 14.6% in Cullen and Perez-Truglia (2018).

Probably because of the sensitive nature of the exercise, we are unaware of other studies that can assess the accuracy of salary perceptions inside a corporation. A notable exception is Lawler (1965), who collected survey responses from a total of 326 managers from four privately owned U.S. companies. His findings are qualitatively consistent with ours. He finds, for example, that employees tend to systematically underestimate the salaries of those in higher positions but do not systematically underestimate the salaries of those in their same position. Our finding that employees underestimate manager salaries is also consistent with evidence from Kiatpongsan and Norton (2014): according to survey data from 16 countries, households underestimate the average pay ratio between chief executive officers and unskilled workers.

4.2 Willingness to Pay for Salary Information

The misperceptions of the salaries of peers and managers may simply reflect a lack of interest on the information. The data on willingness to pay for information can shed light on this question.

For each piece of information (manager and peer salary), respondents had to choose, in five different scenarios, whether to buy the information at a given price. Following the literature, we can compare their responses across different scenarios to assess whether they are consistent (Andersen et al., 2006). For example, if the respondent is willing to pay $130.46 for the information in the fourth scenario, he or she should be also willing to pay $26.09 for the manager information in the third scenario. We find that the vast majority of subjects (80% for manager information and 85% for peer information) made selections that are consistent across scenarios. These rates are in the same order of magnitude as the corresponding rates reported in other studies employing the price-list method (Andersen et al., 2006; Allcott and Kessler, 2019; Fuster et al., 2018). Following the standard practices, the following results focus on subjects with consistent responses. But the results are nevertheless similar under alternative approaches.

Figure 1.d shows the distribution of the willingness to pay for the signal about manager salary. This histogram is based on the raw responses in the five scenarios. For example, the $28.1–$130.5 bar shows that 23.8% of subjects were willing to buy the information for the price of $28.1 but were not willing to buy the information for the price of $130.5. Their maximum willingness to pay must therefore be in between $28.1 and $130.5. The distribution of willingness to pay shown in Figure 1.d indicates that while some employees see little value in information about manager salaries, a substantial fraction of them value it a lot. On the one extreme, 22.9% of employees are not willing to pay more than $6.5 for the signal about manager salaries, an amount that is typically less than an hour's worth of salary.[9] On the other extreme, 24.9% of employees are willing to pay more than $652 for the information, which for most employees constitutes more than a week of their salaries.[10] This substantial willingness to pay for salary information suggests that a great deal of the misperception arises because acquiring information is difficult rather than because individuals are uninterested.

[9]Note that 19.3% of the respondents were not willing to buy the information even for the lowest price of $1.3. In principle, some of these subjects could have a negative willingness to pay: that is, they would like to pay to avoid seeing the information. We did not extend the price list to include negative prices because this information aversion seemed largely inconsistent with what we saw in qualitative interviews with employees.

[10]We refrain from providing more precise information to avoid revealing sensitive information about compensation levels at the firm.

Figure 1.e shows the corresponding results for the willingness to pay for peer information instead of manager information. On aggregate, the willingness to pay for manager and peer information seems to be quite similar. However, there are substantial discrepancies at the individual level. Figure 1.f shows the joint distribution of the willingness to pay for these two types of salary information. Note that while some individuals value the manager and peer information similarly, there are plenty of individuals who are interested in the manager information but not the peer information, and vice versa. Indeed, the correlation coefficient between the willingness to pay for manager and peer information is only 0.28 (p-value<0.001).

The average willingness to pay for the information is quite substantial: one conservative estimate puts the average valuation at $247 for the manager information and $254 for the peer information.[11] To illustrate how large these valuations are, it is useful to compare our results to those from other studies that elicit willingness to pay for other types of information. Relative to the mean valuations found in our study ($247 and $254), these other studies find average valuations that are orders of magnitude smaller: $0.40 for travel information (Khattak et al., 2003), $0.80 for food certification information (Angulo et al., 2005), $3 for home energy reports (Allcott and Kessler, 2019), and $4.75 for housing price information (Fuster et al., 2018).[12]

As suggested by Stigler (1962), back-of-the-envelope calculations indicate that employees can value salary information a lot. For example, assume that an employee is considering acquiring information about peer salary for use in her salary negotiations. If the expectation is that, with 50% probability, the information will help negotiate a one-year 10% raise, then the employee should be willing to pay up to two weeks of her salary for the information. And since employees may plan to use the information for multiple decisions (e.g., whether to switch jobs or positions), the value of the information can add up quite rapidly across the different margins. Indeed, in Section 5.4 we present direct evidence that employees use their perceptions of manager and peer salaries to anticipate, and perhaps even change, their own salaries in the future.[13]

While employees seem significantly interested in the salaries of their managers and peers, they may be interested in other types of salaries, too. Based on conversations prior to conducting the experiment, with employees who were not invited to the survey and with

[11] Following Andersen et al. (2006), we assume that the average of the willingness to pay inside each bin is equal to the midpoint of the bin. And for the highest bin, which has no upper bound, we make the conservative assumption that the average is equal to the lower bound.

[12] All these amounts were converted to 2017 USD PPP to be comparable to our estimates.

[13] There are other approaches to estimating the value of information. For example, Conlon et al. (2018) use a structural model to estimate the value of salary information for unemployed individuals. Their findings also suggest that salary information could be worth a substantial amount. They estimate that the average U.S. college graduate looking for a job should be willing to pay $817 to acquire full information about the distribution of wage offers.

managers from the human resources division, these seemed to be the two most significant comparisons employees had in mind. The results from one of the survey questions confirm the anecdotal evidence: when asked about the piece of salary information they would be most interested in learning about, roughly 50% of subjects ranked their own position first, 45% of subjects ranked higher positions first, and less than 5% of respondents ranked other positions first.

The BDM elicitation is generally preferred to the non-incentivized alternative, but it is of course not perfect – indeed, some imperfections have been documented in the literature (Shogren et al., 2001). In Appendix D.5 we present a number of robustness checks. For example, we use data from a follow-up study (Cullen and Perez-Truglia, 2018) to show that willingness to pay for salary information is similar when using a different incentive-compatible method.

4.3 Learning

If employees think they have inaccurate beliefs and they are willing to pay to acquire new information, they should incorporate that information into their belief formation once they have access to it. In this section, we measure this belief formation using a simple Bayesian learning model. We will present this model in detail, because it plays a key role in the Instrumental Variables model of Section 5 below.

We follow the same econometric model that has been shown to fit the data well in information-provision experiments on a wide range of topics, such as inflation (Armantier et al., 2016; Cavallo et al., 2017), cost of living (Bottan and Perez-Truglia, 2017), and housing prices (Fuster et al., 2018). Let subscript i index employees. Let M_i^{prior} denote the prior belief about the average salary of managers – that is, the belief right before the individual reaches the information-provision experiment. Let M_i^{signal} be the value of the signal on average manager salary that we calculated for individual i (i.e., the average salary from a random sample of five managers), and let T_i^M be a binary variable that takes the value 1 if we showed that signal to individual i and 0 if not. Denote M_i^{post} as the corresponding posterior belief – that is, the perceived manager salary after the individual sees, or does not see, the information.

When priors and signals are distributed normally, Bayesian learning implies that, after the individual sees the signal, the mean of the posterior belief should be a weighted average between the signal and the mean of the prior belief: $M_i^{post} = \alpha \cdot M_i^{signal} + (1-\alpha) \cdot M_i^{prior}$, where the parameter α depends on the relative precision between the prior belief and the signal (Hoff, 2009). The parameter α, the learning rate, ranges from 0 (individuals ignore the signal) to 1 (individuals fully adjust to the signal). We can rearrange this identity as follows:

$$M_i^{post} - M_i^{prior} = \alpha \cdot \left(M_i^{signal} - M_i^{prior}\right) \tag{1}$$

In other words, the Bayesian model predicts that the belief updates ($M_i^{post} - M_i^{prior}$) should be a linear function of the gap between the signal and the prior belief ($M_i^{signal} - M_i^{prior}$). That is, respondents who overestimated salaries would revise their beliefs downward when shown the signal, while those who underestimated salaries would revise their beliefs upward when shown the signal. The model also predicts, moreover, that the slope of that relationship should be equal to the learning rate (α).

In practice, there may be "spurious" reasons for individuals to have revised their beliefs in the direction of the feedback, even if the feedback had not been shown to them. Respondents may, for example, take some additional time to think when asked a question a second time and may get closer to the truth as a result. Employees may also have made a typo on their first try, which they can correct when given the chance. To weed out these spurious reactions, we exploit the information provision experiment through the following regression:

$$M_i^{post} - M_i^{prior} = \alpha \cdot \left(M_i^{signal} - M_i^{prior}\right) \cdot T_i^M + \beta \cdot \left(M_i^{signal} - M_i^{prior}\right) + \epsilon_i \tag{2}$$

The parameter β picks up the spurious reversion towards the signal, while α picks up the true learning: that is, the degree of revisions caused by the information provision, above and beyond the spurious revisions.

