How Much Does Your Boss Make?
The Effects of Salary Comparisons

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Abstract

We study how employees learn about the salaries of their peers and managers, and how those beliefs affect their own behavior. We conducted a field experiment with a sample of 2,000 employees from a multi-billion-dollar corporation. We combine rich data from surveys and administrative records with an experiment that provided some employees with accurate information about the salaries of others. First, we document large misperceptions about salaries and identify some of the sources of these misperceptions. Second, we find significant behavioral elasticities with respect to the perceived salaries of other employees. These effects are different for horizontal and vertical comparisons: while higher perceived peer salary decreases effort, output and retention, higher perceived manager salary has a positive effect on those same outcomes. We discuss evidence on the underlying mechanisms, and implications for pay inequality and pay transparency.

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1 Introduction

Employees may care about the salaries of other employees inside the firm, such as the salaries of peers and managers. They may use this information to form expectations about their own future salary, or they may have fairness or inequity concerns. As a result, changing the salary of one employee can affect the behavior of other employees in the firm. These externalities can have important implications for the provision of incentives within the firm. In this paper, we address this question using a large-scale, high-stakes field experiment in collaboration with a multi-billion-dollar corporation.

We designed and conducted a field experiment involving a sample of 2,000 employees from a large commercial bank. The research design allows us to explore how employees learn about the salaries of their peers and managers, and how those beliefs affect their own behavior. To do this, we combine three sources of data: a tailored survey, information-provision and information-acquisition experiments, and rich administrative data on the behavior of the employees such as real time data on the swipes in and out of the office, emails sent and received, sales performance, promotions, transfers and exits.

The first part of the research design addresses the formation of salary perceptions. Employees report to be equally interested in learning about the salaries of their peers (i.e., employees in the same position and unit) and about the salaries of their superiors. Our survey elicited beliefs about the average salary among one’s peers and the average salary among one’s managers, using an incentivized method. This research design distinguishes between two forms of pay comparisons: horizontal comparisons (i.e., own salary relative to peer salary) and vertical comparisons (i.e., own salary relative to the manager salary).

By comparing the perceived salaries with the actual salaries from the firm’s administrative records, we can measure salary misperceptions. Moreover, to disentangle the sources of these misperceptions, we embedded an information-acquisition experiment: we elicited the willingness to pay for information about peer salary and manager salary, using an incentive-compatible method.

The second part of the research design addresses the behavioral effects of salary perceptions. We employ an information-provision experiment: for each employee, we flipped a coin to decide whether she would receive a piece of information about the average salary among peers; and we flipped another coin to decide if the employee would receive a piece of information about the average salary among managers.

Because employees react to the information provided to them, this experiment generated exogenous variation in the salary perceptions that the employees held after the information-provision. We can use administrative records to see whether these exogenous shocks to
perceived salaries translated into differences in behavior in the days or months after the survey was completed. We introduce a simple instrumental-variables model that exploits the experimental variation in beliefs to estimate the structural parameters of interest, the cross-salary elasticities: e.g., the percent change in effort for each percent increase in the perceived manager salary.

The first set of findings documents significant information frictions. Employees have large and systematic misperceptions about the salaries of their managers and their peers. These misperceptions are substantial at all levels of the firm, and for all demographic subgroups. We show that a substantial part of these misperceptions are due to lack of information instead of just lack of interest: on average, employees are willing to pay 14 days worth of salary for a signal about the average salary of their peers, and a similar amount for a signal about the average salary of their managers.

We show evidence that the information frictions arise from barriers to social learning. Our information-experiment reveals that even though each employee learns from the information provided to herself, that information does not travel to other employees in the network, not even to her closest peers. This evidence is consistent with non-experimental evidence. We show that employees who gossip more and employees who are more central in the network do not have more accurate beliefs about the average peer salary. Moreover, employees perceptions about average peer salary are not more accurate than what you would expect if their own salary was the only information that they had access to.

The second set of findings documents large effects of salary perceptions on employee behavior. The cross-salary elasticities are statistically and economically significant, and their sign depends on whether the comparisons are vertical or horizontal. A higher perceived peer salary has negative effects on effort, output and retention. For example, a 1% increase in perceived peer salary decreases the number of emails sent by 0.63%, implying a behavioral elasticity of -0.63. Vertical comparisons have the opposite effects as horizontal comparisons: higher perceived manager salary has positive effects on effort, output and retention. For instance, a 1% increase in perceived manager salary increases the number of emails sent by 0.18%, implying a behavioral elasticity of 0.18.

