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Multitasking Incentives and Biases in Subjective Performance Evaluation

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Subjective performance evaluation serves as a double-edged sword. While it can mitigate multitasking agency problems, it also opens the door to evaluators' biases, resulting in lower job satisfaction and a higher rate of worker quits. Using the personnel records of individual sales representatives in a major car sales company in Japan, we provide direct evidence for both sides of subjective performance evaluation: (1) the sensitivity of evaluations to sales performance declines with the marginal productivity of hard-to-measure tasks, and (2) measures of potential evaluation bias we construct are positively associated with worker quits, after correcting for possible endogeneity biases

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I. Introduction

An incentive contract that ties compensation to observable performance measures can provide strong incentives (Paarsch and Shearer 1999, 2000; Lazear 2000; Haley 2003; Bandiera, Barankay, and Rasul 2005, 2007). Prior works have provided strong evidence of the link between pay and performance. However, these studies were limited to occupations and workplaces where the tasks are clearly defined and worker productivity is easily measured in all key dimensions including quality. Researchers in the social sciences have long recognized that a contract that is solely based on observable performance measures often produces dysfunctional responses.[1] For example, if car sales representatives are incentivized

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[1] Some early works that compiled anecdotes of dysfunctional responses include Laurence and Hull (1969) and Kerr (1975).

only by commissions on profits, they may ignore other tasks such as mentoring junior sales representatives.

The nature of such problems was formally analyzed by Holmstrom and Milgrom (1991), who showed that when an agent performs multiple tasks, some of which are hard to measure, the incentivized agent allocates more effort to the measured tasks, and may neglect the unmeasured tasks. Following their work, these incentive problems are known as multitasking agency problems.[2] Another related problem is workers gaming the system when presented with short-term incentives by manipulating the performance measure itself (Healy 1985; Holthausen, Larcker, and Sloan 1995; Oyer 1998; Owan and Tsuru 2011; Larkin 2014).

Multitasking agency problems arise because some tasks are hard to measure. Thus, one way to mitigate these problems is to use subjective performance evaluations (Prendergast 1999). For example, even if the mentoring of junior sales representatives is hard to measure, supervisors may still be able to subjectively assess such tasks. A sizable theoretical literature has analyzed the use of subjective evaluation in an incentive contract. By their nature, subjective measures attempt to capture aspects of performance that are not verifiable by a third party. Thus, the earlier literature focused on the conditions under which non-verifiable measures can be incorporated in an incentive contract (Bull 1987; MacLeod and Malcomson 1989; Pearce and Stacchetti 1998). Since subjective evaluation includes inherently private information, more recent literature analyzed the consequences of incorporating performance measures that are based on private opinions in an incentive contract (Baker, Gibbons and Murphy 1994; Levin 2003; MacLeod 2003; Fuchs 2007; Chan and Zheng 2011).

In all theoretical studies, the underlying premise is that subjective evaluation is used to incorporate hard-to-measure tasks in an incentive contract. By hard-to-measure tasks, we mean tasks that are hard to quantify in a way that can be rewarded with formulaic

[2] Baker (2002) also analyzed a similar problem in which the effects of the agent's action on the performance measure differ from its effects on the firm value.

bonuses. A natural empirical question is whether subjectivity is indeed used for this purpose. However, only a few studies have examined this question. By using branch managers' compensation data from 150 auto dealerships, Gibbs et al. (2004) showed that dealerships that face greater multitasking agency problems or a greater threat of gaming behaviors are more likely to use discretionary bonuses. Using CEO compensation data, Bushman, Indjejikian, and Smith (1996) showed that growth opportunities and product development cycles, which are proxies for multitasking agency problems, are positively related to the use of individual performance evaluations.[3] Hayes and Schaefer (2000) showed that performance measures that are unobserved by third parties are indeed used in setting CEOs salaries.[4]

One prior work that did not confirm the beneficial role of subjective performance evaluation is Ittner, Larcker and Meyer (2003), which analyzed the balanced scorecards bonus plans in a US retail bank. They showed that the subjectivity in the scorecard plan allowed supervisors to ignore qualitative measures that were predictive of future financial performance and award bonuses primarily based on current financial performance.[5]

Although subjective evaluation is useful in providing multitasking incentives and preventing gaming, it inevitably opens the door to evaluators' biases, such as favoritism and discrimination, that cause inefficiencies. MacLeod (2003) argued that when a supervisor's assessment of a worker's performance deviates from the worker's own assessment, the worker's effort will be negatively affected. MacLeod termed this deviation the "perceived

[3] Murphy and Oyer (2003), however, did not find this evidence.

[4] Some works provide evidence that the link between pay and easily measured tasks becomes weak or less explicit when there are some concerns about multitasking agency or gaming problems, although they do not show direct evidence of the use of subjective measure on hard-to-measure tasks. For example, Hoppe and Moers (2011) found that firms with greater environmental uncertainty tend to write CEO contracts free of formulaic bonuses. Ederhof (2010) showed that subjectivity is more likely to be used when contractible outcomes are either high or low.

[5] The results do not necessarily imply that the bank's subjective performance assessments did not help to mitigate multitasking and gaming problems. Other factors considered in the performance evaluation system may be affecting the size of bonuses in non-linear way (e.g., only when an individual score falls short of a certain threshold).

bias." Prendergast and Topel (1993) noted that employees who feel discriminated against may quit, resulting in turnover costs (p.359).[6] Similarly, Levin (2003) demonstrated that the use of subjective measures inevitably causes conflicts due to differences in opinions which may motivate workers to quit.

Subjective performance evaluation is therefore a double-edged sword. It can mitigate multitasking agency problems and gaming, but it opens the door for bias, resulting in greater turnover. Thus, the purpose of this study is to provide evidence for both sides of subjective performance evaluation.

Our first goal is to provide new and straightforward evidence that subjectivity is indeed used to incorporate hard-to-measure tasks in an incentive contract. We use personnel and transaction records of new car sales representatives in a major car sales company in Japan, which we have given the pseudonym "Auto Japan." In Auto Japan, new car sales representatives work under commission, which alone provides a strong incentive to perform well. However, Auto Japan also conducts annual performance reviews in which the supervisors subjectively rate workers' performance on a 5-grade scale. The evaluation results are then used to determine annual salary raises. According to our interviews with several branch managers, the reason they conduct the performance reviews is to reward effort on tasks not captured by the sales figures. After several interviews, we identified two important hard-to-measure tasks: (1) mentoring junior workers and (2) building long-term customer relationships. We provide evidence that subjectivity is indeed used to reward these hard-to-measure tasks.[7]

[6] Prendergast and Topel (1993), Levin (2003), and MacLeod (2003) also predicted another type of bias in subjective evaluations, supervisors not sufficiently distinguishing between workers, known as centrality bias. Empirical evidence of this type of bias can be found in Murphy and Cleveland (1991) and Larkey and Caulkins (1992).

[7] It is worth mentioning that this analysis relates to a growing literature that investigates how workers change effort allocation in response to changes in compensation plans. Drago and Garvey (1998) showed that when promotion incentives are strong, individual effort increases while helping effort decreases. Using physicians' compensation data in Canada, Dumont et al. (2008) showed that the move away from a fee-for-service compensation plan, where remuneration is tied to the quantity of care, towards a flatter compensation plan increased the time physicians spent on each patient, as well as the time they allocated

Our empirical strategy is as follows: consider a supervisor in a car dealership who needs to motivate sales representatives not only to sell more, but also to do a hard-to-measure task, either mentoring or customer relationship management activities in our analysis. If the supervisor incorporates the sales representatives' mentoring performance in their evaluations, the weight placed on sales would decline while the weight on mentoring increases, leading to a decrease in evaluations' sensitivity to sales performance. We use this prediction for the test of multitasking incentive provisions. We present two existing models to justify our prediction.

The second goal of this study is to examine the effects of evaluation bias on worker quits. Some studies have pointed to the existence of biases in subjective evaluation (Goldin and Rouse 2000; Elvira and Town 2001). However, the effects of biases on worker quits are largely untested.[8] The key issue is how to construct a measure of bias. We take two approaches. The first approach is based on the idea that if evaluation bias exists, it should appear in the residuals of the evaluation regressions. Thus, we construct an indicator of an "unexplained evaluation gap," a negative residual large enough to lower the evaluation grade by one level (e.g., a B grade when an A is expected). We test if a negative evaluation gap is associated with an increase in the probability of worker quits.

One problem with this approach is that a negative residual could include unobserved worker characteristics that are unrelated to the evaluation bias. This issue is especially problematic when such unobservable worker characteristics are correlated with the probability of worker quits. In order to filter out the effect of these unobservables, we use a 2SLS

to teaching and administrative work. Using a personnel data set from a multinational law firm, Bartel, Cardi, and Shaw (2012) showed that the firm's move from individual incentives towards leadership incentives reduced team leaders' billable hours and increased their non-billable hours. While these studies focused on how workers' effort allocation might change in response to a change in weights among competing tasks, our study focuses on how supervisors might alter these weights given the presence of multitasking problems.

[8] Giuliano, Levine, and Leonard (2005) found that having supervisors of races different from that of their subordinates increases the probability of quits for black and Hispanic workers. This result is suggestive of the existence of bias, but not necessarily the result of biased subjective evaluation. Engellandt and Riphahn (2011) evaluated the effects of favoritism-tainted evaluation on worker productivity, but they did not test the effects of biased evaluations on worker quits.

estimator.

Our second approach also deals with this problem. We estimate the evaluation regressions including supervisor-worker match fixed effects. Once the match fixed effects are identified, we compute within-worker change in the match fixed effects for those who experienced a change in supervisor (i.e. the difference in evaluations given to a worker by the initial supervisors and successive supervisors.). Although this change in the match fixed effect is a noisy signal of evaluation bias, our estimators are unlikely to be biased, because this bias measure does not include time-invariant unobservable worker characteristics. Both approaches produced similar results. When there is a negative deviation from the predicted value of evaluation or a drop in the supervisor-worker match fixed effect, the worker is more likely to quit even after controlling for the evaluation itself.

A strong point of our study is that we can assess the validity of our bias measure by using additional information from a survey of workers we conducted about how fair they believed evaluation results were. The survey included workers' identification numbers, enabling us to merge its results with the evaluation and sales data. If the negative evaluation gap simply captures worker characteristics that are unobservable to the researchers, but are observable to both parties, or if it is persuasively revealed to the worker via effective feedback, it should not affect workers' opinions on the evaluations' fairness. For example, if it simply captures a worker's insufficient ability that is observed by both the supervisor and the worker, the worker would not perceive it as unfair. We will demonstrate that a negative evaluation gap indeed increases the likelihood that workers believe their evaluations were unfair.

The rest of the paper is structured as follows: Section II presents two standard models used to derive our empirical prediction. Section III outlines Auto Japan's performance evaluation systems and possible sources of multitasking agency problems. Section IV describes the data and Section V discusses the results and their implications. Finally, Section VI concludes with a summary of our results and remaining issues.

II. Theoretical Background

We motivate our empirical strategy by first using the standard results from a simple multitasking principal-agent model, following Baker (2002), where the agent performs two tasks, and second by discussing a principal-agent model developed by MacLeod (2003), where the supervisors' assessments and the workers' self-assessments may not be perfectly correlated.

II.A Multitasking Agency Model

If the marginal productivity of one task increases, the principal should induce the agent to reallocate effort towards that task. As long as these two tasks are substitutes in the agent's effort cost function, the principal should increase the weight for that task, and reduce the weight for the other task. We use these implications to develop a testable prediction.