Note that we do not expect subjects to fully update to the signal we provided ($\alpha = 1$) because it is based on a sample of five salaries and is thus subject to sampling variation. Nevertheless, we should expect α to be substantially above zero because the precision of the signal we provided is significantly larger than the precision of the respondents' prior beliefs. For example, for manager salary, the mean absolute error of our signal is 6.8% while the corresponding error for prior beliefs is 28%.

The learning results for manager salary are presented in Figure 2.a. This is a binned scatterplot between the belief revisions (y-axis) and prior gaps (x-axis). Intuitively, the x-axis shows the maximum revision we would expect if the respondent were to fully react to the information, and the y-axis shows the revision observed in practice. The red diamonds from Figure 2.a correspond to individuals in the treatment group (i.e., those who were shown the information about manager salary). As expected, there is a strong relationship between the belief revisions and prior gaps for individuals in the treatment group: an additional percentage point in perception gap is associated with a 0.78 higher revision. In contrast, the blue circles show a much weaker relationship among individuals in the control group: an additional percentage point in the prior gap is associated with a revision of 0.10 percentage points (p-value <0.001). This finding suggests a statistically significant but economically small degree of spurious revision. Indeed, this result is consistent in terms of magnitude with other information-provision studies (Armantier et al., 2016; Cavallo et al., 2017; Bottan and Perez-Truglia, 2017; Fuster et al., 2018).

Figure 2.b estimates equation (2), which yields the learning rate. The y-axis is still the revisions from prior to posterior beliefs, but the x-axis is the interaction between the perception gap and the treatment dummy. This interaction term plays the role of excluded instrument in the Instrumental Variables model presented in Section 5.1 below. Note that the specification weeds out spurious reversion to the signal by controlling for the difference between the prior belief and the signal without the treatment interaction. Figure 2.b shows that the linear relationship predicted by the Bayesian model fits the data tightly. The slope of this relationship (0.69, SE 0.03) indicates that, when forming posterior beliefs about manager salary, employees put a weight of 69% on the signal provided by the experimenter and put the remaining 31% on their prior beliefs.

Let P_i^{prior}, P_i^{signal}, P_i^{post} and T_i^P be respectively the prior, signal, posterior, and treatment dummy for the average peer salary. We can apply the same logic used for the manager salary to the peer salary. The results are presented in Figures 2.c and 2.d, which are the peer salary equivalents of Figures 2.a and 2.b. The results suggest that individuals learned significantly from the peer salary information as well. The slope from Figure 2.c of 0.51 (SE 0.06) indicates that, when forming posterior beliefs about peer salary, employees put a weight of 51% on the signals of peer salary provided by the experimenter and the remaining weight of 49% on their prior beliefs about peer salary.[14]

Appendix D.6 presents some additional results. First, we show that the learning rates are similar across different subsets of the population, such as across gender and pay grades. Second, we show that learning from the feedback was compartmentalized: for instance, individuals did not use the information about peer salary to form beliefs about manager salary.

4.4 Information Diffusion

In this section, we measure whether the information given to one employee was diffused to other employees in the network. This is relevant for two reasons. First, if there were information diffusion, that could create attenuation biases in our estimates, to the extent

[14]While the learning rate is substantially above zero for both manager and peer salary, it is somewhat smaller for peer salary: 0.51 vs. 0.68, with a difference that is statistically significant (p-value<0.001). According to the Bayesian model, this would indicate that individuals thought that the manager signal was more precise, or felt more confident about their prior beliefs about peer salary.

that some individuals in the control group may also be exposed to the information. Thus, knowing whether there was information diffusion or not is useful for the interpretation of the experimental results. Second, measuring information diffusion can give us insights about the sources of the misperceptions documented above. Even if the firm did not disclose any information about salaries, employees could form accurate beliefs by sharing salary information. For instance, if all individuals in a peer group shared their own salaries with each other, everyone in the group could form an exact guess for the average peer salary. Thus, we can test whether the large misperceptions we observe arise either despite information diffusion or because of a lack of diffusion.

We use a simple model of information diffusion. In this analysis, the dependent variable is the level of misperceptions. Let M_i^{true} denote the true average salary among managers and M_i^{abs} denote the absolute error of posterior beliefs: $M_i^{abs} = \left| \frac{M_i^{post} - M_i^{true}}{M_i^{true}} \right|$. The regression of interest is as follows:

$$M_i^{abs} = \kappa_0 + \kappa_1 \cdot T_i^M + \kappa_2 \cdot I_i^M + X_i \theta + \epsilon_i \tag{3}$$

T_i^M is a binary variable indicating whether the individual received information about manager salary and thus is meant to capture the "direct" information provision. The variable I_i^M is intended to measure the "indirect" information provision, through other employees in the network. I_i^M takes the value 1 if and only if i did not receive the information directly and but peer connected to i received the information before the date when i responded to the survey (so that the information could have been shared with i before she started the survey).[15] For instance, in the baseline specification, I_i^M is a binary variable that takes the value 1 if i's closest peer received the information.[16] All regressions include the same basic set of control variables (X_i): a linear time trend, the number of peers and the number and proportion of peers invited to the survey. Appendix D.7 provides descriptive statistics for all of the main variables used for this analysis.

Since the information that we provided to individuals was accurate, it should have lowered misperceptions. We thus expect $\kappa_1 < 0$. Under the null hypothesis of no information diffusion, we expect $\kappa_2 = 0$. On the contrary, finding $\kappa_2 < 0$ would constitute evidence of information diffusion.

The regression results are presented in Table 2. Columns (1)–(4) correspond to misper-

[15] In the baseline specification, we always define I_i^M to take the value 0 if i received the information directly. The rationale behind this specification is that if the individual received the information directly, whatever information received indirectly through peers is largely redundant. In Appendix D.7 we show the results are identical under the alternative specification.

[16] Note that the exogenous variation in this regressor arises from the random assignment to information as well as from the random order in which employees were invited to fill out the survey.

ceptions of manager salary.[17] In column (1), *Direct* is the binary variable indicating whether the respondent received information directly. As expected, and consistent with the findings from the previous section, the direct information provision has a strong negative effect on misperceptions, of 20.5 percentage points (p-value<0.001). For reference, the average of the dependent variable in the control group is 32.9 percentage points, so this effects amounts to a 62% reduction in misperceptions.

Column (2) includes an additional variable related to indirect information provision. We look at the peer who is most closely connected to the subject and thus may be most likely to share the salary information. *Closest Peer* is a binary variable that takes the value 1 if the individual's closest peer received the manager information before the individual responded to the survey. We define the closest peer as the peer who has the highest total of emails exchanged (sent and received) over the three months preceding the start of the experiment. Even though this measure is based on email data, it is plausible that it is also correlated to face-to-face interactions. For instance, data on card swipes confirms that these individuals go to lunch together more often than with other peers.[18]

If employees sometimes share salary information with peers, we would expect the coefficient on *Closest Peer* to be negative. Indeed, if closest peers always share the salary information with each other, the coefficient on *Closest Peer* could be as large as the coefficient on *Direct*. The results indicate, however, an absence of information diffusion. The coefficient on *Closest Peer* is close to zero (0.004), statistically insignificant, and precisely estimated. Moreover, the coefficient on *Closest Peer* (0.004) is substantially smaller than the coefficient on *Direct* (-0.205), with the difference being highly statistically significant (p-value<0.001). In other words, when we provide information about manager salary to one employee, that information affects her own subsequent perceptions but does not affect the perceptions of her closest peer.

Columns (3)–(5) of Table 2 show the results where, instead of *Closest Peer*, we use alternative variables to capture indirect information provision. *No. Peers* measures the number of peers who received information (before the respondent completed the survey, as always). *(No. Peers >0)* is a binary variable that takes the value 1 if at least one peer received the information. And *Share of Peers* measures the share of peers who received information.

[17]One disadvantage of using the manager salary is that different employees from the same peer group may receive information about different managerial positions, which may make the information diffusion more difficult. This is probably not a source of concern, however. As discussed below, the results are the same for peer salaries, for which this additional friction is absent.

[18]For employees working in the headquarters offices, we can use the swipe data to proxy whether a given pair of employees have lunch together: that is, whether the pair of employees swiped in and out of the building during lunch hours and within 30 seconds of each other. We find that, relative to her other peers, an employee is 53% more likely to grab lunch with her closest peer (18.4% vs. 12.0% for the other peers).

The results are the same: the point estimates are close to zero and are statistically insignificant and precisely estimated, indicating a lack of information diffusion. Columns (6)–(9) reproduce the same analysis as columns (1)–(5) but look at peer salaries instead of manager salaries. Again, we find robust evidence of an absence of information diffusion.