These behavioral effects are persistent over time, they are distributed homogeneously through different groups of employees, and they are robust across different specifications. We do not find evidence of asymmetric effects, such as peer comparisons depending on whether the employee is above or below the average peer salary – however, we cannot rule out moderate asymmetries.

We provide some suggestive evidence about the mechanisms behind these behavioral elasticities. To accomplish this, we measure the effects of perceptions on survey outcomes.
(e.g., future salary expectations), and we exploit heterogeneity analysis. Regarding horizontal comparisons, we find evidence of two channels being at play. On the one hand, higher peer salary increases expected future salary, which motivates employees. However, there is a separate negative channel that trumps the first channel, which could be attributed to social concerns. Regarding vertical comparisons, we find again evidence of career concerns: higher manager salary increases expected future salary, which motivates employees. However, we do not find any evidence of social concerns being at play in vertical comparisons.

Since it increased pay transparency at the margin, our information-provision experiment allows for an impact-evaluation of pay transparency. We find that higher transparency has an insignificant average effect on most forms of behavior, except for retention, for which it has a positive and significant average effect. This evidence suggests that this firm may be better off by increasing pay transparency at the margin. At the same time, our evidence suggests that the average employee would benefit from higher transparency. First, transparency provides employees with information for which they revealed to be willing to pay significant amounts. Second, according to survey data, the vast majority of employees report to be in favor of higher transparency as long as it discloses salary averages by positions instead of revealing the salaries of specific employees.

Our findings have a number of implications. First, we find that horizontal rewards have negative externalities while vertical rewards have positive externalities. This provides an explanation for the robust fact that firms provide most of the financial incentives vertically (e.g., promotions) instead of horizontally (e.g., performance pay). Similarly, our evidence can also explain why firms discriminate against minorities, such as females, vertically rather than horizontally: e.g., in the firm where the experiment was conducted, 92% of the gender pay gap comes from vertical differences and only 8% through horizontal differences.

Second, there is a widespread view that social comparisons put pressure to compress salary differentials within the firm (Frank, 1984; Hamermesh, 1975), and a similar morale-based argument is used to explain wage rigidities (e.g., Solow, 1979; Bewley, 1999). Our findings suggest that this view is only true in a narrow sense: while this channel may force firms to reduce horizontal inequality, firms are not restricted in their use of vertical inequality. Moreover, our findings suggest that policies aimed at reducing pay inequality by promoting pay transparency may be less effective than previously thought.

Our paper is related to several strands of literature. While there is a long standing theoretical literature on relative salary (e.g., Frank, 1984; Romer, 1984; Summers, 1988; Akerlof and Yellen, 1990), the empirical evidence has been lagging behind. In a seminal contribution, Card, Mas, Moretti and Saez (2012) conducted a field experiment to explore the effects of wage transparency in a university. They took a sample of university employees,
and sent a random subsample of them an email with information about website that listed the salaries of every employee at that university. One week later, the researchers sent a follow-up survey to the entire subject pool. They found that, for workers with below-median salaries within their occupation, receiving the link to the website decreased job satisfaction and increased the stated job search effort.

Since Card et al. (2012), others have studied the broader consequences of pay transparency (Cullen and Pakzad-Hurson, 2015; Perez-Truglia, 2015; Rege and Solli, 2015; Mas, 2017). Additionally, there is a long standing literature in psychology and economics that measures the effects of unequal piece-rates on task performance, both in the laboratory (Charness and Kuhn, 2007; Clark, Masclet and Villeval, 2010) and in the field (Pritchard, Dunnette & Jorgenson, 1972; Valenzi and Andrews, 1971; Schmitt and Marwell, 1972; Breza, Kaur, Shamdasan, 2017; Huet-Vaughn, 2017).

Our study makes four key contributions to the existing literature. First, while the existing evidence focuses on horizontal comparisons, we are the first to incorporate vertical comparisons as well. This is important because vertical inequality comprises the vast majority of the within-firm inequality. In the firm where our experiment was conducted, 95% of the pay inequality is vertical and the remaining 5% is horizontal – this decomposition is in the same order of magnitude for other firms.¹ Since individuals react with opposite signs to horizontal and vertical comparisons, the distinction between the two ends up being critical.