To be more precise, we first provide the model used by Baker (2002) below. For simplicity, we assume that the performance of both tasks is contractible. However, contract theory literature generally finds that when a performance measure is observable to both the principal and the agent (or their beliefs are perfectly correlated), they can achieve the same outcome that would be obtained under a verifiable performance measure (Bull 1987; MacLeod and Malcomson 1989; Pearce and Stacchetti 1998; MacLeod 2003). In short, the verifiability assumption is not actually required. Loosening the assumption is critical here because performance is unlikely to be verifiable on either hard-to-measure task we discuss in our empirical section.

Let t_s and t_m be the effort spent on sales and mentoring tasks, respectively. Let W be the wage for the agent. The value of the firm is then given by: $V{=}B_1t_s{+}B_2t_m{+}\varepsilon\text{-}W$ where B_1 is the marginal productivity of sales effort and B_2 is the marginal productivity of mentoring effort. The term ε captures the environmental uncertainty the firm faces with mean zero.

The supervisor evaluates both tasks, which are given as: $p_s = t_s + \eta_s$ and $p_m = t_m + \eta_m$. The terms p_s and p_m are the measures of sales and mentoring effort, respectively. The terms η_s and η_m are the random errors in measuring performance; these are assumed to be normally distributed with variances σ_s and σ_m. For simplicity, we assume that these errors are independent.

Consider the linear compensation scheme $W = \beta_0 + \beta_1 p_s + \beta_2 p_m$. Assuming that the agent's cost of effort function is C(t_s,t_m)=$at_s^2 + bt_m^2 + ct_s t_m$, and that the agent has the exponential utility function U=-exp(-r(W-C)), the optimal weights are given by:

$$\begin{aligned} \beta_1^* &= \frac{(1 + rb\sigma_m^2)B_1 - rc\sigma_m^2 B_2}{1 + rb\sigma_m^2 + ra\sigma_s^2 + r^2(ab - c)\sigma_s^2\sigma_m^2} \\ \beta_2^* &= \frac{(1 + ra\sigma_s^2)B_2 - rc\sigma_s^2 B_1}{1 + rb\sigma_m^2 + ra\sigma_s^2 + r^2(ab - c)\sigma_s^2\sigma_m^2} \end{aligned}$$

It is natural to assume that sales effort is more tiring if one is also mentoring junior workers. Therefore, we assume that sales and mentoring are substitutes in the agent's cost function so that $c > 0$. The denominators in the formula above are positive due to the second order condition. Thus, the above equations have the following two implications:

Implication 1: β_1^* (the weight for the sales effort) is a decreasing function of B_2 (the marginal productivity of the mentoring effort).

Implication 2: β_2^* (the weight for the mentoring effort) is an increasing function of B_2 (the marginal productivity of the mentoring effort)

Regarding Implication 1, Holmstrom and Milgrom (1991) demonstrated that even if the principal does not measure the hard-to-measure task at all, the weight for the sales task is still a decreasing function of B_2 in order to reduce effort misallocation.[9] Showing that β_1^* is a decreasing function of B_2, thus, does not necessarily indicate that subjective

[9] This is the case when σ_m goes to infinity in the above model (in such a case, β_2^* will be set equal to zero).

evaluation is actually used to measure the performance in hard-to-measure tasks. Such a result, however, is clear evidence that the firm is adjusting pay to deal with multitasking agency problems.

Because we cannot directly measure β_1^* and β_2^*, we need to press on a little further to obtain a testable prediction. Note the following derivations:

$$\frac{\partial W}{\partial B_2}|_{p_s=\widetilde{p}} \quad = \quad \frac{\partial \beta_0^*}{\partial B_2} + \frac{\partial \beta_1^*}{\partial B_2}\widetilde{p} + \frac{\partial \beta_2^*}{\partial B_2}p_m^* + \beta_2^* \frac{\partial p_m^*}{\partial B_2}$$

Now, by plugging in the optimal mentoring effort, t_m^*, in p_m^* as $p_m^* = t_m^* + \eta_m$, we obtain:

$$\frac{\partial W}{\partial B_2}|_{p_s=\widetilde{p}} = (\frac{\partial \beta_1^*}{\partial B_2} - \frac{c}{2b}\frac{\partial \beta_2^*}{\partial B_2})\widetilde{p} + \frac{\partial \beta_0^*}{\partial B_2} + \frac{\beta_2^*}{b}\frac{\partial \beta_2^*}{\partial B_2} + \widetilde{\varepsilon}$$

where $\widetilde{\varepsilon} = \frac{\partial \beta_2^*}{\partial B_2}(\frac{c}{2b}\eta_s + \eta_m)$. Note that $\frac{\partial^2 W}{\partial B_2 \partial p_s} = \frac{\partial \beta_1^*}{\partial B_2} - \frac{c}{2b}\frac{\partial \beta_2^*}{\partial B_2} < 0$. Therefore, pay sensitivity to sales performance declines with B_2 because of two reasons. First, the optimal weight on sales performance declines. Second, the optimal weight for mentoring effort increases. We can therefore make the following prediction.

Testable Prediction: The sensitivity of wage to sales performance declines with the marginal productivity of other hard-to-measure activities. Namely, $\frac{\partial^2 W}{\partial B_2 \partial p_s} < 0$.

II.B Model of Subjective Evaluation

One problem with the standard multitasking model is the strong assumption that the supervisor and the worker observe the same performance information or come to hold the same assessment of the worker's performance. In order to extend our prediction to cases where the supervisor's assessment of the worker's performance is not perfectly correlated with the worker's self-evaluation, we refer to MacLeod (2003) that considers a static principal-agent model where the agent's efforts affect the probability that a benefit has been realized. The principal does not, however, verify whether the benefit has been realized. The principal, instead, observes a signal of performance, $s_p \in \{1, ..., n\}$, which satisfies the standard monotone likelihood ratio condition; (i.e. $\Pr(Success|s_p)/\Pr(Failure|s_p)$ is increasing in s_p). The risk-averse agent judges his own performance and observes a signal: $s_a \in \{1, ..., n\}$.

By using the revelation principal, MacLeod first shows that, in order to induce a positive worker effort, the budget balancing condition cannot be satisfied so that the principal's payment (W) exceeds the agent's consumption (C) for some (s_p, s_a) with $s_p \neq s_a$. One interpretation of the difference, $W - C$, is that it represents a loss due to conflicts in which the agent engages in mutually unproductive behavior such as sabotage or strikes. This result demonstrates that implementation of an incentive based on subjective evaluation is not possible unless both sides are capable of punishing the other ex post. MacLeod (2003) also shows that if the principal's and agent's signals are perfectly correlated, the optimal contract with subjective evaluation is the same as the optimal principal-agent contract with verifiable information.

When the signals are not perfectly correlated, partial pooling appears in the equilibrium. Assuming a particular signaling structure, MacLeod (2003) shows that when the correlation of the signals is not perfect, agents whose performance is above a certain threshold receive the same pay (i.e., pooling takes place at the top of the distribution); and when the correlation of the signals is sufficiently low, agents receive the same pay except when the worst signal is observed by the principal.[10]

MacLeod's results offer useful implications for our study when we interpret the multitasking problem as a primary source of imperfections in the correlation between the supervisor's assessment and the worker's self-assessment. On the one hand, when the worker has a single task and the performance of the task is precisely measured, both assessments are likely to be perfectly correlated. For example, if the salesperson's sole responsibility is selling cars and if sales performance is perfectly measured by the number of vehicles sold

[10] Since the monotonic relationship is not proved in the paper, the statement is correct as a limit property.

and gross profits earned, there is little room for disagreement about the worker's performance. On the other hand, when the worker has other duties that are hard to measure, the supervisor's and the worker's assessments are likely to diverge.

Thus, when the multitasking problem is severe (i.e., B_2 is large), the correlation between the supervisor's assessment and the worker's self-assessment is generally weak, and therefore, Macleod's results imply that the supervisors would not distinguish the performance of those exceeding a given threshold, which in turn leads to the weakening of pay-to-sales-performance sensitivity. Thus, the testable prediction is $\frac{\partial W}{\partial p_s \partial B_2} < 0$. This is the same prediction as derived from the previous standard multitasking agency model. We now turn to our empirical analysis.

III. Performance Evaluation System, Practices, and Empirical Strategy

III.A Auto Japan's Performance Evaluation System

The sales force at Auto Japan has four vertical ranks called "bands", depicted in Figure 1. They are, from the highest to the lowest, general managers, branch managers, supervisors, and sales representatives. Further, workers in each band fall into one of five "salary stages"– S, A, B, C, and D.[11] A salary stage simply indicates the pay range of a worker's base salary. A salary stage is given to each worker automatically (see Figure 1) after the worker's base salary is updated. For example, in the sales representative band, if the base monthly salary is above 200 thousand yen, the sales representative is in salary stage S.

The annual raise to the base salary is determined by the annual performance evaluation conducted at the end of each fiscal year, and each worker is given an evaluation grade out of five letter grades, s, a, b, c, and d.[12] Table 1 exhibits the evaluation sheet. Column

[11] S stands for superior performance that exceeded expectations.

[12] Auto Japan actually uses the capital letters S, A, B, C, and D as evaluation grades. In order to distinguish them from salary grades, we denote them using lowercase letters.

1 shows the items evaluated. Except for interview results conducted by supervisors, all items are verifiable information.[13] At the beginning of the year, sales representatives set goals in both the quantity and quality dimensions of their work. At the end of the year, the supervisor interviews each sales representative individually to discuss the degree to which his goals were achieved.[14] These interviews are scored based on the workers' level of goal attainment, taking into account the difficulty of achieving those goals. Importantly, supervisors' subjective judgments carry substantial weight because the interview scores count for 140 out of a possible 400 total points in the evaluation grading scale.

The supervisor gives a score to each item (column 3), then, the total score is calculated (column 4). The supervisor can further adjust the total score in order to reward or punish some dimensions of work that are not captured by these items (column 5), adding further room for supervisors to exercise their subjective judgment in the evaluation. The final numerical score (column 6 = column 4+column 5) automatically determines the evaluation letter grade (column 7) according to the score conversion tables set by Auto Japan (not shown here). The conversion tables are distributed to the sales representatives.

Auto Japan set a "wage raise matrix", shown in Table 2, to determine the wage raises for every worker in accordance with their evaluation grades and salary stages. Formally, we can express this relationship using the equation $\Delta W_{t+1} = f(Evaluation_t, Salary\_stage_t) = \widetilde{f}(Evaluation_t, W_t)$ where the salary stage variable is replaced by the current wage because the former is automatically determined by the latter. As shown in Table 2, a worker needs to obtain an evaluation equivalent to his salary stage or above to receive a pay raise. An evaluation that is lower than one's salary stage leads to a wage cut. In other words, any deviation of workers' actual evaluations from their salary grade has a strong implication for

[13] Note that "the number of days it took to collect money from the customers" and "the percentage of former customers who brought their cars in for mandated car inspections" are included to provide incentives to attract customers who are financially sound and to enhance customer satisfaction.

[14] We use the masculine pronouns "his" and "him" throughout the paper because there were only several female managers and representatives in our samples.

their wages in the following year.

Importantly, according to Auto Japan's stated evaluation procedure, the evaluation is supposed to be *absolute* rather than *relative*, and all sales representatives are held to the same standards and not differentiated by salary stage. This is to say that equal performers are supposed to receive equal evaluations, regardless of their salary stage. Despite this stated policy, supervisors may use salary stages as "reference grades", then determine how much to deviate from the reference grades, assuming that they incur the psychic cost of giving their subordinate a grade that induces a wage cut.