In Appendix D.7 we provide some additional results. We show that the experimental results are robust under alternative specifications. Moreover, we complement these experimental findings with non-experimental tests of information diffusion. We exploit two predictions borne by models of information diffusion (Alatas et al., 2016; Banerjee et al., 2013; Mobius and Rosenblat, 2014): employees with higher network centrality and employees who talk more should have lower misperceptions. We find that, as is consistent with a lack of information diffusion, those two relationships are precisely estimated around zero.

There are multiple potential explanations for the lack of information diffusion. The firm's pay secrecy rule may discourage employees from discussing salaries with coworkers. Alternatively, some employees may not want to share salary information for strategic reasons. For instance, they may see information as a rivalrous asset. Last, employees may not want to discuss salaries in response to privacy norms (i.e., the "salary taboo") – indeed, a follow-up study (Cullen and Perez-Truglia, 2018) provides evidence in support of this channel.

5 Results: The Effects of Salary Beliefs on Behavior

In the previous section, we presented evidence about how employees perceive the salaries of others. In this section, we study the effects of those salary perceptions on the employees' own behavior.

5.1 Econometric Model

Let Y_i^{post} be a measure of employee i's average effort (e.g., hours worked) in the period starting from the survey date and ending 90 days later.[19] Following the notation from Section 4.3, let M_i^{post} be the posterior belief about the average manager salary that the individual holds at the end of the survey, and let P_i^{post} be the posterior belief about the average peer salary. The following equation establishes the relationship of interest:

[19]In the results section, we discuss alternative horizons. For the small fraction of employees who leave the company during the relevant time window, we use the average outcome between the survey date and the exit date. Also, for the sales outcome, which is based on monthly data, the post-treatment period corresponds to the month when the survey was taken and the following two months. This specification can lead to an attenuation bias because individuals who respond to the survey on the first day of the month (who were exposed to the information for a full month) would be coded the same as individuals responding on the last day of the month (who were exposed for one day).

$$\log\left(Y_i^{post}\right) = \eta_0 + \eta_{mgr} \cdot \log\left(M_i^{post}\right) + \eta_{peer} \cdot \log\left(P_i^{post}\right) + \nu_i \quad (4)$$

The parameter η_{mgr} captures the motivating (or demotivating) effects of manager salaries. On the one hand, according to models of career concerns, employees may have an extra incentive to work if they think their managers are paid more (Lazear and Rosen, 1981; Harris and Holmstrom, 1982; Rosen, 1986; Gibbons and Murphy, 1992; Holmstrom, 1999; Dewatripont et al., 1999; Gibbons and Waldman, 1999a,b). If the employee expects to be promoted to the managerial position, this higher managerial salary should incentivize the individual to work harder to get promoted. As a result, this mechanism predicts $\eta_{mgr} > 0$ (for a formalization of this argument, see the model in Appendix C).

On the other hand, according to models of social preferences, employees may be demoralized for being paid less than their managers. For example, employees may feel jealous of their managers. The employee morale may be a function of the employees' compensation relative to their managers. Holding the employees' own salaries constant, a higher perceived manager salary worsens the employees' relative status and thus may demoralize them (Godechot and Senik, 2015). As a result, this mechanism predicts $\eta_{mgr} < 0$ (this mechanism is also formalized in the model in Appendix C).

In turn, the parameter η_{peer} captures the motivating (or demotivating) effects of peer salaries. The same mechanisms discussed for the vertical comparisons may operate for the horizontal comparisons. Regarding career concerns, employees who discover that peers are getting paid more may expect to use that information in salary negotiations and thus take it as a positive signal of their own future, as in the "tunnel effect" (Hirschman and Rothschild, 1973). As discussed in more detail in Appendix C, this mechanism predicts $\eta_{peer} > 0$. And regarding social preferences, there are a number of models in which employee morale increases with relative pay (Solow, 1979; Bewley, 1985; Frank, 1984; Romer, 1984; Summers, 1988; Lazear, 1989; Akerlof and Yellen, 1990), resulting in $\eta_{peer} < 0$. For example, employees may follow a reciprocity norm (Akerlof, 1982; Gneezy and List, 2006) and thus feel obligated to work harder if they are paid more than their peers and less obligated to work hard if they are paid relatively worse.

Obtaining causal estimates of η_{mgr} and η_{peer} is challenging. A simple regression of behavior on perceived salaries would be subject to the usual concerns about omitted variable biases. For instance, employees who are more optimistic about manager salary may be the same ones who have higher intrinsic motivation or higher ability, resulting in a spurious $\eta_{mgr} > 0$. To estimate these parameters, our empirical framework exploits the random variation in beliefs induced by the information provision experiments.

To understand the intuition behind this model, consider a pair of employees who have the same bias about perceived peer salary: both of them underestimate the actual manager salary by 20%. We then randomly assign information about the true manager salary to one of these two employees. We would expect that, relative to the individual who does not get the information, the individual who receives the information ends up with a perceived manager salary that is higher. For the sake of argument, let's assume that the individual who did not receive the information continues to underestimate the actual manager salary by 20%, but that the individual who did receive the information reacts to the information and ends up underestimating the manager salary by just 10%. The information provision is thus equivalent to a +10% shock to the perceived manager salary. This allows us to check what happened to the behavior of this pair of employees in the months after they received the information. If the 10% shock to the perceived manager salary translates into higher effort, it would imply that perceived manager salary motivates employees. On the contrary, a negative effect on effort would imply that manager salary demotivates employees. Moreover, we can estimate η_{mgr} from this data. Again, for the sake of the argument, assume that the 10% shock to perceived manager salary causes a 2% increase in effort. We can calculate the implied η_{mgr} by taking the ratio between these two values: $\eta_{mgr} = 0.2 = \frac{2\%}{10\%}$.

The above analysis is based on a group of employees who underestimated their managers' salaries by 20%. In practice, only a small share of the sample will underestimate by around 20%, so there will not be enough statistical power to limit the analysis to this group alone. However, there is nothing special about the 20% underestimation in the calculations described above. We could repeat the analysis for individuals who underestimate by 50%, for individuals who overestimate by 20%, and so on and so forth. While we do not have enough power to estimate precisely within each of those groups, once we aggregate all of the groups we should have enough precision. That is what the following Instrumental Variables (IV) model is designed for:

$$\begin{aligned}\log\left(Y_i^{post}\right) &= \pi_0 + \eta_{mgr} \cdot M_i^{post} + \eta_{peer} \cdot P_i^{post} \\ &\quad + \pi_1 \cdot \left(M_i^{signal} - M_i^{prior}\right) + \pi_2 \cdot \left(P_i^{signal} - P_i^{prior}\right) + \pi_3 \cdot M_i^{prior} + \pi_4 \cdot P_i^{prior} + X_i \pi_5 + \epsilon_i\end{aligned} \tag{5}$$

$$\begin{aligned}M_i^{post} &= \mu_0 + \mu_1 \cdot \left(M_i^{signal} - M_i^{prior}\right) \cdot T_i^M + \mu_2 \cdot \left(P_i^{signal} - P_i^{prior}\right) \cdot T_i^P \\ &\quad + \mu_3 \cdot \left(M_i^{signal} - M_i^{prior}\right) + \mu_4 \cdot \left(P_i^{signal} - P_i^{prior}\right) + \mu_5 \cdot M_i^{prior} + \mu_6 \cdot P_i^{prior} + X_i \mu_7 + \xi_i^1\end{aligned} \tag{6}$$

$$\begin{aligned}P_i^{post} &= \nu_0 + \nu_1 \cdot \left(M_i^{signal} - M_i^{prior}\right) \cdot T_i^M + \nu_2 \cdot \left(P_i^{signal} - P_i^{prior}\right) \cdot T_i^P \\ &\quad + \nu_3 \cdot \left(M_i^{signal} - M_i^{prior}\right) + \nu_4 \cdot \left(P_i^{signal} - P_i^{prior}\right) + \nu_5 \cdot M_i^{prior} + \nu_6 \cdot P_i^{prior} + X_i \nu_7 + \xi_i^2\end{aligned} \tag{7}$$

Equations (6) and (7) correspond to the first stage of the IV regression. These regressions measure the effect of the information provision experiments on the posterior beliefs, and are based on the Bayesian learning model given by equation (2) from Section 4.3. The vector of additional control variables (X_i) is included to reduce the variance of the error term and thus improve the precision of the estimates. It contains the following variables: the employee's own salary (in logs), tenure (in logs), dummies for performance evaluations in the previous year, and, following the standard practice in field experiments (McKenzie, 2012), the pre-treatment outcomes.