Second, we contribute from a methodological perspective. Rather than estimating the reduced-form effects of inequality or transparency, our novel research design identifies the structural parameters of interest, the cross-salary elasticities, by combining an information provision experiment, survey data and behavioral data.

Third, this study is unique in terms of the scope of the field experiment. This is a high-stakes environment for our subjects, with thousands of careers and billions in revenues at stake. This firm provides an environment that is quite representative of large firms around the world in key aspects such as pay transparency, salary inequality and norms about pay secrecy. Moreover, the close collaboration with the firm allowed us to measure some aspects of the employee’s life that had not been studied before, such as their behavior, perceptions, willingness to pay for information and diffusion of information in the network, among others.

Fourth, our information-provision experiment provides arguably the first impact-evaluation analysis of higher pay transparency. Most companies in the world, including the firm where this experiment was implemented, actively choose not to be transparent about pay (??) even though they do not have hard evidence that such policy is in the firm’s best interest. Even though we cannot generalize the findings to all firms, our evidence at least suggests

¹Details about the inequality decomposition reported in Appendix B.
that more firms should experiment with higher transparency, and that governments should consider disclosure policies too.

Our study also contributes to a growing literature showing that individuals have substantial misperceptions of their relative income (e.g., Cruces, Perez-Truglia, and Tetaz 2013; Karadja, Mollerstrom, and Seim 2017). While this literature shows that correcting these misperceptions has significant effects on stated preferences for redistribution, there is little evidence on whether these misperceptions affect behavior (Bottan and Perez-Truglia, 2017). We show that misperceptions about relative salary can have substantial consequences for behavior.

The rest of the paper proceeds as follows. Sections 2 and 4 describe the research design and the survey design, respectively. Section 4 presents the implementation details and the administrative data. Section 5 presents the results about the formation of salary perceptions. Section 6 presents the results on behavioral elasticities. Section 7 presents the impact-evaluation of transparency. The last section concludes.

2 Research Design

In this section, we introduce the conceptual framework, hypotheses and the econometric model used to identify the behavioral elasticities.

2.1 Conceptual Framework and Hypotheses

We focus on perceptions of average salary among peers and among managers. Following Card et al. (2012), we define peers as those employees who share the same position title and organizational unit. For instance, the peers of a teller in a certain branch would be all the other tellers in the same branch; the peers of a junior researcher in the investment banking division would be all the other junior researchers in the same division. In turn, one’s manager can consist of employees who are in positions of higher level, which supervise the individual’s work to some extent and to which the employee could aspire to be promoted to. For example, the managers of a teller could be the teller supervisors; the managers of a junior researcher in the investment banking division could be the senior researchers.

Let subscript $i$ index employees. Let $O_i$ be $i$’s own salary – think of basic salary, without any commissions or bonuses. Let $P_i$ be $i$’s perception about the average salary among $i$’s peers. Let $M_i$ be $i$’s perception about the average salary among $i$’s managers.

Let $Y_i$ be a form of employee $i$’s behavior, such as the effort or sales performance. The following equation establishes the potential relationship between behavior and salaries:
\[
\log(Y_i) = \eta_0 + \eta_{\text{own}} \cdot \log(O_i) + \eta_{\text{peer}} \cdot \log(P_i) + \eta_{\text{mgr}} \cdot \log(M_i) \tag{1}
\]

The parameter \(\eta_{\text{own}}\) denotes the own-salary elasticity. If \(Y_i\) is the labor supply, then \(\eta_{\text{own}}\) would correspond to usual labor supply elasticity. In addition to the own-salary elasticity, equation (1) allows employees to react to their perceptions of the salaries of other employees, through \(\eta_{\text{peer}}\) and \(\eta_{\text{mgr}}\). We denote these two parameters as the cross-salary elasticities.

Equation (1) makes a number of simplifying assumptions. Some of these functional form assumptions, such as linearity and symmetry, will be relaxed in the empirical section. Also, our baseline specification does not allow employees to care about the average salary in other groups (e.g., their subordinates), or to care about other characteristics of the distribution of salaries (e.g., the maximum salary instead of the average salary). Ideally, we would estimate a fully saturated model where the right hand side of equation (1) includes one term for every employee in the company. However, estimating such model would be unfeasible.