III.B Multitasking Agency Problem

This section outlines possible sources of multitasking agency problem in Auto Japan. The main purpose is to identify proxies for the marginal productivity of hard-to-measure tasks. Auto Japan is an auto dealership and, unlike other white-collar jobs, sales representatives have readily available, objective performance measures–the gross profit earned by each representative.[15] Why, then, does Auto Japan not incentivize sales representatives solely with commissions? Figure 2 illustrates other tasks that might need to be taken into account in providing incentives. First, the management may care more about the quality of sales activities than salespeople do because of obvious externalities–for example, good customer care by individual sales representatives helps build the reputation of the company.

The other two tasks illustrated in Figure 2 are what we learned from interviews with several branch managers and one executive. They said that subjective performance evaluation is needed because sales figures miss some important tasks performed by the sales representatives: mentoring junior sales representatives and building and sustaining long-term customer relationships.

[15] Profit is a better measure of performance than the number of vehicles sold because the profit margin differs across car models, and sales representatives are encouraged to increase profits by selling car accessories and insurance.

Mentoring junior sales representatives is the most frequently mentioned reason for the use of annual performance evaluations. If sales representatives are incentivized solely based on the profits they earn, mentoring tasks will be neglected. The annual performance evaluation is used to reward mentoring. The following excerpt from our interview with a branch manager effectively illustrates this:

"Suppose that there are sales representatives A and B. Representative A sells 10 cars per month, and at the same time takes good care of junior representatives, sitting beside them and helping them negotiate deals and giving advice in the various phases of sales activities. Representative B also sells 10 cars per month but he does things only for himself. Representative A will definitely receive a much higher evaluation at the performance review. Some of these junior representatives will eventually become supervisors themselves and educating them is absolutely necessary."[16]

As a proxy for the marginal productivity of mentoring efforts, we use the ratio of junior representatives to experienced representatives in a sales group because we do not have information about exactly who provided mentoring. Seventy-four Auto Japan branches have new car sales departments, each of which typically has one or two sales groups with its own supervisor. We define a junior sales representative as a representative in his first year at Auto Japan, since a new entrant is likely to receive the most extensive mentoring. We define an experienced representative as a representative who is in the third year of tenure or greater.[17] This definition is derived from our interview with an executive who stated that it takes approximately three years for a worker to be able to perform all sales tasks competently. Thus, we construct this variable as:

$$Junior\ to\ experienced\ rep\ ratio_{it}$$

[16] According to our interview with a branch manager in May 2006.

[17] This definition assumes that the second year representatives neither receive nor provide mentoring.

$$= \begin{cases} \dfrac{\#junior\ sales\ reps.\ in\ the\ sales\ group_{it}}{\#\ of\ experienced\ sales\ reps.\ in\ the\ sales\ group_{it}}, & \text{for experienced sales reps} \\ 0, \ \text{for those not classified as `experienced reps'} \end{cases} \tag{1}$$

We interpret this variable as the average number of junior sales representatives that each experienced representative has to mentor in the sales group, or the probability of being assigned to a junior representative as a mentor. We set this variable to be zero for those who are not classified as 'experienced'. As Table 3 shows, the average *Junior to experienced rep ratio* is 0.046. Note that our proxy variable for mentoring is constant for all experienced representatives within a sale group.

The need to build long-term customer relationships is another frequently cited reason why annual performance evaluations are needed in addition to commission payments. If sales representatives maintain good relationships with their former customers, these customers are more likely to bring their cars to Auto Japan's maintenance department and more likely to purchase another car from Auto Japan in the future. For this reason, Auto Japan encourages sales representatives to periodically visit or call their customers who have purchased cars from Auto Japan. Because the maintenance departments' profits do not directly benefit the salespersons, the externality is not internalized. In addition, given that sales representatives may be too myopic to foster customer loyalty that may not pay off for several years, it makes sense to provide additional incentives through subjective evaluation.

In order to test the multitasking incentive provisions, we need *variation* in the importance of long-term customer relationships. We argue that the efforts to build long-term relationships have a higher marginal productivity for corporate customers than for individual customers since corporate customers buy multiple vehicles and thus replacement demand and car inspection demand arise more frequently. In addition, sales to corporate customers are more likely to be the results of teamwork and thus free-riding may be of some concern–another reason why additional incentives via subjective evaluation is important for corporate customer sales.

Therefore, we use the share of total gross profit generated by corporate customers for each sales group to measure the importance of building long-term customer relationships. Our transaction data contain basic information about each transaction including the name of salesperson who sold the car, the name of the customer, and gross profit earned from the sale. From the customer name, we can roughly distinguish corporate customers from individual ones. Thus, we construct the following variable:

$$\begin{aligned} & (Corporate\ customers\ share) \\ = \ & \frac{(Annual\ profit\ from\ corporate\ customers\ in\ each\ sales\ group)}{(Total\ annual\ profit\ earned\ in\ each\ sales\ group)} \end{aligned} \tag{2}$$

We expect that the efforts to build long-term customer relationships are more important if the share of corporate sales in the total profit is greater. Thus, we use this variable as a proxy for the marginal productivity of the customer relationship management task.

III.C Empirical Strategy

The theoretical implication we need to test is whether the sensitivity of wages to sales performance declines with these proxies. Given the fact that all aspects of worker performance in period t affect wages in period $t+1$ only through the period-t evaluation grade in accordance with the company's wage raise matrix described earlier, it is more natural to examine the sensitivity of evaluation to sales performance instead. Therefore, testing our prediction is equivalent to estimating the following equation:

$$\begin{aligned} Evalation^*_{it} &= \alpha_1 (Profit)_{it} + \alpha_2 (Profit)_{it} (Junior\ to\ experienced\ rep\ ratio)_{it} \\ &+ \alpha_3 (Junior\ to\ experienced\ rep\ ratio)_{it} \\ &+ \alpha_4 (Profit)_{it} (Corporate\ customers\ share)_{it} \qquad (3) \\ &+ \alpha_5 (Corporate\ customers\ share)_{it} + X_{it}\beta + (Branch\ fixed\ effects) + u_{it} \end{aligned}$$

where $Evaluation^{*}_{it}$ is the latent variable for the actual evaluation that takes value from 1, 2, 3, 4, 5. This is a conversion from the evaluation letter grade where the highest grade of S corresponds to 5, and the lowest grade of D corresponds to 1. $Profit_{it}$ is the annual gross profit earned by the i^{th} worker in the year t. X_{it} is a vector of other determinants of an evaluation–characteristics of the worker and the supervisor. As will be detailed later, we include branch fixed effect to control for the potential endogeneity of our marginal productivity measures. The coefficients of interest are α_2 and α_4. If our prediction holds, both α_2 and α_4 should be negative.

IV. Data, Variables, and Descriptive Statistics

IV.A Data Set

We use three data sets we obtained from Auto Japan. The first data set contains detailed information about car sales made during fiscal years 1999 to 2004[18], such as the gross profit obtained from each sale and customer names. The second data set contains employee information including the year salespeople were hired and which branches and sales group they worked at particular points in time during our sample period. In addition, we are able to identify the supervisor of each sales group in each branch at particular points in time. The third data set contains the annual evaluation results for each sales representative. We merged these three data sets using the worker identification numbers. As we obtained the performance evaluation data only for the period 2000 to 2003, our analysis is restricted to these four years.

Observations for sales representatives who left the firm before the end of a fiscal year were dropped from the sample for that year because these workers are not evaluated. We measure each worker's tenure at the end of each fiscal year. If workers entered the firm in

[18] Fiscal years at Auto Japan start in April and end in March. We obtained data from December 1998 to December 2005, so the complete fiscal years in this period are 1999 to 2004.

the middle of a fiscal year, their initial year's observations were dropped if they had worked less than nine months at the time of evaluation because the supervisor was unlikely to have sufficient information to judge their productivity.[19] In addition, sales representatives over age 60 are also excluded from our analysis, as they were former employees rehired as fixed-term contract workers after reaching the mandatory retirement age of 60, and their performance is not evaluated.

Auto Japan also has a sales department specializing in fleet sales to large corporations and its own affiliated rental car company. The sales representatives in this department are excluded from our dataset because the sales activities, profit margins, and incentive schemes in the department are very different from those of other sales departments.[20]

The above sample selection criteria resulted in an unbalanced panel of 686 new car sales representatives in 74 different branches, in 120 sales groups, over the period 2000-2003, resulting in a total of 2148 representative-year observations.

IV.B Variables and Summary Statistics

Table 3 shows the descriptive statistics of the variables that we use for our estimation. The average of *Evaluation* is about 2.7 (between b and c). Ninety-three percent of the observations received either a, b, or c. The variable *Profit* is the annual gross profit each worker earned, in millions of yen, during the fiscal year–the average is 20.71 million yen (approximately US$200,000).[21] Most of the variables are self-explanatory, though some variables are motivated by existing theories of subjective evaluation and worker behavior and thus deserve some explanation.

[19] This eliminated 33 observations.

[20] In many cases, the deals are negotiated directly between the clients and the car manufacturer. Sales to the affiliated rental car company, for example, do not generate any profits. For this reason, workers in this department do not receive commissions on profits from sales.

[21] There are 157,897 transactions recorded in our data for the new car sales representatives. The majority of these transactions are actual sales (90%). The rest of the transactions were lease contracts. Our variable, *Profit*, includes both sales and lease contracts.

Merchant (1989) and Gibbs et al. (2004) emphasized that subjectivity is used to filter out uncontrollable risks. For example, if a supervisor judges that the sales at his branch are low due to factors beyond the workers' control, such as the relocation of a branch to a temporary site, he or she may inflate their evaluations so as not to punish workers for their bad luck. Uncontrollable risks could be either systemic–affecting all branches in the same way–or idiosyncratic, limited to a single branch. Note that the managers can evaluate idiosyncratic risks more easily than systemic ones by comparing branch performance with the firm-wide average.

We attempt to capture idiosyncratic, uncontrollable risks at the branch level using the variable *Branch-firm productivity differences*, defined as branch productivity minus the firm-wide productivity. Branch productivity is the profit per salesperson at each branch. Firm-wide productivity is defined similarly.[22] Gibbs et al. (2004) emphasized that supervisors may adjust evaluations only in response to negative shocks because filtering out positive shocks would cause the workers to cut back their sales efforts in good years (ratchet effect). To capture this asymmetry, we split the productivity differences into positive and negative parts in the estimation.

We control for the total number of sales representatives in a branch for the following reason. Within a branch, each sales representative has his given territory. When sale people leave their branches, their territories are reassigned within that branch. Therefore, a reduction in the total number of representatives typically leads to an increase in profit per representative. However, this rise in profit may not be perceived as better performance and the supervisor may reduce the weight on profit in evaluations. Inclusion of the total number of sales representatives at a branch alleviates this effect.[23]

[22] In the computation of profit per representative, first-year representatives are excluded so that branch turnovers do not affect the variable.

[23] Note that when a representative worked for a fraction of a fiscal year, the fraction is used for the computation of the number of representatives.

We also control for two additional branch characteristics: the average worker tenure, and the standard deviation of worker tenure at each branch. Most employees at Auto Japan joined the firm right after finishing school and the hiring of mid-career workers is rare. Therefore average worker tenure represents the average sales experience in each branch, whereas the standard deviation of worker tenure captures the degree of age diversity in a branch.