The key instrument exogeneity assumptions are $E\left[\left(M_i^{signal} - M_i^{prior}\right) \cdot T_i^M \cdot \epsilon_i\right] = 0$ and $E\left[\left(P_i^{signal} - P_i^{prior}\right) \cdot T_i^P \cdot \epsilon_i\right] = 0$. The identification is based on the random assignment of $\{T_i^M, T_i^P\}$. The implicit assumption is that there are no unobservable characteristics that can both explain heterogeneity in the effects of the information and are correlated with the prior gaps in beliefs. Given the evidence discussed in the previous section, that prior gaps in beliefs are largely unpredictable, we believe this is a reasonable assumption.

This model also makes some implicit functional form assumptions: the relationship between outcomes and beliefs is log-log linear and symmetrical. This is the simplest possible specification and thus provides a good starting point. This is also a common specification in the literature on relative income concerns (Senik, 2004; Luttmer, 2005; Clark et al., 2008; Clark and Senik, 2010). We start with this simplest specification and in Section 5 below we discuss tests of these assumptions such as through binned scatterplots.

It is plausible that different employees may react differently to the salaries of managers and peers, amounting to heterogeneity in η_{mgr} and η_{peer}. In the spirit of Imbens and Angrist (1994), our estimates would identify the local average treatment effects of the perceptions – that is, weighted averages of η_{mgr} and η_{peer} with a higher weight given to employees whose beliefs are more affected by the information-provision experiment. By construction, this weight will be higher for individuals who have larger prior misperceptions and, conditional on the misperceptions, for individuals who react more to feedback. In any case, given the evidence discussed above that misperceptions and learning rates are largely unrelated to any observable characteristics, it is likely that the weights are quite homogeneous.

Last, we can exploit the timing of the intervention to provide a falsification test in an event-study fashion. Let Y_i^{prior} denote the average behavior in the period prior to the information-provision experiment (i.e., in the days before the date of survey completion rather than in the days after the survey completion). We can estimate the same IV model from above, but using Y_i^{prior} instead of Y_i^{post} as the dependent variable. Intuitively, the information-provision experiment should not affect behavior in the pre-treatment period because the individuals have not yet been exposed to the information. We thus expect the coefficients for η_{mgr} and η_{peer} to be close to zero and statistically insignificant in this falsification regression.

5.2 Effects of Beliefs on Behavior

Table 3 presents the results from the IV model outlined above. For obvious space constraints, this table presents the IV coefficients directly (Appendix D.8 provides the results for the reduced form and first stage regressions). Each column of Table 3 uses a different form of behavior as the dependent variable. The main outcomes of interest, effort and performance, are presented in columns (1) through (3). Column (1) corresponds to the daily average number of hours worked from the date of survey completion until 90 days later. This measure is only available for 29% of the sample (employees based in the headquarters). The coefficient on *Log(Manager-Salary)* is positive (0.150) and significant, both statistically (p-value = 0.042) and economically. This coefficient indicates that believing that their managers' salaries are higher, on average, motivates employees. Since the right-hand-side and left-hand-side variables are defined in logs, this coefficient implies a behavioral elasticity of 0.150: i.e., increasing the perceived manager salary by 10% would increase the number of hours worked by 1.5%.

The effects on the other measures of effort and performance are similar to the effects on hours worked. Column (2) of Table 3 uses our alternative measure of effort: the average number of emails sent, which is available for the entire subject pool.[20] The coefficient on *Log(Manager-Salary)* is positive (0.130) and highly significant (p=0.001). The findings are not only qualitatively consistent across the two measures of effort, but also quantitatively similar: we cannot reject the null hypothesis that the manager elasticity for hours worked (0.150) is equal to the manager elasticity for emails sent (0.130) – p-value=0.816. Column (3) of Table 3 uses the measure of performance as the dependent variable. This outcome is available only for 38.4% of employees who have a sales role. Consistent with the positive effects on effort, we observe a positive effect on performance: the coefficient on *Log(Manager-Salary)* from column (3) is positive (0.106) and on the same order of magnitude as the effort elasticities. Still, we should take this finding with a grain of salt because this coefficient is less precisely estimated and is thus statistically insignificant (p-value=0.383).

As a benchmark for the manager elasticities, we turn to the peer elasticities. In column (1), for the number of hours worked, we find a coefficient on *Log(Peer-Salary)* that is negative (-0.943) and statistically significant (p-value = 0.045). This coefficient is also economically

[20] This measure of effort focuses on the total number of emails sent. In Appendix D.11, we break down the results by emails sent and received, by emails sent inside and outside of the firm, and by emails sent to employees with higher, same or lower pay grade.

substantial, equivalent to a behavioral elasticity of -0.943: in other words, increasing the perceived peer salary by 10% would decrease the hours worked by 9.4%. The results in columns (2) and (3) suggest that the coefficient on peer salary for hours worked (-0.943) is qualitatively and quantitatively similar to the corresponding coefficients for the number of emails (-0.431, p-value = 0.041) and sales performance (-0.731, p-value = 0.014).

While the coefficients on *Log(Peer-Salary)* are larger in absolute value than the coefficients on *Log(Manager-Salary)*, one should not conclude that horizontal comparisons are more important for behavior because there is much more variation in *Log(Manager-Salary)* than in *Log(Peer-Salary)*. Note also that the manager coefficients are substantially more precisely estimated than the corresponding peer coefficients – in column (1), for instance, the coefficient on manager salary has a standard error of 0.074 while the corresponding coefficient on peer salary has a standard error of 0.472. This difference in precision arises from the fact that our information shocks induced more variation in manager perceptions than in peer perceptions: the prior beliefs about manager salary were less accurate than the prior beliefs of peer salary, and the learning rate was higher for manager salary than for peer salary.

One of the most important and robust findings is that the manager and peer elasticities have opposite signs: while manager salary motivates employees on average, peer salary has demotivating effects. To provide a more rigorous comparison, the bottom of each column of Table 3 reports the p-value of the test of the null hypothesis that the peer elasticity is equal to the manager elasticity. We always reject this null hypothesis, with p-value=0.026 for hours worked (column (1)), p-value=0.007 for emails sent (column (2)), and p-value<0.001 for sales performance (column (1)).

One unique aspect of our setting is that subjects are in a continuing contract with the firm, which allows us to follow what happens to this relationship going forward, such as through exits or salary negotiations. The effects on these career outcomes are reported in columns (4)–(7) of Table 3. Columns (4) and (5) explore two forms of retention. Column (4) uses a binary dependent variable indicating whether the employee leaves the firm. The results suggest that a 10% increase in perceived peer salary increases the probability of leaving the company by 2.35 percentage points (p-value = 0.029). This effect is at least directionally consistent with the effects on effort and performance: a higher perceived peer salary demoralizes employees to the extent that they are more likely to leave the firm. With regard to vertical comparisons, a 10% increase in perceived manager salary decreases the probability of leaving the company by 0.15 percentage points, but the effect is economically and statistically insignificant.

In column (5), we use a binary dependent variable indicating whether the individual is transferred to another unit within the firm. Even though the signs of the coefficients are consistent with those in column (4), the coefficients are closer to zero and statistically insignificant. In column (6), the dependent variable is the logarithm of the base salary three months after the completion of the survey. The coefficients on both manager and peer salaries are close to zero, statistically insignificant, and precisely estimated. Similarly, column (7) uses a binary dependent variable indicating a change in position title. Again, the manager and peer coefficients are close to zero and statistically insignificant. These results suggest that the manager and peer information did not affect most career outcomes three months later. We must note, however, that few employees experience these career changes in such a short time horizon. However, these perceptions could possibly affect career outcomes over longer horizons, such as years into the future – we return to this discussion in Section 5.4, where we present the results on short-term versus long-term salary expectations.

5.3 Robustness Checks

In this section, we provide a number of robustness checks for the results reported above.

One potential concern with IV models is that of weak instruments (Stock et al., 2002). Given the strong reaction to the information documented in Section 4.3, this should not be a source of concern. For a rigorous assessment, Table 3 reports the Cragg-Donald F statistic, which is commonly used to diagnose weak instruments. The value of this statistic in each regression is well above the rule of thumb of $F > 10$ that was proposed by Stock et al. (2002); it takes the values of 29.8, 204.0, 98.2, 203.7, 203.4, 203.6 and 203.3 respectively in columns (1) through (7) of Table 3.

In Section 3.1 we showed that, consistent with successful random assignment, the observable characteristics are balanced across the four treatment groups. The third and fourth row of coefficients of Table 3, labeled "Pre-Treatment (Falsification)", present a more direct check. Those coefficients are estimated in a regression with the pre-treatment behavior (i.e., the average during the months before the survey) instead of post-treatment behavior (i.e., during the months after the survey) as the dependent variables. We expect these falsification coefficients to be close to zero and statistically insignificant, because the information that was randomly provided on the date of the survey could not have possibly affected the behavior prior to the survey date. As expected, all of the falsification coefficients are close to zero and statistically insignificant. For example, the post-treatment coefficient in column (1) of Table 3 is positive (0.150, p-value = 0.042), while the corresponding pre-treatment coefficient is close to zero (0.001) and statistically insignificant. Some of these falsification coefficients are not very precisely estimated, so they must be taken with a grain of salt on their own. However, when all the coefficients are together, they are quite re-assuring about the validity of the experiment.