If anything, to the extent that our choice of specification may miss the most important beliefs, our baseline model could only lead to under-estimation of the true cross-salary elasticities. We choose to focus our specification on the average salary of peers and managers based on prior studies, interviews with employees and with managers from the Human Resources division.

We collected survey data to validate this choice of specification. At the end of the survey, we asked employees to rank how interested they would be in learning about the average salary in different positions at the company: their same position, the positions right above their own level, two or more levels above, and other positions. The distribution of responses confirms that peer and manager salary are the single most important information for employees: roughly 50% of subjects ranked their own position first, 45% of subjects ranked higher positions, and less than 5% of respondents ranked other positions.

We classify the potential mechanisms at play in two main groups: career concerns and social concerns.

Career concerns refer to the possibility that worker’s effort depends not only on the current salary but also on expectations about future salary (Gibbons and Murphy, 1992; Holmstrom, 1999). When individuals find out about the salary of their peers and managers, they may update their expectations about their own future salary and may want to change their behavior accordingly. For instance, an individual who finds out that peers are getting paid more can use that information as leverage in future salary negotiations, thereby increasing the expected future salary. This change in expectations should make it more attractive to work at the firm and should affect the individual’s behavior, such as making her more likely to exert effort to retain her job, and less likely to search for another job. Similarly, if the individual
thinks there is a positive probability of reaching a given managerial position, an increase in
the perceived manager salary would also increase the expected own future salary. Again,
this change in expected salary should incentivize the individual to work harder and dissuade
her from looking for another job. As a result, this mechanism would predict $\eta_{\text{peer}} > 0$ and
$\eta_{\text{mgr}} > 0$.

On the other hand, social concerns refer to the possibility that employees have social
preferences over the distribution of salaries such as positional concerns, inequity aversion
and unfairness aversion (Frank, 1984; Romer, 1984; Summers, 1988; Akerlof and Yellen,
1990; Lazear, 1989). Consider for example the case of an employee whose morale depends
on his relative salary. A higher perceived peer salary should decrease the employee’s morale
and thus reduce the employee’s effort and the likelihood that the employee stays in the firm.
Similarly, a higher perceived manager salary should also decrease the employee’s morale and
thus reduce her effort and her willingness to stay at the firm. As a result, this mechanism
predicts $\eta_{\text{peer}} < 0$ and $\eta_{\text{mgr}} < 0$.

2.2 Econometric Model

In this section, we present the empirical framework used for identification.

Obtaining causal estimates of the cross-salary elasticities $\eta_{\text{peer}}$ and $\eta_{\text{mgr}}$ is challenging
since a simple regression of behavior on perceived salaries would be subject to the usual
concerns with omitted variable biases. For instance, individuals with lower perceived peer
salary may have lower ability, which would create a spurious elasticity $\eta_{\text{peer}} < 0$. Our
empirical framework exploits the random variation in beliefs induced by the information
provision experiments to estimate the cross-salary elasticities.

To understand the intuition of this model, consider a pair of employees who have the
same bias in perceived peer salary: both of them under-estimate the actual peer salary by
10%. Then, we randomly assign information about the true peer salary to one of these two
employees. The first stage of the regression measures the effect of the information provision
on beliefs. Let’s say that, relative to the individual who does not get the information, the
individual who gets the information ends up updating her perceived peer salary by 5%. The
reduced form regression follows these two individuals after they complete the survey, and
compares the behavior of the employee who was randomly chosen to receive the information
to the behavior of the employee who was randomly chosen not to receive the information.
Let’s say that, relative to the individual who does not get the information, the individual
who gets the information ends up exerting an additional 2.5% effort. The instrumental
variables model infers the elasticity by putting these two results together: in this example,
the information provision increases perceived peer salary by 5% and effort by 2.5%, which
implies a cross-salary elasticity of 0.5 ($= \frac{2.5}{5}$).

The above example followed a pair of individuals who under-estimated their peer salary by 10%. In practice, we may encounter pairs of employees who under-estimate the peer salary by 5%, others who over-estimate it by 15%, and so on. Thus, we would have to repeat this exercise for different pairs and then estimate an average elasticity by averaging over all of these pairs. We accomplish this by means of the following instrumental variables specification, which builds on a simple Bayesian learning model.