There are two reasons why we include the average worker tenure. First, the average worker tenure captures the average human capital. More experienced workers know more about how to develop and maintain good relationships with corporate customers and how to coordinate with other departments to increase cross-selling. The benefits of these activities may not be captured in short-term sales figures but may be highly evaluated in the subjective evaluation. To the extent to which these skills are shared, junior salespeople will benefit from the presence of experienced representatives in their branches. Second, higher average worker tenure means smaller average age differences between sales representatives and their branch managers, which may imply more effective cooperation and coordination within a branch.[24] Subjective evaluations may improve if the sales representatives respond well to the branch managers' directions.

Inclusion of the standard deviation of worker tenure is the result of our consideration of social identity theory or self-categorization theory in organizational behavior.[25] If small age diversity enhances group identity, supervisors may attempt to reinforce that identity by giving similar grades to every member of the branch, especially by giving lenient grades to low performers.

To provide some preliminary ideas about how evaluation is related to a worker's performance, Figure 3 plots *Evaluation* against *Profit.* We observe a clear positive relationship.

[24] See Zenger and Lawrence (1989) who found a negative effect of age diversity on communication

[25] See Mannix and Neale (2005) for related discussion

However, because we can see that the same profit often translates into different evaluations, we asked one executive why this occurs. He answered that supervisors evaluate not only profits but also other tasks, such as mentoring and customer follow-ups, and this is why the same profits often translate into different evaluations. As noted already, this is exactly what we test in this study.

V. Empirical Analyses

V.A Do managers adjust their evaluation for multitasking agents?

Our main goal is to determine if a worker's evaluation takes into account the performance of hard-to-measure tasks such as mentoring and building long-term customer relationships. We test our main prediction by estimating equation (3). We first estimate ordered probit models, the results of which are shown in Table 4.

Model 1 is the most parsimonious baseline model. The interactive terms between *Profit* and *Junior to experienced rep ratio*, and *Profit* and *Corporate customer share* are both negative and significant at the 5 percent level. Thus, the results are consistent with our prediction, which states that the sensitivity of wages to sales performance declines with the marginal productivity of other hard-to-measure tasks. Model 2 adds worker, supervisor and branch characteristics. The coefficients for both interactive terms remain negative and significant. Although the coefficient for the first interactive term is only weakly significant, the results are generally consistent with our prediction. The weak significance of the result for *Junior to experienced rep ratio* may come from the lack of information about who are the actual mentors of junior representatives.

As noted earlier, Auto Japan's stated evaluation procedure calls for "equal evaluation for equal performance regardless of one's salary stage". Hence, we did not include salary stage dummies in the first two specifications. However, as we also noted earlier, the wage raise matrix in Table 2 indicates that supervisors may use the worker's salary stage as a reference grade, and then determine how much to deviate from it. If this is the case, a person's evaluation might be higher than his true performance simply because his salary stage is higher. To account for this possibility, we add salary stage dummies in Model 3. As expected, the coefficients for stage dummies monotonically increase from C to S.

Our main prediction is not fully supported in this model specification. Although the coefficient for the interactive term between *Profit* and *Corporate customer share* is still significant at the 1 percent level, the coefficient for the interactive term between *Profit* and *Junior to experienced rep ratio* became insignificant. This does not necessarily mean that the evaluators do not take into account the increased role of mentoring in giving evaluation grades. If the chance of being asked to mentor is correlated with worker productivity (which is reflected in the salary stage), the coefficients of salary stage dummies should be overestimated while that of the interactive term between *Profit* and *Junior to experienced rep ratio should* be underestimated.[26]

One concern we have is that the number of junior workers in a branch is likely to be influenced by unobserved branch characteristics. In Auto Japan, once sales representatives are assigned to a branch, they will move to another branch only when promoted to a supervisor position. Thus, a job vacancy occurs only when a branch expands, a representative quits, or a representative is promoted. The frequency of these events is likely to be influenced by unobserved branch characteristics. Similarly, the corporate customer share is likely to be influenced by the demographics of the neighborhoods surrounding the branch.

[26] Another concern is model misspecification. In this analysis, we assume that the coefficients of current performance are constant, but it is quite possible that, as time goes by, the supervisor accumulates more information about each worker's ability and potential, and thus lower weights will be given to measures of current performance. Because ability is more fully revealed for workers in the higher salary stages due to their longer tenure, salary stages may capture the decreasing weight on current performance over tenure. We can test this hypothesis by including the interaction between *Profit* and *Worker's tenure*. If the hypothesis is correct, the inclusion of the cross term will reduce the effect of salary stages. We have conducted this test in an unreported regression, and found that: (i) the coefficient for the additional cross term is small and insignificant, and (ii) all other coefficients (including salary stage dummies) are essentially unaffected. Therefore, this employee learning hypothesis cannot explain the salary stage effects.

To control for the potential endogeneity caused by correlations between these unobserved characteristics and our multitasking proxies, we include branch dummies in Model 4. The inclusion of branch dummies did not alter the results qualitatively, but improved the significance of the interactive term between *Junior to experienced rep ratio* and *Profit* to the 5 percent level, although the statistical significance of the other interactive term dropped to the 10 percent level (pval=5.3 percent).

Unobservable supervisor characteristics such as managing and training ability may also be correlated with sales group turnover and promotion rates, which affects *Junior to experienced rep ratio.* Thus, we include supervisor dummies in Model 5. The coefficient for the interaction between *Profit* and *Junior to experienced rep ratio* became insignificant (pval=12 percent). However, the coefficient for the interaction between *Profit* and *Corporate customer share* is still significant at the 10 percent level. Relatively low significance of the interactive terms in Model 5, however, is likely to be due to smaller within-manager variations of our two focal variables.

Now, let us examine some other results. Did supervisors adjust for shocks that affected all salespeople in the branch and were beyond their control? Across all the models, the coefficients for the negative components of *Branch-firm productivity differences* are negative and significant. The coefficient for the positive part is also negative for all the models, but is insignificant for some models. Also note that the coefficient for the negative component is much larger in absolute value than that for the positive component, indicating that it is mainly the negative shocks that are filtered out in evaluation. Interestingly, this result is consistent with Gibbs et al. (2004), who predicted that uncontrollable risks would be filtered out only when they are negative, although our results do suggest that supervisors may adjust evaluation downward when there are positive shocks. The quadratic form of worker tenure has a significant coefficient across all the models, and offers a natural interpretation of a learning curve which reaches its peak at twenty-five years.

One possible criticism of our results so far is that a negative coefficient of an interactive term in non-linear regression does not necessarily imply substitutability of the two variables, which is defined by the cross-derivative of the expected value of *Evaluation* (see Ai and Norton 2003). To answer to this criticism, we re-estimate the same models using tobit regression.[27] Tobit regressions impose a linear relationship between *Evaluation* and the explanatory variables that may be restrictive, but as a result, the cross term coefficient is the same as the interaction effect of the two variables in *Evaluation*, as long as we confine our attention to the interior between the two endpoints. Thus, tobit models can provide a quick check of the directions of the interaction effects.

The tobit results in Table 5 provide stronger support for our prediction. The coefficient of the interactive term between *Junior to experienced rep ratio* and *Profit* is negative and significant at the 5 percent level even for Model 3 and at the 1 percent level for the rest. The strong statistical significance of the coefficient in Models 4 and 5 is especially notable. The coefficient for the interaction between *Corporate customer share* and *Profit* is negative and statistical significance improved for all the models except for Models 4 and 5. The insignificant results for Models 4 and 5 may simply suggest that the importance of the relationship with corporate customers does not change for each branch year by year as measured by *Corporate customer share.*

Overall, the hypothesis that supervisors take into account hard-to-measure tasks such as mentoring and long-term customer relationship building in their evaluations has been supported by the data.

[27] The lower limit is 1 and the upper limit is 5.

V.B Does the performance evaluation contain information about future sales performance?

Hard-to-measure tasks such as developing good relationships with corporate customers tend to have an impact on future sales performance, as posited pictorially in Figure 2. It is now useful to examine the claim that an evaluation does in fact contain information predicting future sales performance. If this claim is confirmed, the empirical evidence for the hypothesis that managers use subjective performance evaluation to mitigate the multitasking problem will be further reinforced.

Consider the following model that regresses the one-period-ahead Profit on the current *Evaluation* and the current *Profit*. X_{it} contains other variables that are observable to the third party.

$$\begin{aligned}(Profit)_{i,t+1} &= \beta_0 + \beta_1 (Profit)_{it} + \beta_2 (Evaluation)_{it} + \beta' X_{it} \\ &+ (Branch\ fixed\ effects) + u_{it}\end{aligned}$$

If the current evaluation is solely determined by observable performance measures and observable worker characteristics, it should not affect future performance once the current observables are all controlled for. However, if the evaluation contains information on expected future performance as well as observed performance, it should explain future sales performance. This idea parallels that of Hayes and Schaefer (2000), who tested whether the annual performance evaluations of CEOs contain information that predicts firms' future performance.

Table 6 shows the results. In the model, we include worker, supervisor and branch characteristics that may predict future profit. Some branch characteristics, such as location, affect sales, and these time-invariant characteristics should be controlled for. Therefore, we include branch fixed effects. The coefficient for *Evaluation* is positive and significant at the 5 percent level. This confirms that annual performance evaluations capture information that is not fully captured by current profits.

V.C How should we construct the measure of evaluation bias?

We now turn to the second goal of this study: understanding the effects of supervisor bias in subjective evaluations on worker quits. Let us first discuss the definition of bias in subjective evaluation. Prendergast and Topel (1996) modeled favoritism in terms of supervisors' altruism towards particular workers. In their model, the supervisor's utility depends on the subordinate's wage. Given that the supervisors' reports affect the subordinates' wages, the supervisors overstate the performance of their favorite subordinates and understate the performance of subordinates they dislike in their evaluations.

In MacLeod (2003), the difference between the supervisor's assessment of the worker's performance and the worker's own assessment is labeled as the worker's perceived bias. Unlike taste-based bias as modelled in Prendergast and Topel (1996), perceived bias arises even when both the supervisor and the worker impartially report their beliefs about the worker's performance because the disagreement comes from different priors or non-overlapping information they receive. Nevertheless, in either type of bias, conflict arises and the worker may quit the firm.

To estimate the effects of bias on worker quits, we use two approaches. The first approach is based on the idea that, if evaluation bias exists, it should appear in the residuals of the evaluation regressions. Thus, we construct an indicator of an "unexplained evaluation gap" in which a substantially positive or negative residual indicates favoritism or discrimination, respectively.

One problem with this approach is that the residuals of the evaluation regressions could contain unobserved individual characteristics that affect the worker's propensity to quit. These unobserved worker characteristics can lead to either overestimation or underestimation of the effects of evaluation bias on worker quits. For example, a worker who lacks commitment to work may receive a low evaluation due to his poor performance in hard-to-measure tasks and at the same time he is likely to quit his job for reasons unrelated to his evaluation, leading to an overestimation. Another example is that a worker with strong social skills may spend more time developing customer relationships that lead to a better evaluation. Such a worker would have greater employment opportunities elsewhere and therefore has a higher quit probability, leading to an underestimation.

Time varying unobservables can also cause an endogeneity problem. For example, a manager may stop assigning mentoring tasks to a worker because he does not sufficiently take care of junior representatives. Then, the worker may become happier because he can focus on sales activities and earn more commission, or he may become less happy if the manager gives him a lower evaluation grade than what his sales performance indicates. Thus, a change in task assignment may affect both the worker's evaluation and his decision to quit.