We conducted a number of additional robustness checks that, due to space constraints, we can only mention briefly but are reported in detail in Appendix D. For example, the baseline specification estimates the effect on behavior in the 90 days after the survey date. Appendix D.9 shows that the effects are still present even when looking at the longest horizon we have data for: 180 days after the survey date. However, because of the precision of the estimates, we cannot rule out the possibility that the effects diminished somewhat over time.

In the baseline model, we make the implicit assumption that the relationship between salary perceptions and behavior is log-log linear. In Appendix D.10, we use binned scatter-plots to demonstrate that this log-log linear specification fits the data well. Appendix D.10 also explores the possibility of asymmetries. Regarding vertical comparisons, finding out that the managers are paid more than initially thought may have stronger or weaker effects than finding out that the managers are paid less than initially thought. For example, employees may find it easier or more difficult to adjust their effort upwards versus downwards, or they may be able to focus on good news while ignoring bad news (Eil and Rao, 2011). However, we do not find any significant evidence for this type of asymmetry. Regarding horizontal comparisons, there is evidence that average peer salaries may affect retention differently depending on whether the average peer salary is above or below the employee's own salary (Card et al., 2012; Dube et al., 2019; Breza et al., 2018). We find significant evidence of this same asymmetry in the effects on retention, but do not find significant evidence of such asymmetry in the effects on effort or performance. The statistical power available to conduct this type of analysis is limited, so one should not conclude the effects are perfectly symmetric. However, the evidence does suggest that the symmetry implied in the baseline specification is a sensible approximation. Finally, Appendix D.12 estimates the effects of salary perceptions on different subsamples of the population, such as male versus female employees, and finds no significant differences.

In the next sections, we discuss some potential interpretations of the findings and provide suggestive evidence of specific mechanisms at play.

5.4 Mechanisms: Career Concerns

The career-concerns model suggests that when employees find out that their managers earn more, they work harder because they want to be promoted to that position. The fact that we find positive effects of manager salary on effort is directionally consistent with the career-concerns mechanism. To probe this mechanism further, we provide two tests below.

The first test is based on the prediction that, to the extent that they aspire to be promoted to the managerial position, learning that the manager is better paid should make employees more optimistic about their own future salaries (for more details, see the model in Appendix C). To test this hypothesis, we estimate the effects of the perceived manager salary on the survey-based salary expectations. The effects of salary perceptions on these and other survey outcomes are presented in Table 4. Each of the five columns corresponds to a different dependent variable, based on the five questions included in the survey after the information-provision experiment. All coefficients are estimated with the same IV specification from Table 3. The only difference is that, when using survey outcomes, we do not observe pre-treatment outcomes so we cannot use them for falsification tests or include them as control variables.[21]

Column (1) of Table 4 corresponds to the effects on the (log) expected future salary one year in the future. Recall that managers are separated by multiple promotions from the respondents, so it is almost impossible that the respondent will be promoted to the manager's position in just a year (most likely, employees will remain in the same position a year later). Thus, if employees have realistic expectations, we should expect a null effect of manager salary on employees' salary expectations one year in the future. Consistent with this hypothesis, the coefficient on *Log(Manager-Salary)* from column (1) is positive (0.025) but close to zero, statistically insignificant, and precisely estimated. A 10% increase in perceived manager salary increases the expected salary one year in the future by a statistically insignificant 0.25%.

Column (2) of Table 4 corresponds to the effects on the (log) expected future salary in five years instead of just one year. According to our survey data, the average employee expects a probability of 55.8% of being promoted to the manager's position within the following five years. As a result, we would expect the average employee to use the information on manager salary to form expectations in this longer time horizon. Consistent with this hypothesis, the coefficient on *Log(Manager-Salary)* from column (5) is positive (0.166), precisely estimated, and highly statistically significant (p-value = 0.003). A 10% increase in perceived manager salary increases the expected salary five years in the future by 1.66%. Note that the magnitude of this effect of manager salary on salary expectations (elasticity of 0.166) is not only consistent in sign but also similar in magnitude to the effects of manager salary on effort (e.g., elasticities of 0.150 for hours worked and 0.130 for emails sent).

Table 4 also reports the coefficients on *Log(Peer-Salary)*. These coefficients are positive but less precisely estimated and statistically insignificant: 0.071 (p-value=0.431) with respect to salary one year in the future (column (1)) and 0.280 (p-value=0.111) with respect to salary five years in the future (column (2)). This constitutes weak evidence in favor of the "tunnel effect" from Hirschman and Rothschild (1973): in other words, employees become more

[21]To compensate for this lack of pre-treatment controls, we include some additional control variables: dummies for sales role, pay band, unit, and position title.

optimistic about their own future salary when they find out that their peers earn more.

The second test of the career-concerns model leverages heterogeneity in the distance between the employee's own position and the managerial position we asked them about. According to this model, the effects of perceived manager salary should be stronger for managerial positions the employee could realistically aspire to than to managerial positions that the employee could aspire to less.

The results from the heterogeneity analysis are presented in Table 5. These specifications are identical to the baseline specifications from Table 3 except that they allow the coefficients on manager and peer salary to be different across the following two groups: *Closer* and *Farther*, denoting whether the managerial positions are more or less accessible to the respondent. We use two alternative classifications for *Closer* and *Farther*. In the first model, which is presented in the top panel of Table 3, we split respondents based on whether their perceived probability of being promoted to the managerial position is below or above the median. Respondents in the group *Closer* expect to be promoted, on average, with a probability of 75.5%, while respondents in the group *Farther* expect to be promoted with an 18.1% probability.

In columns (1) and (2) of Table 5 the dependent variables are the expected future salary in one and five years, respectively. Column (1) indicates that the perceived manager salary does not affect expectations for one year in the future, regardless of the distance to the manager. The results from column (2) indicate that the manager salary increases the employees' own salary expectations five years in the future, but only for positions that are within reach: the coefficient on *Log(Manager-Salary)* is positive (0.204) and statistically significant (p-value=0.001) for the group *Closer* but smaller (0.086) and statistically insignificant (p-value= 0.349) for the group *Farther*. We find similar heterogeneity for the effects on effort and performance. Column (3), corresponding to hours worked, shows that the coefficient on *Log(Manager-Salary)* is positive (0.212) and statistically significant (p-value=0.033) for the group *Closer* but negative (-0.074) and statistically insignificant (p-value=0.424) for the group *Farther*. Column (4), corresponding to emails sent, shows a coefficient on *Log(Manager-Salary)* that is positive (0.170) and statistically significant (p-value=0.001) for the group *Closer* but smaller (0.019) and statistically insignificant (p-value=0.856) for the group *Farther*. Column (5), corresponding to sales, also shows significant effects for the group *Closer* and insignificant effects for the group *Farther*. However, the coefficient for the latter group is so imprecisely estimated that it makes it totally uninformative.

In the second model, presented in the bottom panel of Table 3, we use an alternative split between *Closer* and *Farther*, based on the expected numbers of promotions needed to reach the managerial position. The splits from Model 1 and Model 2 are similar but far from identical: the correlation between the expected probability of promotion and the expected number of promotions is statistically significant (p-value<0.001) and large (-0.415) but still substantially short of -1. The results from this second model are consistent with the first model: the effects of manager salary are large and statistically significant for the group *Closer* and close to zero and statistically insignificant for the group *Farther*. In summary, the evidence from Table 5 shows that, consistent with the career-concerns model, the effects of manager salary are driven by the managerial positions that the employees can aspire to.[22]

Lastly, when we consider how our results generalize to other contexts, we note the importance of upward mobility. In some contexts, where employees have little expectation of reaching higher echelons in their organizations (e.g., Lyft drivers), disclosing the salaries of managers may not generate the same motivation as in our context, where movement along the career ladder is frequent.

5.5 Mechanisms: Social Preferences

The fact that manager salary motivates employees goes against the predictions of the social-preferences mechanism, according to which employees will be demoralized by the size of the manager's paycheck. It is possible that social preferences are still at play but are outweighed by the motivating effects of career concerns. In this section, we use the survey outcomes to probe the relevance of social preferences.