Let $P_{i, prior}$ denote the prior belief about the average salary of peers – that is, the belief right before the individual reaches the information-provision experiment. Let $P_{i, signal}$ be the value of the signal on average salary that is randomly assigned in the information-provision experiment. Let $T_{i, P}$ be a dummy variable that takes the value 1 if the individual is shown the signal and 0 if not. Denote $P_{i, post}$ as the corresponding posterior belief – that is the perceived peer salary right after the information-provision experiment was conducted. And define $M_{i, prior}, M_{i, signal}, T_{i, M}, M_{i, post}$ as the corresponding variables for perceived manager salary instead of perceived peer salary.

Let $Y_{i, post}$ denote some average behavior in the period beginning with the information provision experiment and ending some time later. For instance, $Y_{i, post}$ could be the average number of hours in the office in the 60 days right after the information-provision. Let $Y_{i, prior}$ denote the the average behavior in the period prior to the information-provision experiment instead of the period after it.

The first stage of the instrumental variables model corresponds to belief updating. When priors and signals are normally distributed, Bayesian learning implies that the mean of the posterior belief should be a weighted average between the signal and the mean of the prior belief. In the case of perceived peer salary, Bayesian learning can be summarized as follows:

$$P_{i, post} = \alpha \cdot (P_{i, signal} - P_{i, prior}) + P_{i, prior} \quad (2)$$

This simple linear model, or slight variations of it, has been found to have an excellent fit in a number of contexts such as inflation expectations (e.g., Armantier et al., 2016; Cavallo, Crues and Perez-Truglia, 2017) and perceptions of relative income (Bottan and Perez-Truglia, 2017). We can introduce the information provision experiment to this learning specification:

$$P_{i, post} = \alpha \cdot (P_{i, signal} - P_{i, prior}) \cdot T_{i, P} + \beta \cdot (P_{i, signal} - P_{i, prior}) + P_{i, prior} \quad (3)$$

The parameter $\alpha$ represents the learning rate, which ranges from 0 (individuals ignore the signal) to 1 (individuals fully adjust to the signal). In turn, the parameter $\beta$ controls for
any spurious reversion to the signal. This equation provides a simple way to summarize the effects of random assignment \((T^P_i)\) on the posterior beliefs \((P^\text{post}_i)\). The instrumental-variables model simply isolates that random variation in beliefs to identify the cross-elasticities:

\[
\log(Y^\text{post}_i) = \pi_0 + \eta_{\text{peer}} \cdot \hat{P}^\text{post}_i + \eta_{\text{mgr}} \cdot \hat{M}^\text{post}_i + \\
\pi_1 \cdot (P^\text{signal}_i - P^\text{prior}_i) + \pi_2 \cdot (M^\text{signal}_i - M^\text{prior}_i) + \pi_3 \cdot P^\text{prior}_i + \pi_4 \cdot M^\text{prior}_i + \epsilon_i \quad (4)
\]

\[
P^\text{post}_i = \nu_0 + \nu_1 \cdot (P^\text{signal}_i - P^\text{prior}_i) \cdot T^P_i + \nu_2 \cdot (M^\text{signal}_i - M^\text{prior}_i) \cdot T^M_i + \\
\nu_3 \cdot (P^\text{signal}_i - P^\text{prior}_i) + \nu_4 \cdot (M^\text{signal}_i - M^\text{prior}_i) + \nu_5 \cdot P^\text{prior}_i + \nu_6 \cdot M^\text{prior}_i + \xi^1_i \quad (5)
\]

\[
M^\text{post}_i = \mu_0 + \mu_1 \cdot (P^\text{signal}_i - P^\text{prior}_i) \cdot T^P_i + \mu_2 \cdot (M^\text{signal}_i - M^\text{prior}_i) \cdot T^M_i + \\
\mu_3 \cdot (P^\text{signal}_i - P^\text{prior}_i) + \mu_4 \cdot (M^\text{signal}_i - M^\text{prior}_i) + \mu_5 \cdot P^\text{prior}_i + \mu_6 \cdot M^\text{prior}_i + \xi^2_i \quad (6)
\]

The relevant exclusion restrictions for the estimation of \(\eta_{\text{peer}}\) and \(\eta_{\text{mgr}}\) are:

\[
E\left[(P^\text{signal}_i - P^\text{prior}_i) \cdot T^P_i \cdot \epsilon_i\right] = 0 \text{ and } E\left[(M^\text{signal}_i - M^\text{prior}_i) \cdot T^M_i \cdot \epsilon_i\right] = 0 \quad (7)
\]

The random assignment of \(\{T^P_i, T^M_i\}\) satisfies these exclusion restrictions. In practice, we include a set of additional control variables, with the goal of reducing the variance of the error term and thus improving the precision of the estimates. Most important, we control for pre-treatment outcomes, which is typically used in field experiments to gain precision (McKenzie, 2012). Additionally, we include controls for tenure (in months) and dummies for birthplace, gender and sales-role.