To eliminate the endogeneity bias caused by such unobservables, we use instrumental variables that are correlated with evaluation bias, but not correlated with such unobservables. They are the dummy variables indicating if the supervisor's education level is higher or lower than the worker's education. We find that, whatever the education levels of supervisors and workers may be, if the supervisor's education level is lower than that of the worker, the former tends to give lower evaluations than what is justified by the objective performance measures. However, they are unlikely to be correlated with personal traits or changes in task assignments.

The second approach is based on the idea that evaluation bias is supervisor-worker match specific, thus the supervisor-worker match fixed effects estimated in evaluation regressions can be treated as the measure of bias. One problem with this approach is that workers rarely transfer across branches at Auto Japan. Thus, we cannot estimate the supervisor-worker match fixed effects separately from branch fixed effects and worker fixed effects. The estimated match fixed effects will reflect both the branch and worker fixed effects.

To solve this problem, we examine the effects of the *change* in the supervisor-worker match fixed effect. Our assumption is that a drop in the match fixed effects would be perceived as unfair by the worker, leading to greater turnover. By taking the change in the estimated match fixed effect within the same worker, we effectively eliminate both the branch fixed effects and worker fixed effects.

For the first approach, we need to compute the residuals of the evaluation regression. Let us define the residuals of our ordered probit evaluation regressions as:

$$(Residual)_{it} = (Actual\ evaluation)_{it} - (Predicted\ evaluation)_{it}$$

where predicted evaluation is defined as $\sum_{k=1}^{5} Prob(Evaluation_{it} = k)k$.

Discrepancies between actual and predicted evaluations arise naturally due to the discrete nature of evaluation. However, if workers were to have access to the same information as researchers do and predict their evaluation in the same way, they would expect to receive the evaluation that is nearest to the predicted evaluation. If they receive evaluations that are not nearest to the predicted evaluations, they may feel either discriminated against (*residual*<-0.5), or favored (*residual*>+0.5). Therefore, we consider the following potential bias indicators:

Potential Bias Indicator:

$I\{\text{Residual}_{it}<-0.5\}$ = Potential discrimination indicator

$I\{\text{Residual}_{it}>+0.5\}$ = Potential favoritism indicator

where $I\{\}$ is the indicator variable. We use the Model 4 evaluation regression (in Table 4), which includes branch fixed effects, to compute residuals. Given that workers are rarely transferred to other branches, if their perception of fairness in evaluations is formed through past experience, branch-specific, time-invariant factors that are taken for granted should be filtered out in calculating bias. This is the reason why we use this model specification.

Figure 4 is the histogram of the Model 4 evaluation residuals. Ninety-nine percent of the observations are between –1 and +1. Thus, it is rare to receive evaluations that are "way off" the predicted evaluation. Our potential discrimination and favoritism indicators correspond to the 12^{th} percentile and the 88^{th} percentile of the distribution of the residuals, respectively.

Do these bias indicators predict the worker's dissatisfaction with their evaluation results? In Appendix A, we answer this question using additional information from a survey of workers we conducted about how fair they believed evaluation results were. If the negative evaluation gap simply captures worker characteristics that are unobservable to the researchers, but are observable to both parties, or if it is persuasively revealed to the worker via effective feedback, it should not necessarily affect workers' opinions on the evaluations' fairness. Thus, if the bias measure is significantly associated with the worker's perceived fairness of the evaluation results, our bias measure is consistent with either taste-based bias or Macleod's perceived bias interpretation.

In Table A.1 of the appendix, we show that a negative evaluation gap indeed reduces workers' perception of fairness about the evaluation results, and such effects arise mainly when the workers did not receive feedbacks from their supervisors. The impact of a negative evaluation gap on the reported perceived fairness rating (in percentage) is a 13 to 17 percentage point reduction on average, and a 30 percentage point reduction in the absence of feedback. These results will further strengthen our analysis of the effects of evaluation bias on worker quits in the next section.

For the second approach, given that we need to include a large number of supervisor-worker match dummy variables, we re-estimate the evaluation regression by OLS using the same control variables as Models 2, 4 and 5 of Table 4. The estimated supervisor-match fixed effects will capture any evaluation bias specific to the supervisor-worker pair. We compute the change in the supervisor-worker match fixed effects only for the workers who experienced a change in supervisors. Among the 686 sales representatives in our sample, 409 experienced at least one change in supervisor. If a worker experienced a change in supervisor more than once, the first supervisor was used as the reference to compute the change. The actual variable is constructed as:

$$\begin{aligned} & \Delta(Match\ fixed\ effects_{it}) \\ = \ & (Match\ fixed\ effect_{it}) - (Match\ fixed\ effect_{iT_i}) \end{aligned} \tag{4}$$

where T_i indicates the initial period in which the i^{th} individual appears in the sample. In the next subsection, we examine how these measures are related to the workers' probability to quit.

V.D Does evaluation bias cause the worker to quit?

First, we present the results of our first approach that uses the residual-based potential bias indicators. We estimate the following regressions:

$$Quit_{it} = \beta_1 I\{Residual_{it} < -0.5\} + \beta_2 I\{Residual_{it} > +0.5\} + \beta' Z_{it} + e_{it}$$

where $Quit_{it}$ is a dummy variable that takes the value 1 if the i^{th} worker quits at the end of financial year t, or in the middle of year t+1. In our sample, 75 workers left the company. Our data contain detailed reasons for each separation. Among these 75 workers, 49 took other jobs, 12 left to inherit their family businesses, 12 retired, and 2 were fired. We exclude retirements and firings from our definition of quits and therefore the total number of quits in the sample is 61.

Column 1 in Table 7 shows the estimation result for a simple probit model that does not account for worker fixed effects. It suggests that workers who received negatively biased evaluations are more likely to quit their jobs. Positively biased evaluations have no effect on worker quits. The results are robust to the change in model specification to a linear probability model with worker fixed effects (see Column 2), which corrects for the bias caused by time-invariant unobservables. According to Column 2 result, perceived discrimination, $I\{Residual_{it}<-0.5\}$, would increase the probability of quitting by 4.7 percentage points.

Column 3 further controls for evaluation to separate the effects of low evaluation from the effects of biases. The coefficients for the potential discrimination indicator dropped and became insignificant. This could imply that workers with substantial negative residuals have a higher propensity to quit not because of potential bias but because of the low evaluation itself. Interestingly, the potential favoritism indicator has a positive and statistically significant coefficient. This does not necessarily mean that favoritism increases worker quits. There may be unobserved tasks performed by workers that are correlated with quits. For example, a worker who takes a leadership role, such as motivating other workers, may receive higher evaluations. At the same time, he might have better outside opportunities due to his leadership ability.

As for the insignificant effect of potential negative bias, another interpretation is that, workers' dissatisfaction about their evaluations may have heterogeneous effects on worker turnover. For example, younger and less experienced workers may find it easier to move to other jobs than older and more experienced workers and therefore less experienced workers may be more sensitive to perceived discrimination. Thus, in Column 4, we include the interaction terms between worker tenure and the bias indicators. The coefficient for the perceived discrimination is positive and significant. The coefficient for the interaction between perceived discrimination and tenure is negative and significant. The estimated effects of perceived discrimination on quit probabilities at two, five, and ten years of experience are, 4.5, 3.8 and 2.6 percentage points respectively.

Finally, to correct for possible bias due to unobservables that are time varying, we estimate the two-stage least square model in Column 5. As explained earlier, the instruments are the dummy variables indicating if the supervisor's education level is higher or lower than the salesperson's level. Because we have only two instrumental variables, we keep only the negative evaluation gap in the model. As can be seen, the effect of a negative evaluation gap is positive and significant at the 5 percent level. Based on this result, the quit probability would increase by 40 percentage points when a worker faces a negative evaluation gap.

This effect may appear to be too large, given that the average annual separation rate of sales representatives in our sample is 2.8 percent. However, the first stage results, shown in Table 8, give a plausible interpretation for this large effect. When the supervisor's education level is lower than that of the worker's, the supervisor is more likely to give a low evaluation to that worker. When the supervisor is more educated than the worker, there is no effect. Thus, the 2SLS estimate of the effect of the negative evaluation gap can be interpreted as the local average treatment effect among the workers who experienced negative bias from supervisors whose education level was lower than their own. Evaluation biases that stem from such a situation may induce a more escalated response than would occur otherwise. Thus, our 2SLS estimate should be treated as the upper limit.

Now, we turn to our second approach to correct for the endogeneity bias. We begin by estimating the following quit regressions using the linear probability model and by including the change in the supervisor-worker match fixed effects we introduced in equation (4):

$$Quit_{it} = \beta\Delta(Match\ fixed\ effects_{it}) + \beta' Z_{it} + e_{it}$$

Table 9 Column 1 shows the estimation results with bootstrapped standard errors. The coefficient for $\Delta(Match\ fixed\ effects_{it})$ is negative and significant at the 1 percent level, indicating that a reduction in match fixed effect increases worker quits. Column 2 splits the variable into positive and negative parts to capture the possible non-linear effects. As

such, we find a significant effect when there is a reduction in the supervisor-worker match fixed effect, but we do not find a significant effect when there is an increase in the match fixed effect.

What are the magnitudes of these estimated effects? To be comparable with the residual-based analyses in Table 7, we first compute the average of Δ*Match fixed effect* for those who experienced changes that are large enough to downgrade their evaluation by one (i.e., a change that is lower than -0.5). The average is -0.78. Thus, Column 2 results indicate that if the match fixed effect drops by this much, the quit probability would increase on average by $-0.070\times-0.78\simeq$ 5.5 percentage points. This result is much smaller than the one estimated based on 2SLS in Table 7. However, this is still a large change in comparison to the annual separation rate of 2.8 percent in the original sample. In sum, we find evidence consistent with the hypothesis that negative evaluation bias increases worker quits.

VI. Discussion and Conclusion

Subjective performance evaluation cuts two ways in an incentive contract. It can mitigate multitasking problems, but it also opens the door for biases, resulting in higher turnover and lower job satisfaction. We have provided new evidence for both sides of subjective performance evaluation. Let us briefly summarize our new contributions to the related literature.

First, we showed that subjective evaluation is indeed used to incorporate hard-to-measure tasks in an incentive contract, namely mentoring junior workers and building long-term customer relationships. Specifically, by using proxies for the marginal productivity of these hard-to-measure tasks, we showed that the sensitivity of wages to sales performance declines with these proxies. One of our contributions to the literature is that we looked inside the "black box" and examined what hard-to-measure tasks are actually measured by subjective evaluations. In earlier studies with similar goals, what subjective evaluations are meant to assess is somewhat unclear. Thus, our results serve as useful complementary evidence for these studies. For example, Gibbs et al. (2004) showed that the amount of spending on personal training is positively related to the use of subjectively determined bonuses. This observation is consistent with our finding that mentoring tasks are rewarded in subjective evaluations. Bushman, Indjejikian, and Smith (1996) found that growth opportunity and the length of the product development cycle are positively related to the use of subjective individual performance evaluations in determining CEO compensation. This result is consistent with our finding that subjective evaluation is used to reward long-term, value enhancing activities.