Columns (3) - (5) of Table 4 show the effects of salary perceptions on the proxies for employee morale (pay satisfaction and job satisfaction) and tolerance for inequality. Finding effects on any of these outcomes would constitute suggestive evidence in favor of the social-preferences channel. Column (3) shows that the effect of *Log(Manager-Salary)* on pay satisfaction is close to zero (-0.015), statistically insignificant (p-value=0.906), and precisely estimated. Columns (4) and (5) show that the corresponding effects on job satisfaction and tolerance for inequality are also close to zero (-0.086 and 0.008), statistically insignificant (p-value=0.399 and 0.920), and precisely estimated. These three coefficients imply that a 10% increase in manager salary would reduce pay satisfaction by a mere 0.16% of a standard deviation, job satisfaction by 1.1% of a standard deviation, and tolerance for inequality by just 0.14% of a standard deviation.

In contrast to the findings for manager salary, we find evidence that the peer salary has significant effects on these same three survey outcomes. In column (3), the effect of peer

[22]These differences in coefficients between *Closer* and *Farther* must be taken with a grain of salt, however: due to the lower precision of the estimates, each specific pairwise difference can be large in magnitude but still statistically insignificant (all of the p-values for the pairwise differences are presented at the bottom of Table 5). On the other hand, the fact that these differences are so robust across models and across outcomes suggests that they are meaningful.

salary on pay satisfaction is negative (-0.762) and statistically significant (p-value=0.078). This effect is economically large, implying that a 10% higher peer salary decreases pay satisfaction by roughly 8.3% of a standard deviation. Moreover, we can reject the null hypothesis that the coefficient on peer salary (-0.762) is equal to the coefficient on manager salary (-0.015), with a p-value=0.084. The coefficient for job satisfaction (-0.444, from column (4)) is similar in magnitude (and statistically indistinguishable from) the corresponding effect on pay satisfaction (-0.762, from column (3)). However, we should take this coefficient with a grain of salt because, due to the imprecision of the estimate, it is not statistically significant (p-value=0.366). The peer salary coefficient from column (5) is negative (-0.373) and statistically significant (p-value=0.084). This coefficient implies that a 10% increase in peer salary reduces inequality tolerance by 6.5% of a standard deviation.

One interpretation for the above findings is that social preferences do not play a role in the vertical comparisons but do play a role in the horizontal comparisons. This result is particularly stunning in light of how much larger the vertical salary gaps are, relative to gaps in peer salaries. We can speculate why social preferences are activated by horizontal comparisons but not vertical comparisons. The broader literature on concerns about relative standing (Clark and Senik, 2010) argues that individuals tend to compare themselves to specific reference groups. In our context, employees may feel naturally inclined to compare their salaries to the salaries of their peers, creating morale effects, while they may not feel the need to compare themselves to their managers. A second explanation is based on beliefs about fairness. Employees may feel demoralized about horizontal comparisons because, given their common responsibilities, they perceive these salary differences as unfair Breza et al. (2018). Employees may think that these horizontal salary differences are due to unmeritocratic factors such as luck or workplace connections (Cullen and Perez-Truglia, 2019).[23] Employees may find it easier to justify vertical inequality because, for instance, they may think that managers deserve higher salaries because they add more value to the firm or because they worked hard to get to that position. Indeed, this interpretation echoes a robust finding in the literature about preferences for redistribution that some poor people do not want to tax the rich because they think the rich are deserving of their wealth (Di Tella et al., 2016).

It is also important to note that while the above evidence suggests that social preferences may play a role in horizontal comparisons, we by no means suggest that this is the only or even the main mechanism at play. Since the horizontal comparisons are not the main object of study in this paper, we defer the discussion to Appendix D.13, where we discuss and present evidence related to other mechanisms, such as whether employees learn about their

[23] However, employees could also attribute these salary differences to meritocratic factors, such as being able to attract outside offers (Caldwell and Harmon, 2018).

productivity, their returns to effort, their chances of being promoted, and other channels.

Regarding the finding that social preferences appear to be absent in vertical comparisons, we caution that these results could depend on the working environment. We study a private sector organization operating in a competitive financial industry, and the general view inside the organization is that promotions are largely performance based. If instead we were studying public employees in a corrupt government, or offspring operating a family business, then disclosure of large differences in pay between employees and management could elicit very different reactions, including anger and resentment. We hope that future work can shed light on the potential for different organizational features to affect attitudes toward pay inequality.

6 Conclusions

We presented the results from a field experiment involving 2,060 employees from a multibillion-dollar corporation. The research design combines survey data, administrative data, and an information-provision experiment to shed light on how employees learn about the salaries of their managers and peers, and how those beliefs affect their own behavior.

We documented large misperceptions of the salaries of managers and peers and identified some of the sources of these misperceptions. We showed that perceptions of the salaries of managers and peers have significant effects on the employee's own behavior. We find that the reaction to the manager salary is largely motivating, consistent with a career-concerns channel, but we do not find any evidence that social preferences are at play when employees are confronted with these vertical salary comparisons. In stark contrast, social preferences seem to be present in how employees react to horizontal pay differences.

Our findings have implications for understanding how firms operate. We find that rewarding one employee with a higher salary has a negative externality on the effort of all peers. In contrast, increasing the salary of the manager level has a positive externality on the behavior of all employees who aspire to be promoted to that level. Because of these externalities, firms may find it optimal to load rewards vertically rather than horizontally. Indeed, these findings may help to explain why firms tend to provide financial incentives vertically, in the form of promotions, rather than providing horizontal incentives such as pay-for-performance (Baker et al., 1988).

Lastly, the view that social preferences create incentives for the firm to compress salary differentials internally is widespread (Frank, 1984). This argument is commonly made by policy makers when promoting pay transparency. Moreover, some firms even experimented with capping the ratio of earnings at the top and bottom with the explicit intention of increasing employee morale (Dvorak, 2007). Our evidence offers an explanation for why such policies did not gain more traction. While transparency may pressure firms to reduce horizontal inequality, our findings suggest that employees' reactions are unlikely to place similar constraints on vertical inequality. Our evidence thus suggests that transparency policies may not be nearly as effective at curbing inequality as previously thought.

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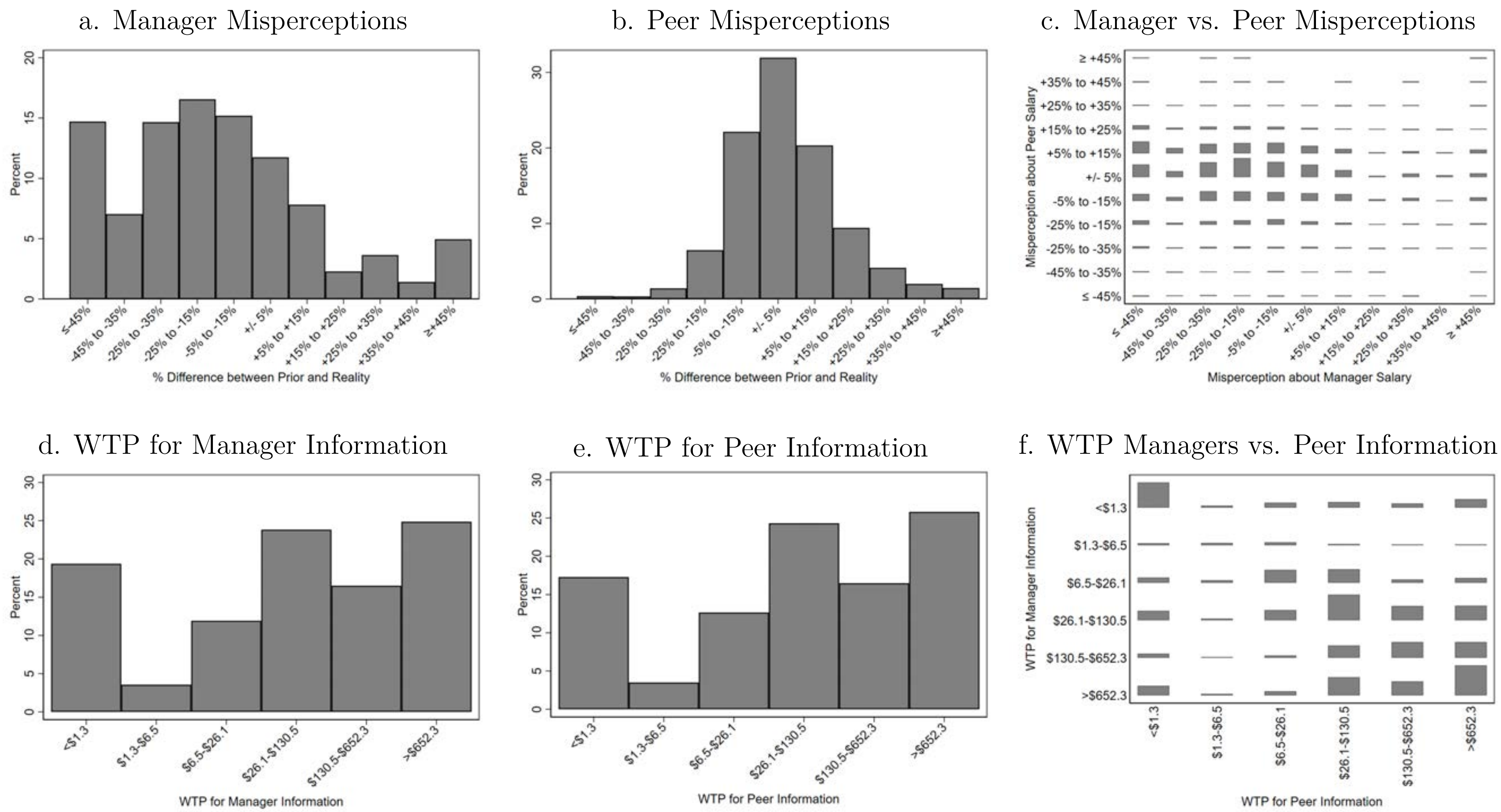
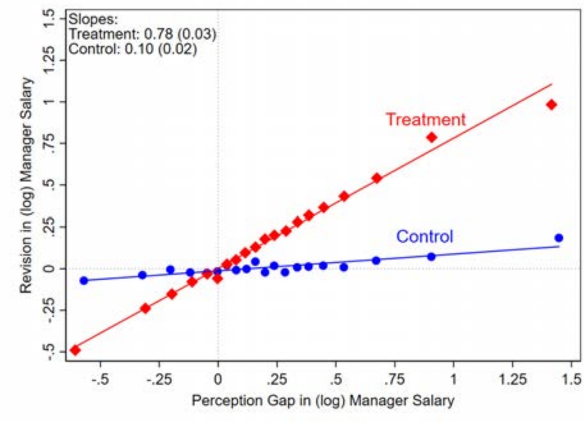
Figure 1: Salary Misperceptions and Willingness to Pay for Salary Information