Last, we can exploit the timing of the intervention to provide a falsification test, in an event-study fashion. For that, we can estimate the same instrumental variables model from above, but using \(Y^\text{prior}_i\) instead of \(Y^\text{post}_i\) as dependent variable. Intuitively, the information-provision experiment should not affect the behavior in the pre-treatment period, because the individuals have not been exposed to the information yet. Thus, the cross-salary elasticities should be zero when we use pre-treatment outcomes as dependent variable.

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Footnote: For example, if individuals take a little additional time to think when asked a question a second time, they would tend to revert to the truth, even if they did not actually receive any further information from the experimenter.
3 Survey Design

A sample of the full online survey is included in Appendix ?? – to protect the identity of the firm, we stripped the formatting and identifying information. In this section, we discuss the most important aspects of its design.

3.1 Training

The first part of the survey was designed to train the respondents to understand the rest of the survey. The survey begins by explain how the incentivized questions work and why it is in the respondent’s best interest to respond honestly. To illustrate these principles in practice, we included some sample incentivized questions on topics that are unrelated to salaries. Additionally, we explain the definition of basic salary used in the rest of the survey. We chose to focus on basic salary: i.e., the salary before any additions or deductions such as taxes, allowances, commissions or bonuses. More specifically, we phrase all the questions about monthly basic salary. According to interviews with the HR department and employees who were not participating in the experiment, this is supposed to be the most important aspect of the compensation that employees paid attention to. Moreover, this basic salary accounts for 90% of the total compensation for the subjects in our sample.³

To confirm that respondents understood this definition, we asked them to provide a guess about their own salary for the month of March of 2017. We told them that in the next screen we would tell them if they got it right or not, and we would pay them a reward for accuracy. In the following screen we show them their guess and the truth side-by-side. If the respondent’s guess is not within 5% of the truth, we show them an additional screen explaining the definition again, and ask them whether they agree or not with our figure. This question was also intended to make it clear to the surveyor already knows the salary of the respondent, to minimize any concerns that respondents may not be honest in their salary responses because they worry about revealing information about their own pay.

3.2 Salary Perceptions

This is the main part of the survey, which is repeated twice: once regarding the average salary of peers, and a second time regarding the average salary of managers. Each of these modules was comprised of the following four steps:

³The main form of performance pay corresponds to sales commissions. Performance pay is important for employees at the very high level of the firm, but those employees are excluded from the sample.
Step 1 (Elicit Prior Belief): We ask respondents about the average monthly basic salary among their peers/manager. To elicit truthful responses, we offered reward for accuracy using the traditional quadratic loss function (up to $2.61 per question). Moreover, to have a proxy for certainty in beliefs, we also elicited the distribution of beliefs over a series of bins around the respondent’s guess – this question was incentivized as well.

Step 2 (Elicit Maximum Willingness to Pay): We offered respondents to buy information about the average salary computed over a random sample of five peers/managers. To elicit this information in an incentive-compatible way, we employed the multiple price list variation of Becker-DeGroot-Marschak (Andersen et al., 2006). This method consists of making respondents choose in five hypothetical scenarios. In each scenario, they were given the choice of either seeing the information about average salary of peers/manager or receiving extra money as part of the survey reward. The five scenarios differed in the reward amount: $1.3, $6.5, $26.1, $130.5 and $652.3. We explained to subjects that it was in their best interest to choose truthfully, because with a 1% probability one of the five scenarios would be randomly chosen to be executed. For the 2% of respondents who had their scenarios executed (1% for the peer salary and 1% for the manager salary), that was the end of the experiment and thus we exclude them from the subject pool.