Second, we provided the first empirical evidence that negative bias in subjective performance evaluation increases worker quits.[28] Throughout our analyses, our major concern was the possibility that unobserved worker characteristics might affect both our bias measures and the incidence of worker quits. In order to deal with this issue, we took two approaches. We used 2SLS estimators in our "residual analysis" to correct for the bias caused by unobserved worker characteristics on the estimated effect of the negative evaluation gap, our potential discrimination indicator. In addition, as a measure of potential bias, we used the within-worker changes in the supervisor-worker match fixed effects that eliminate any influence of time-invariant, unobservable worker characteristics. The results from both approaches imply that indicators of potential negative bias are significantly associated with an increase in worker quits even after controlling for the actual evaluation grades. We also showed that our measures of negative evaluation gaps are negatively associated with the workers' views on the fairness of their evaluation results.

[28] There is some anecdotal evidence for this relationship. Stewart (1993) describes a case in First Boston, where the firm announced that senior managers would receive reduced bonuses. Many senior managers claimed that they had been promised more while the company argued that the bonuses merely reflected disappointing financial results. The dispute ended with many managers leaving the firm. Endlich (1999) also describes a similar a case at Goldman Sachs.

There are, however, two additional important issues that we have been unable to study using our dataset. First, one important task for sales representatives is to “cross-sell” by soliciting repair and maintenance work for their branch’s service department, work that can be more profitable than new car sales. In some cases, it is better to divert the sales representatives’ efforts towards cross-selling, especially when the capacity utilization of the service department in their branch is low. Therefore, if we had detailed data on cross-selling, such as how many previous customers come back for repair and maintenance work, and data about service departments’ capacity utilization (e.g., revenue per mechanic), we could examine in detail whether a high level of cross-selling during a time of low capacity utilization is highly evaluated in subjective performance evaluations.

Second, subjective evaluation should reflect employer learning. Because sales performance is affected by factors beyond the control of sales representatives, supervisors may try to smooth their evaluation grades by taking the weighted average of current performance and the workers’ expected ability or productivity. The latter is actually the average of their past evaluation scores. In that case, the weight on current performance should be a decreasing function of how long the worker and supervisor have held their respective posts. Since we only have four years of data, we cannot satisfactorily evaluate this hypothesis. These issues are left for future research.

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Figure 1: Bands and Salary Stages

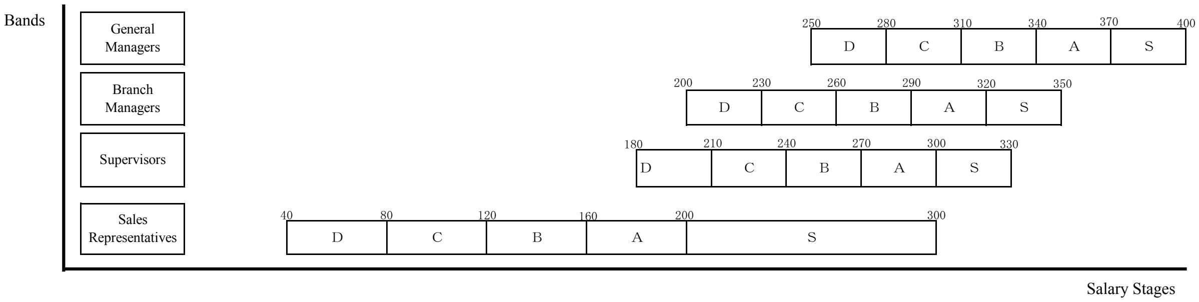
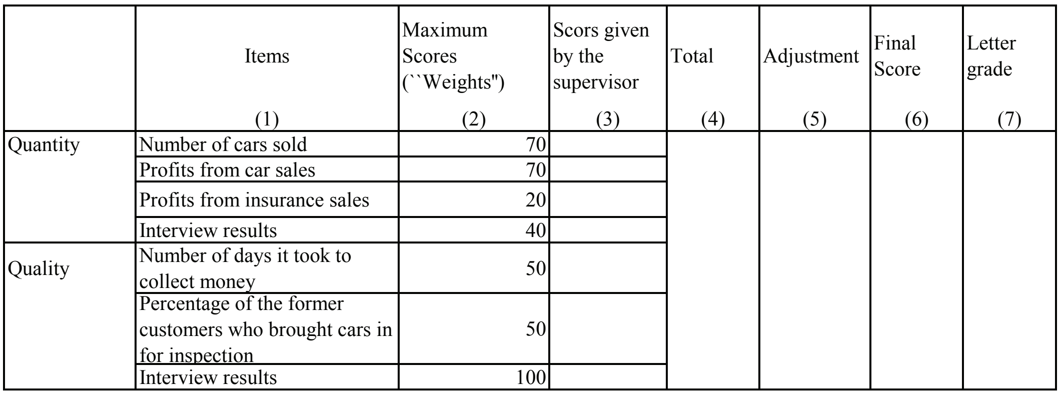


Table 1: Auto Japan's evaluation sheet



	Items (1)	Maximum Scores (``Weights") (2)	Scors given by the supervisor (3)	Total (4)	Adjustment (5)	Final Score (6)	Letter grade (7)
Quantity	Number of cars sold	70					
	Profits from car sales	70					
	Profits from insurance sales	20					
	Interview results	40					
Quality	Number of days it took to collect money	50					
	Percentage of the former customers who brought cars in for inspection	50					
	Interview results	100					

Table 2: Wage raise matrix (Raise in monthly base salary in 1000 yen)

		Evaluation results				
		s	a	b	c	d
Salary Stage	S	2.3	-2.2	-40	-50	-70
	A	5.6	2.1	-2.1	-40	-50
	B	40	5.2	2	-1.9	-40
	C	50	40	4.8	1.8	-1.8
	D	70	50	40	4.4	1.7

Figure 2: Hard to measure tasks

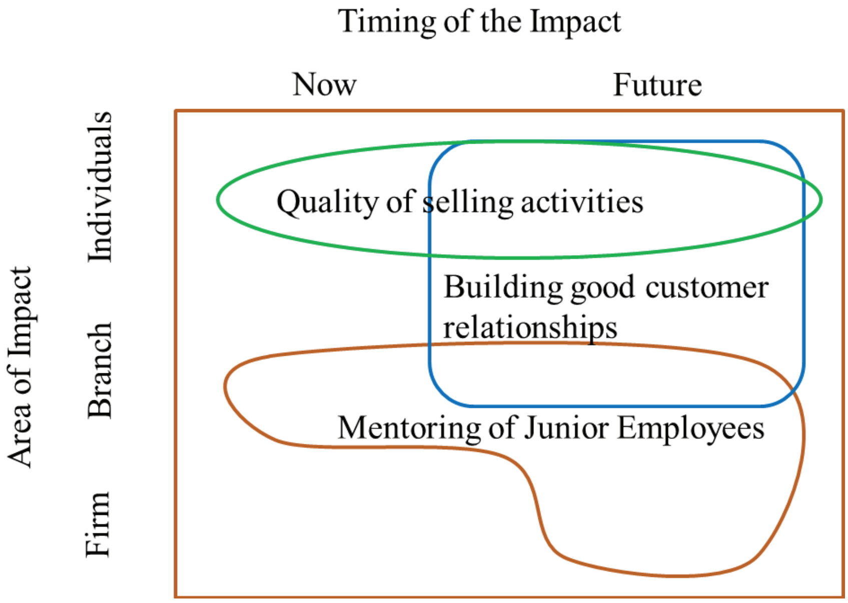

Timing of the Impact
Now
Future
Area of Impact
Individuals
Branch
Firm
Quality of selling activities
Building good customer relationships
Mentoring of Junior Employees

Table 3: Summary Statistics: # Obs=2148

Variables	Mean	St Dev	Min	Max
Evaluation	2.664	(0.837)	1	5
Evaluation=s	0.008	(0.091)	0	1
Evaluation=a	0.141	(0.348)	0	1
Evaluation=b	0.430	(0.495)	0	1
Evaluation=c	0.348	(0.477)	0	1
Evaluation=d	0.073	(0.260)	0	1
Profit (million yen)	20.716	(7.991)	1.373	61.578
Junior to experienced rep ratio	0.046	(0.101)	0	0.667
Corporate customer share	0.267	(0.105)	0.076	0.796
Branch-firm productivity difference (million yen)×(it is positive)	1.224	(2.130)	0	14.370
Branch-firm productivity difference (million yen) ×(it is negative)	-1.149	(1.585)	-7.960	0
Worker's tenure	12.876	(9.518)	0.750	42
Supervisor's tenure	25.753	(5.712)	5	36
Worker's educ=university	0.650	(0.477)	0	1
Worker's educ=vocational school	0.123	(0.329)	0	1
Worker's educ=high school	0.214	(0.410)	0	1
(The excluded category=below high school)				
#Sales reps at the branch$^{(a)}$	8.273	(2.033)	3.833	13
Average worker tenure at the barnch	12.808	(3.216)	5.329	22.561
Sd of worker tenure at the branch	9.227	(2.765)	2.871	18.507
Salary stage=S	0.027	(0.161)	0	1
Salary stage=A	0.101	(0.302)	0	1
Salary stage=B	0.259	(0.438)	0	1
Salary stage=C	0.307	(0.461)	0	1
Salary stage=D	0.306	(0.461)	0	1
Year 2001	0.256	(0.436)	0	1
Year 2002	0.251	(0.434)	0	1
Year 2003	0.242	(0.428)	0	1

(a) When a worker workers for a fraction of a year, the fraction is added in the computation of # Sales reps at the branch. Thus, this variable can take a non-integer value.