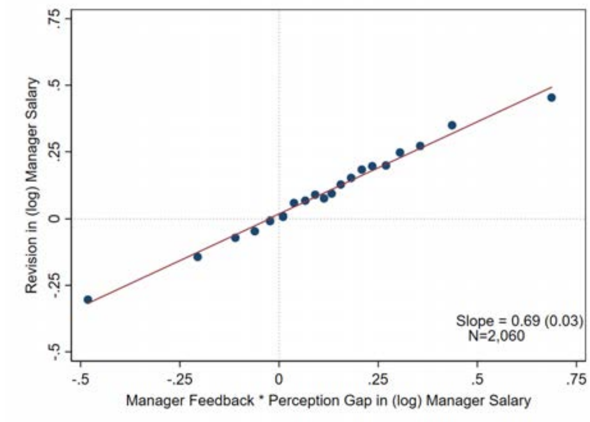
Notes: Panel (a)–(c) correspond to salary misperceptions defined as the employee's prior belief (according an incentivized survey question) and the actual salary (according to the firm's administrative records), divided by the actual salary (N=2,060). Panel (a) is about the average manager salary, panel (b) is about the average peer salary, and panel (c) is about the joint distribution of (a) and (b). Panels (d)–(e) shows the willingness to pay (WTP) for a specific information piece (an average based on a sample of five manager/peer salaries) based on the responses to multiple price list questions. Panel (a) corresponds to the WTP for the manager information (N=1,637 respondents with consistent responses across the five scenarios). Panel (b) corresponds to the WTP for peer information (N=1,748 respondents with consistent responses across the five scenarios). Panel (c) corresponds to the joint distribution of (a) and (b) (N=1,478 respondents with consistent responses for both the manager and peer scenarios).

Figure 2: Effects of Information Provision Experiment on Salary Perceptions

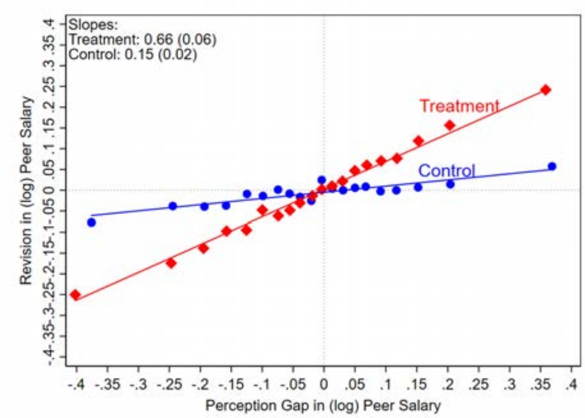
a. Manager Salary: Treatment vs. Control



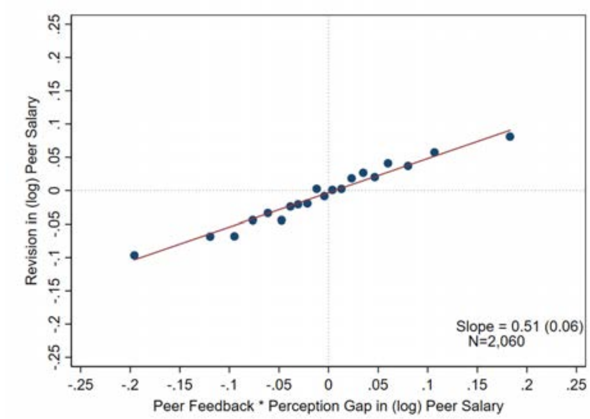
b. Manager Salary: Bayesian Learning



c. Peer Salary: Treatment vs. Control



d. Peer Salary: Bayesian Learning



Notes: N=2,060. In panel (a), the y-axis is the respondent's update (i.e., the posterior belief about the average manager salary minus the corresponding prior belief) and the x-axis corresponds to the difference between the feedback chosen for the employee (the average salary among the random sample of 5 managers) and the respondent's prior belief. The red diamonds (labeled "Treatment") correspond to the respondents who were shown the feedback about manager salary and the blue circles correspond to the respondents who were not shown such feedback. Panel (b) estimates the Bayesian learning equation (2) from Section 4.3. The y-axis is the same as that from panel (a), while the x-axis corresponds to the same x-axis from panel (b) but multiplied by a binary variable for whether the information was randomly chosen to be shown to the respondent. The regression controls for the difference between the feedback chosen for the employee and the employee's prior belief; also, it controls for the prior belief and position title dummies. The dots correspond to the binned scatterplot, the slope to the linear regression, and the standard error are clustered at the position level and presented in parentheses. Panel (c) and (d) are equivalent to panel (a) and (b) except that they are about peer salary instead of manager salary.

Table 1: Descriptive Statistics and Randomization Balance Test

	All	Treatment Group				
		Manager	Peer	Both	None	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.73	0.74	0.71	0.74	0.74	0.32
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	
Age	29.20	29.35	29.35	28.92	29.19	0.99
	(0.11)	(0.22)	(0.21)	(0.19)	(0.22)	
College (or Higher)	0.86	0.84	0.87	0.86	0.86	0.14
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	
Tenure (Years)	4.99	5.14	5.08	4.92	4.79	0.81
	(0.08)	(0.16)	(0.15)	(0.14)	(0.16)	
Own Salary (Masked)	0.72	0.72	0.72	0.72	0.72	0.93
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	
Avg. Manager Salary (Masked)	2.84	2.80	2.89	2.86	2.80	0.54
	(0.05)	(0.10)	(0.10)	(0.10)	(0.11)	
Avg. Peer Salary (Masked)	0.72	0.72	0.72	0.73	0.73	0.91
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	
Observations	2,060	510	528	559	463	

Notes: Average pre-treatment characteristics of the employees, with standard errors in parentheses. *Female* takes the value 1 if the employee is female and 0 otherwise. *Age* is the employee's age (in years) as of March 2017. *College* takes the value 1 if the employee finished College or a higher degree, and 0 otherwise. *Tenure* is the number of years from the date when the employee joined the company until March 2017. *Own Salary* is the employee base monthly salary as of March 2017. *Avg. Manager Salary* and *Avg. Peer Salary* are the true average salaries among the manager and peer groups, respectively. Due to the sensitive nature of the data, we do not reveal the unit of measurement for salary variables. Column (1) corresponds to the entire subject pool, while columns (2) through (5) correspond to the four treatment groups that subjects were randomly assigned to: receiving information about the average manager salary only (column (2)); receiving information about the average peer salary only (column (3)); receiving information about both manager and peer salary (column (4)); and receiving no salary information (column (5)).