Step 3 (Information-Provision Experiment): There were two pieces of information to be randomly allocated: the average salary among a random sample of five peers; and the average salary among a random sample of five managers. We cross-randomized these two pieces of information. As a result, there were four treatment groups, with equal probability of assignment: one group receives a signal about the average salary of peers but no salary information about their manager; one group receives a signal about the salary of their manager but not peers; one group receives information about both their peers’ and manager’s salary; and one group receives no salary information. To avoid individuals 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 will be randomly chosen to receive some information about the average salary of peers/manager. In the following screen, we let the subjects know whether they have been chosen to receive the information or not.

We did not inform subjects whether they got any of the specific questions right or wrong, to avoid subjects learning information from their rewards. Furthermore, the rewards are paid jointly with participation fees, which includes a substantial portion that is randomly determined, so that the total payment received is a weak signal of the overall accuracy of the responses during the survey.
Step 4 (Elicit Posterior Belief): We let the subjects know that we want to give them the opportunity to reassess their answer to one of the previous questions. To avoid subjects making inferences based on the opportunity to re-elicit their guesses, we explicitly note that this opportunity was given automatically to all survey participants, regardless of their responses.

There are a few additional details that are worth discussing. For the average peer salary, we ask subjects to guess the average salary among individuals in the same position and unit. To minimize ambiguity, we state the full position title, the full name of the unit, and we state the number of employees currently working in the peer group.

When eliciting beliefs about manager salary, we elicit beliefs about the average salary of all employees who have a certain managerial position, which we state in full. To choose the managerial position to ask about, we used administrative data to identify a set of managers: i.e., other employees in the same unit who have power over the respondent such as approving performance evaluations, leaves of absence and internal transfers. According to this definition, one employee can have multiple managers, in which case we pick one position from this set.

The distance between the respondent and the manager can be important to understand the mechanisms at play. For this reason, before eliciting the perceived manager salary, we included two questions to assess the perceived distance: the number of promotions needed to reach that position, and the likelihood of reaching that position in the next five years. To give a sense of the average distance, the average respondents perceived their manager to be 2.X promotions ahead and expected to reach that position with X% probability.

3.3 Survey Outcomes

The main focus of the paper consists on measuring the effects of salary perceptions on behavior. Additionally, included a few survey questions at the end of the survey to be used as survey outcomes. We are not interested in the effects of salary perceptions on survey outcomes per se, but as complementary evidence on the mechanisms driving the behavioral effects.

The first three questions are intended to provide suggestive evidence about the social concerns mechanisms. First, we wanted to measure the effects of salary perceptions on employee morale. We follow a literature that uses self-reported employee satisfaction to proxy for employee morale (Clark and Oswald, 1996). We included one question about pay satisfaction: “How satisfied are you with your current salary?” This question was asked in a 5-point scale from very dissatisfied (1) to very satisfied (5). Additionally, we included a question on overall job satisfaction: “Taking all the aspects of your job into account, how
satisfied are you with your current job?” This question is asked using the same 5-point scale.

Another form of social concerns has to do with social preferences. To assess the importance of this channel, we asked individuals whether they agree or disagree with the pay inequality across the firm. This attitude is measured by the question perceived salary inequality: “Across the thousands of [Bank Name] employees, salaries vary with the nature of work, education, experience, responsibilities, etc. What do you think of wage differentials in the company today?” The possible answers were (1) they are too small, they are adequate (2) and they are too large (3). This question follows the tradition of the literature on preferences for redistribution, which uses a similar question about attitudes towards inequality (Perez-Truglia, 2015).

We included two questions to assess the importance of the career concerns channel. According to this channel, the effects of salary perceptions operate by changing expectations about future salary. We measure those expectations directly, by asking individuals their expected future salary 1 year ahead as well as 5 years ahead. To incentivize this question, we told individuals that we would compare their guesses with our own predictions for these future salaries, and reward them based on how close their answers fall from our own predictions.

The last survey outcome is intended to test another form of career concerns. It is possible that individuals react to the information about salaries of others not because they learn about their relative pay but because they learn about their relative productivity. To test this channel, we included a question about perceived productivity rank. In each annual review, each employee is assigned to a productivity rating on a scale from D to A+. Thus, we elicit the individual’s perception about the share of employees who have been assigned to each grade during the last yearly review. We incentivized this question by rewarding individuals for accurate responses. With these perceived shares and the employee’s own grade, we can infer the employee’s self-perceived rank in the distribution of productivity rating.
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