Figure 3: Evaluation and profits from car sales

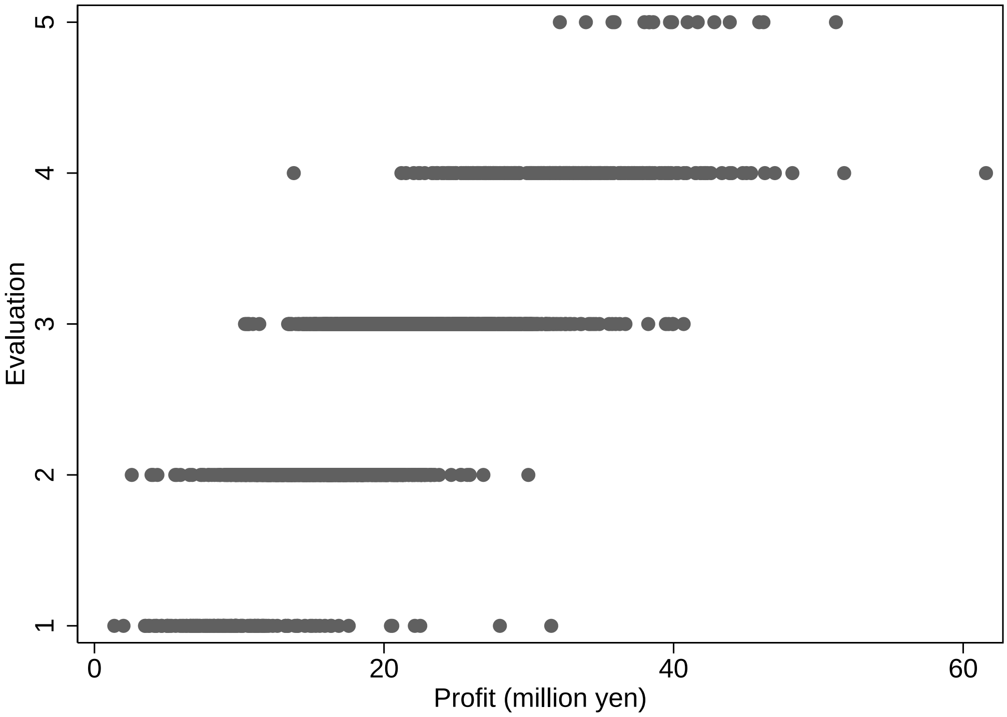


Table 4: Ordered probit evaluation regressions

	Model 1	Model 2	Model 3	Model 4	Model 5
Profit	0.261 ***	0.263 ***	0.248 ***	0.274 ***	0.282 ***
	(0.013)	(0.014)	(0.013)	(0.014)	(0.015)
(Profit)×(Junior to experienced rep ratio)	-0.107 **	-0.081 *	-0.071	-0.101 **	-0.080
	(0.045)	(0.048)	(0.048)	(0.049)	(0.051)
Junior to experienced rep ratio	2.382 **	2.162 *	1.766	3.013 **	2.474
	(1.022)	(1.138)	(1.172)	(1.193)	(1.253)
(Profit)×(Corporate customer share)	-0.103 ***	-0.090 **	-0.104 ***	-0.085 *	-0.081 *
	(0.038)	(0.040)	(0.036)	(0.044)	(0.047)
Corporate customer share	2.650 ***	2.240 ***	2.554 ***	1.637	1.287
	(0.693)	(0.764)	(0.728)	(1.169)	(1.257)
Branch-firm productivity difference		-0.033 *	-0.026	-0.066 **	-0.042 *
×(it is positive)		(0.020)	(0.018)	(0.029)	(0.024)
Branch-firm productivity difference		-0.088 ***	-0.081 ***	-0.162 ***	-0.129 ***
×(it is negative)		(0.022)	(0.022)	(0.034)	(0.035)
Worker's tenure		0.086 ***	0.038 ***	0.087 ***	0.090 ***
		(0.014)	(0.015)	(0.015)	(0.015)
Worker's tenure^2		-0.001 ***	-0.001 ***	-0.001 ***	-0.001 ***
		(0.000)	(0.000)	(0.000)	(0.000)
Supervisor's tenure		0.001	-0.002	-0.010 *	-2.277
		(0.007)	(0.007)	(0.006)	(1.418)
Worker's educ=university		0.801	0.703	0.944 **	1.026 **
		(0.499)	(0.548)	(0.474)	(0.492)
Worker's educ=vocational school		0.660	0.568	0.846 *	0.954 *
		(0.514)	(0.558)	(0.495)	(0.513)
Worker's educ=high school		0.834 *	0.732	0.968 **	1.085 **
		(0.486)	(0.535)	(0.457)	(0.473)
#Sales reps at the branch		-0.022	-0.068	-0.839 *	-0.268
		(0.104)	(0.102)	(0.434)	(0.277)
#Sales reps at the branch^2		0.002	0.005	0.037	0.018
		(0.006)	(0.006)	(0.024)	(0.016)
Average worker tenure at the branch		0.015	0.014	0.070 ***	0.059 *
		(0.012)	(0.012)	(0.023)	(0.023)
Sd of worker tenure at the branch		-0.044 ***	-0.038 **	-0.088 ***	-0.060 **
		(0.016)	(0.016)	(0.025)	(0.029)
Salary stage=S			2.144 ***		
			(0.233)		
Salary stage=A			2.104 ***		
			(0.175)		
Salary stage=B			1.533 ***		
			(0.125)		
Salary stage=C			0.803 ***		
			(0.086)		
Year dummies	Yes	Yes	Yes	Yes	Yes
Branch dummies	No	No	No	Yes	No
Supervisor dummies	No	No	No	No	Yes
Pseudo R squared	0.45	0.50	0.53	0.53	0.54
#Obs	2148	2148	2148	2148	2148

Cluster robust sd errors at sales group level are in the parentheses. *, **, ***, significant at 10, 5, 1 percent.

Table 5: Tobit Evaluation Regressions

Variable	Model 1	Model 2	Model 3	Model 4	Model 5
Profit	0.103 ***	0.094 ***	0.083 ***	0.093 ***	0.092 ***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
(Profit)×(Junior to experienced rep ratio)	-0.067 ***	-0.047 ***	-0.041 **	-0.051 ***	-0.044 ***
	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)
Junior to experienced rep ratio	1.584 ***	1.274 ***	1.086 ***	1.544 ***	1.404 ***
	(0.429)	(0.435)	(0.416)	(0.430)	(0.423)
(Profit)×(Corporate customer share)	-0.038 **	-0.028 *	-0.031 **	-0.023	-0.020
	(0.015)	(0.014)	(0.012)	(0.015)	(0.015)
Corporate customer share	1.010 ***	0.714 **	0.796 ***	0.350	0.191
	(0.302)	(0.282)	(0.259)	(0.405)	(0.422)
Branch-firm productivity difference		-0.014 **	-0.011 *	-0.029 ***	-0.018 **
×(it is positive)		(0.007)	(0.006)	(0.010)	(0.008)
Branch-firm productivity difference		-0.037 ***	-0.033 ***	-0.061 ***	-0.046 ***
×(it is negative)		(0.008)	(0.008)	(0.012)	(0.012)
Worker's tenure		0.038 ***	0.018 ***	0.037 ***	0.036 ***
		(0.005)	(0.005)	(0.005)	(0.005)
Worker's tenure^2		-0.001 ***	0.000 ***	-0.001 ***	-0.001 ***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)
Supervisor's tenure		0.000	-0.001	-0.003 *	-0.631
		(0.003)	(0.002)	(0.002)	(0.524)
Worker's educ=university		0.245	0.199	0.279 *	0.278 *
		(0.178)	(0.178)	(0.160)	(0.162)
Worker's educ=vocational school		0.184	0.145	0.238	0.246
		(0.183)	(0.181)	(0.166)	(0.168)
Worker's educ=high school		0.251	0.205	0.283 *	0.297 *
		(0.174)	(0.175)	(0.155)	(0.157)
#Sales reps at the branch		-0.017	-0.032	-0.303 *	-0.084
		(0.038)	(0.033)	(0.157)	(0.097)
#Sales reps at the branch^2		0.001	0.002	0.013	0.006
		(0.002)	(0.002)	(0.009)	(0.006)
Average worker tenure at the branch		0.007	0.007	0.029 ***	0.026 ***
		(0.005)	(0.004)	(0.008)	(0.008)
Sd of worker tenure at the branch		-0.019 ***	-0.016 ***	-0.034 ***	-0.025 **
		(0.006)	(0.006)	(0.009)	(0.010)
Salary stage=S			0.737 ***		
			(0.073)		
Salary stage=A			0.717 ***		
			(0.057)		
Salary stage=B			0.528 ***		
			(0.041)		
Salary stage=C			0.314 ***		
			(0.031)		
Year dummies	Yes	Yes	Yes	Yes	Yes
Branch dummies	No	No	No	Yes	No
Supervisor dummies	No	No	No	No	Yes
Pseudo R squared	0.40	0.46	0.49	0.48	0.50
#Obs	2148	2148	2148	2148	2148

Cluster robust sd errors at sales group level are in the parentheses. *, **, ***, significant at 10, 5, 1 percent.

Table 6: Does the performance evaluation contain information about future sales performance?

Dependent Variable=Profit$_{i,t+1}$

Variables	Coefficients
Evaluation$_{it}$	0.580 **
	(0.278)
Profit$_{it}$	0.700 ***
	(0.031)
Worker's tenure$_{it}$	-0.103 ***
	(0.022)
Supervisor's tenure$_{it}$	0.002
	(0.032)
Branch-firm productivity difference$_{it}$	-0.690 ***
×(it is positive)	(0.110)
Branch-firm productivity difference$_{it}$	-0.755 ***
×(it is negative)	(0.134)
Average worker tenure at the branch$_{it}$	0.094
	(0.120)
Sd of worker tenure at the branch$_{it}$	0.165
	(0.151)
#Sales reps at the branch$_{it}$	-1.166
	(2.240)
#Sales reps at the branch$_{it}{}^2$	0.055
	(0.114)
Salary stage$_{it}$=S	4.305 **
	(1.841)
Salary stage$_{it}$=A	2.680 ***
	(0.913)
Salary stage$_{it}$=B	1.703 ***
	(0.579)
Salary stage$_{it}$=C	1.222 ***
	(0.429)
Constant	8.033
	(10.954)
Year dummies	Yes
R squared (within)	0.63
#Obs	1393

Cluster robust sd errors at the worker level are in the parentheses. *, **, ***, significant at 10, 5, 1 percent.

Figure 4: Residuals from Model 4 (Table 4) ordered probit evaluation regression

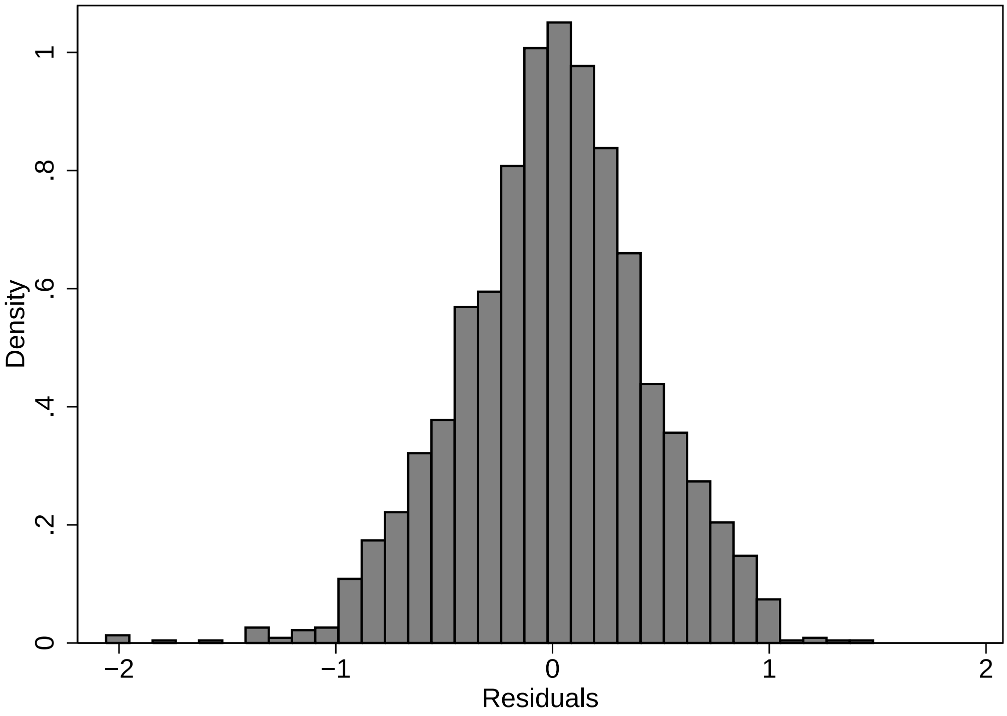


Table 7: Predicting worker quits using the residuals of the evaluation regression