Table 2: Information Diffusion

	Misperceptions on Manager Salary					Misperceptions on Peer Salary				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Information Assignment										
Direct	-0.205***	-0.204***	-0.215***	-0.214***	-0.198***	-0.045***	-0.043***	-0.043***	-0.042***	-0.045***
	(0.016)	(0.017)	(0.019)	(0.024)	(0.019)	(0.005)	(0.005)	(0.006)	(0.008)	(0.006)
Indirect: Closest Peer		0.004					0.014			
		(0.025)					(0.011)			
Indirect: No. Peers			-0.003					0.001		
			(0.004)					(0.001)		
Indirect: (No. Peers > 0)				-0.013					0.005	
				(0.027)					(0.009)	
Indirect: Share of Peers					0.053					0.005
					(0.100)					(0.026)
Constant	0.307***	0.307***	0.307***	0.312***	0.304***	0.108***	0.106***	0.107***	0.105***	0.107***
	(0.025)	(0.025)	(0.025)	(0.027)	(0.025)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
Observations	2,060	2,060	2,060	2,060	2,060	2,060	2,060	2,060	2,060	2,060

Notes: N= 2,060. Significant at *10%, **5%, ***1%. Standard errors in parentheses clustered at the peer group level. Each column corresponds to a different regression. All regressions follow the econometric model described in Section 4.4. In columns (1)-(5), the dependent variable is the absolute value of the %-difference between the posterior belief about average manager salary and the true average. *Direct* is a binary variable indicating if the subject received the signal on manager salary. The other independent variables measure if the peers of the subject received the information before the subject completed the survey. Those variables take the value 0 if the employee received the information directly. *Closest Peer* is a binary variable indicating if the individual's closest peer (defined as the peer with whom the employee exchanges the most number of emails in Jan-Mar 2017) received the information. *No. Peers* is the number of peers who received the information. *(No. Peers>0)* is a binary variable indicating if at least one peer received the information. *Share of Peers* is the share of peers who received information. All regressions include as control variables the date when the survey was completed, peer group size and the number and proportion of the peer group invited to the survey. Columns (6)-(10) are equivalent to columns (1)-(5), but using peer salary instead of manager salary. See Appendix Table D.4 for descriptive statistics.

Table 3: Effects of Salary Perceptions on Behavior

	Effort and Performance			Career Moves			
	log($Hours$)	log($Emails$)	log($Sales$)	P(Left)	P(Transfer)	log($Salary$)	$P(\Delta Title)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-Treatment:							
Log (Manager-Salary)$^{(i)}$	0.150**	0.130***	0.106	-0.015	-0.003	0.002	0.012
	(0.074)	(0.041)	(0.122)	(0.022)	(0.030)	(0.011)	(0.029)
Log (Peer-Salary)$^{(ii)}$	-0.943**	-0.431**	-0.731**	0.235**	0.093	0.004	0.114
	(0.472)	(0.210)	(0.297)	(0.107)	(0.106)	(0.052)	(0.123)
Pre-Treatment (Falsification):							
Log (Manager-Salary)	0.001	-0.101	0.063	-0.022	0.029	0.002**	0.009
	(0.114)	(0.071)	(0.160)	(0.050)	(0.029)	(0.001)	(0.010)
Log (Peer-Salary)	-0.205	-0.184	-0.191	-0.139	0.212	-0.001	-0.071*
	(0.542)	(0.289)	(0.412)	(0.218)	(0.163)	(0.005)	(0.040)
P-value H_0: (i)=(ii)	0.026	0.007	$<$0.001	0.015	0.398	0.963	0.424
Cragg-Donald F-Stat.	29.8	204.0	98.2	203.7	203.4	203.6	203.3
Mean Outcome	5.98	35.57	0.48	0.05	0.09	0.92	0.10
Std. Dev. Outcome	1.88	44.93	0.23	0.21	0.28	0.70	0.30
Observations	602	2,060	791	2,060	2,060	2,060	2,060

Notes: Significant at *10%, **5%, ***1%. Standard errors in parentheses clustered at the position level. Each column presents results for two sets of IV regressions, following the specification from Section 5.1: in *Post-Treatment*, the dependent variable is the average behavior 90 days after the completion of the survey; in *Pre-Treatment (Falsification)*, the dependent variable is the average behavior before the completion of the survey. *Manager-Salary* is the posterior belief about the average manager salary and *Peer-Salary* is the posterior belief about the average peer salary. The regressions control for three monthly lags of the dependent variable, (log) own salary, (log) tenure, and five productivity rating dummies. *Hours* is the daily number of hours worked. *Emails* is the daily number of emails sent. *Sales* is the sales performance index. $P(Left)$, $P(Transfer)$ and $P(\Delta Title)$ are dummies for whether the employee leaves the firm, transfers inside the firm and changes position title, respectively. log($Salary$) is the logarithm of own salary at the end (beginning) of the post-treatment (pre-treatment) period. For the dependent variables in columns (1), (2), (3) and (6), the mean and std. dev. reported in the bottom rows correspond to the values prior to taking the logarithm function. Columns (1) corresponds to the subsample of employees in the headquarter offices and column (3) to the subsample of employees with sales roles.

Table 4: Effects of Salary Perceptions on Survey Outcomes

	Log(E[Future Salary])		Rank(Prod.)	Satisfaction		Ineq. Tol.
	+1 year	+5 years		w/Pay	w/Job	
	(1)	(2)	(3)	(4)	(5)	(6)
Log (Manager-Salary)$^{(i)}$	0.025	0.166***	0.000	-0.015	-0.086	-0.008
	(0.025)	(0.055)	(0.015)	(0.125)	(0.102)	(0.075)
Log (Peer-Salary)$^{(ii)}$	0.071	0.280	0.044	-0.762*	-0.444	-0.373*
	(0.090)	(0.176)	(0.040)	(0.433)	(0.491)	(0.216)
P-Value (i)=(ii)	0.595	0.532	0.280	0.084	0.433	0.135
Cragg-Donald F-Stat.	253.5	255.3	250.5	253.6	254.3	254.3
Mean Dep. Var.	2.58	3.22	0.47	2.79	3.60	1.80
Std. Dev. Dep. Var.	0.51	0.59	0.22	0.92	0.78	0.57
Observations	2,033	2,026	1,999	2,030	2,027	2,027

Notes: Significant at *10%, **5%, ***1%. Standard errors in parentheses clustered at the position level. Each column presents results for a different IV regressions, following the specification described in Section 5.1. *Manager-Salary* is the posterior belief about manager salary, and *Peer-Salary* is the posterior belief about the average peer salary. All the dependent variables correspond to survey questions asked after the elicitation of the posterior beliefs. *E[Future Salary]* corresponds to the expected salary one and five years in the future. *Satisfaction with Pay* and *Satisfaction with Job* are measures in a 5-point scale from very dissatisfied (1) to very satisfied (5). *Ineq. Tol.* measures tolerance for pay inequality in a 3-point scale. All regressions include the following control variables: the log of own salary, log of tenure, and sets of dummies for sales role, pay band, unit, productivity rating and position title.

Table 5: Effects of Perceived Manager Salary by Distance to Manager

	Log(E[Future Salary])		Effort and Performance		
	+1 year	+5 years	log($Hours$)	log($Emails$)	log($Sales$)
	(1)	(2)	(3)	(4)	(5)
Model 1 (by Promotion Prob.):					
Log (Manager-Salary)					
Closer$^{(i)}$	0.041	0.204***	0.212**	0.170***	0.437***
	(0.030)	(0.059)	(0.099)	(0.052)	(0.154)
Farther$^{(ii)}$	-0.008	0.086	-0.074	0.019	0.468
	(0.033)	(0.092)	(0.093)	(0.104)	(0.755)
Model 2 (by No. of Promotions):					
Log (Manager-Salary)					
Closer$^{(iii)}$	0.008	0.200***	0.431*	0.185***	0.437**
	(0.036)	(0.059)	(0.226)	(0.061)	(0.200)
Farther$^{(iv)}$	0.057	0.134	-0.016	0.068	0.516
	(0.038)	(0.096)	(0.135)	(0.062)	(0.526)
P-value H_0 : (i)=(ii)	0.216	0.229	0.040	0.243	0.972
P-value H_0 : (iii)=(iv)	0.322	0.560	0.170	0.212	0.908
Observations	2,033	2,026	602	2,060	755

Notes: Significant at *10%, **5%, ***1%. Standard errors in parentheses clustered at the position level. Each column shows results from two regressions (one for each model). In Model 1, *Closer* indicates a probability of reaching the managerial position above 40%. In Model 2, *Closer* indicates managerial positions that are less than 5 promotions ahead. The regressions in columns (1)–(2) follow the same specification used in columns (1)–(2) of Table 4 (see notes therein for more details), and the regressions from columns (3)–(5) follow the same specifications used in columns (1)–(3) of Table 3 (see notes therein for more details). The only difference with the baseline IV regressions of Tables 4 and 3 is the addition of the binary variable *Closer* as control variable as well as its interaction with *Log(Manager-Salary)*. *E[Future Salary]* correspond to the expected salary one and five years in the future. *Hours* is the daily number of hours worked. *Emails* is the daily number of emails sent. *Sales* is the sales performance index.