	Probit	Linear prob worker fixed effects			2SLS
Variables	(1)	(2)	(3)	(4)	(5)
Residual$_{it}$<-0.5	0.755 ***	0.047 ***	0.014	0.050 **	0.399 **
	(0.194)	(0.014)	(0.014)	(0.024)	(0.165)
(Residual$_{it}$<-0.5)×(Worker tenure)				-0.002 **	
				(0.001)	
Residual$_{it}$>+0.5	0.031	0.007	0.034 **	0.041 *	
	(0.246)	(0.009)	(0.015)	(0.022)	
(Residual$_{it}$>+0.5)×(Worker tenure)				-0.001	
				(0.001)	
Profit	-0.115 ***	0.000	0.004 *	0.004 *	-0.017 **
	(0.030)	(0.002)	(0.002)	(0.002)	(0.007)
(Profit)×(Junior to Experienced Rep Ratio)	0.075	0.000	-0.003	-0.002	0.010
	(0.120)	(0.003)	(0.003)	(0.003)	(0.007)
Junior to Experienced Rep Ratio	-0.566	0.011	0.071	0.070	-0.230
	(1.954)	(0.084)	(0.079)	(0.080)	(0.195)
(Profit)×(Corporate Customer Share)	0.154 **	-0.009	-0.010	-0.011	0.010
	(0.076)	(0.007)	(0.007)	(0.007)	(0.006)
Corporate customer Share	-2.143	0.318	0.323	0.332 *	-0.208
	(1.551)	(0.202)	(0.197)	(0.196)	(0.157)
(Branch-firm productivity difference)	0.032	-0.004	-0.006 *	-0.006 *	0.005 **
×(it is positive)	(0.050)	(0.003)	(0.003)	(0.003)	(0.003)
(Branch-firm productivity difference)	0.010	-0.002	-0.004 *	-0.004 *	0.001
×(it is negative)	(0.033)	(0.002)	(0.002)	(0.002)	(0.003)
Worker tenure	0.005	0.026 ***	0.033 ***	0.033 ***	-0.007 **
	(0.029)	(0.004)	(0.006)	(0.006)	(0.003)
Worker tenure^2	-0.003	-0.001 ***	-0.001 ***	-0.001 ***	0.0001
	(0.002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Worker's education in years	-0.110 ***				-0.004
	(0.041)				(0.003)
Supervisor's education in years	0.042				0.005 **
	(0.055)				(0.002)
Supervisor's tenure	0.003	0.000	-0.001	-0.001	0.001
	(0.016)	(0.001)	(0.001)	(0.001)	(0.001)
Average worker tenure at the branch	-0.003	-0.005 **	-0.004 *	-0.004 *	-0.001
	(0.037)	(0.002)	(0.002)	(0.002)	(0.002)
S.D. of worker tenure at the branch	0.008	-0.002	-0.003 *	-0.003 *	0.005
	(0.038)	(0.002)	(0.002)	(0.002)	(0.003)
Evaluation=s			-0.200 **	-0.190 *	0.451 *
			(0.079)	(0.077)	(0.260)
Evaluation=a			-0.177 ***	-0.169 ***	0.327
			(0.066)	(0.065)	(0.211)
Evaluation=b			-0.131 **	-0.124 **	0.186
			(0.056)	(0.055)	(0.152)
Evaluation=c			-0.106 **	-0.098 **	0.017
			(0.046)	(0.044)	(0.081)
Constant	1.141				0.234 ***
	(1.214)				(0.083)
R squared (Pseud or within)	0.21	0.07	0.09	0.10	
#Obs	2148	2148	2148	2148	2148

For (1) to (4), bootstrapped sd errors are in the parentheses. For 2SLS model, cluster robust sd errors at the sales group level are in the parentheses. Residuals are computed from Table 4 Model 4. *, **, ***, significant at 10, 5, 1 percent.

Table 8: The first stage regression of Table 7 Column 5.

Dep var= Residual$_{it}$<-0.5	
Profit	0.041 ***
	(0.003)
(Profit)×(Junior to experienced rep ratio)	-0.033 ***
	(0.008)
Junior to experienced rep ratio	0.881 ***
	(0.199)
(Profit)×(Corporate customer share)	-0.017 **
	(0.007)
Corporate customer share	0.380 **
	(0.161)
(Branch-firm productivity difference)	-0.012 ***
(× it is positive)	(0.003)
(Branch-firm productivity difference)	-0.008 **
(× it is negative)	(0.004)
Worker tenure	0.015 ***
	(0.002)
Worker tenure^2	0.000 ***
	(0.000)
Worker's education (years)	-0.001
	(0.004)
Supervisor's education (years)	-0.008 **
	(0.004)
Supervisor tenure	-0.002 *
	(0.001)
Average worker tenure at the branch	0.003
	(0.003)
S.D. of worker tenure at the branch	-0.013 ***
	(0.003)
Evaluation=s	-1.627 ***
	(0.066)
Evaluation=a	-1.320 ***
	(0.051)
Evaluation=b	-0.951 ***
	(0.038)
Evaluation=c	-0.496 ***
	(0.033)
Year 2001	0.057 ***
	(0.016)
Year 2002	0.068 ***
	(0.016)
Year 2003	0.149 ***
	(0.017)
Constant	-0.116
	(0.121)
(Excluded instruments)	
Supervisor's educ > Worker's educ	0.058
	(0.040)
Supervisor's educ < Worker's educ	0.153 ***
	(0.058)
R squared	2148
#Obs	0.3843

Cluster robust sd errors are in the parentheses. *, **, ***, significant at 10, 5, 1 percent.

Table 9: Predicting worker quits using supervisor-worker match fixed effects. (Dep var = Quit$_{it}$)

Variables	(1)	(2)
Δ Supervisor-worker match fixed effect	-0.048 ***	
	(0.015)	
Δ Supervisor-worker match fixed effect		-0.070 ***
×(it is negative)		(0.027)
Δ Supervisor-worker match fixed effect		-0.025
×(it is positive)		(0.021)
Profit	-0.008 ***	-0.008 ***
	(0.003)	(0.003)
(Profit)×(Junior to experienced rep ratio)	-0.001	0.000
	(0.005)	(0.005)
Junior to experienced rep ratio	0.069	0.046
	(0.163)	(0.171)
(Profit)×(Corporate customer share)	0.009	0.009
	(0.006)	(0.006)
Corporate customer share	-0.160	-0.154
	(0.141)	(0.138)
(Branch-firm productivity difference)	0.002	0.002
×(it is positive)	(0.002)	(0.002)
(Branch-firm productivity difference)	-0.001	-0.002
×(it is negative)	(0.004)	(0.004)
Worker tenure	-0.003	-0.003
	(0.003)	(0.003)
Worker's tenure^2	0.000	0.000
	(0.000)	(0.000)
Worker's education (years)	0.000	0.000
	(0.003)	(0.003)
Supervisor's education (years)	0.001	0.001
	(0.002)	(0.002)
S.D. of worker tenure at the branch	0.000	0.000
	(0.001)	(0.001)
Supervisor's tenure	-0.002	-0.002
	(0.002)	(0.002)
Average worker tenure at the branch	0.001	0.001
	(0.002)	(0.002)
Constant	0.268	0.242
	(0.103)	(0.102)
Evaluation dummies	Yes	Yes
Year dummies	Yes	Yes
R squared	0.08	0.08
#Obs	766	766

Inside parentheses are bootstrapped standard errors. For the first stage regression to compute the match fixed effects, we used the full 2148 observations (In STATA's bootstrap routine, nodrop option was used.). *, **, ***, significant at 10, 5, 1 percent.

For Online Publication: Appendix A

Do the bias indicators predict the workers' perceived fairness?

We investigate whether the bias measures we constructed in Section V.C in fact predict the workers' acceptance of their evaluation results. In August 2006, we conducted a survey of workers who were randomly sampled at Auto Japan. Among new car sales representatives, the survey was distributed to a sample of 297 subjects, of which 291 responded. Among the respondents, 241 were in our 2003 sample. Thus, our perceived fairness regressions contain only 241 observations. The survey asked the respondents, among other things, to rate the fairness of their evaluations in percentage terms; the higher the percentage, the more fair the evaluation was perceived to be.[29] The survey also asked respondents whether they had received feedback from their supervisors regarding their evaluation results and 59 percent said they had received some feedback. The questionnaires included worker identification numbers, so this survey data could be merged with the sales and evaluation data.

One drawback of our survey was that it was conducted after our sample period. At the time of the survey, the most recent evaluation that workers had received was for the 2005 fiscal year, while our data ends with the 2003 fiscal year evaluation. This gap forces us to use the 2003 explanatory variables to predict perceived fairness in 2005. However, the responses to the survey would reflect the workers' experiences over the intervening period. In addition, a substantial portion of workers were working under the same supervisors in 2005 as in 2003. Thus, 2003 bias indicators would still have some power to predict the levels of perceived fairness reported in 2005.

[29] The respondents were assured that individual responses are used only for academic research and that only the summary information would be given to the management.

Note that workers who moved to other sections, were promoted to supervisor, or quit prior to the survey are not included in the survey. To correct for the potential bias stemming from this sample selection, we include the inverse Mill's ratio in the perceived fairness equation.[30] To separate the effects of bias from low evaluations, the regression models also control for evaluation itself.

Table A.1 shows the tobit regression results with bootstrapped standard errors. Column 1 predicts perceived fairness using our potential bias indicators. The dummy for a negative evaluation gap has a negative and statistically significant coefficient. The computed marginal effect (not shown in the table) indicates that a negative evaluation gap would decrease the perceived fairness rating by 17 percentage points. This model, however, does not control for evaluation itself. To separate the effect of bias from the effect of low evaluation, Column 2 controls for evaluation itself. The coefficient for the negative evaluation gap somewhat reduced, but it is still significant at the 5 percent level. The marginal effect of the negative evaluation gap is a 13 percentage point decrease in the perceived fairness rating.

Column 3 includes the interaction between the negative evaluation gap and feedback to capture the possibility that the effect of bias may differ depending on whether one has received any feedback. As such, there is a negative and statistically significant effect of the negative evaluation gap for those who did not receive any feedback. The computed marginal effect indicates that a negative evaluation gap would decrease the perceived fairness rating by 30 percentage points for those who did not receive any feedback. On the other hand, negative evaluation gap has no effect for those who received some feedback. This result shows the importance of feedback in reducing the perception of unfairness in evaluation.

[30] We derived the inverse Mill's ratio from the probit model where the dependent variable is the dummy variable indicating whether a particular observation is included in the perceived fairness regression. The selection equation contains the same variables in Model 4 (Table 4) except the year and branch dummies.

Table A.1: Determinants of perceived fairness about the evaluation results: Tobit regressions, Dept Var=Perceived Fairness.

Variables	(1)	(2)	(3)
Residual$_{it}$<-0.5	-17.766 ***	-13.856 **	-31.689 **
	(6.825)	(7.025)	(14.816)
(Residual$_{it}$<-0.5) × (Received Feedback)			29.036 *
			(17.477)
Residual$_{it}$>+0.5	7.589	-0.596	-0.471
	(5.113)	(5.997)	(6.042)
Received Feedback	11.844 ***	11.538 ***	8.902 ***
	(3.233)	(3.093)	(3.272)
Worker tenure	0.659	-0.301	-0.241
	(0.609)	(0.665)	(0.660)
Worker tenure^2	-0.008	0.014	0.013
	(0.017)	(0.019)	(0.018)
Worker's education (years)	0.838	1.131	1.463
	(1.272)	(1.231)	(1.186)
Supervisor's education (years)	0.004	-0.048	-0.002
	(0.366)	(0.360)	(0.378)
Supervisor tenure	-1.274	-1.300	-1.224
	(0.852)	(0.929)	(0.954)
(Branch-firm productivity difference)	1.156	0.649	0.462
×(it is positive)	(0.847)	(0.811)	(0.815)
(Branch-firm productivity difference)	-0.795	-1.272	-1.057
×(it is negative)	(0.987)	(1.010)	(0.998)
Average worker tenure at the branch	0.440	0.912	0.828
	(0.668)	(0.610)	(0.544)
S.D. of worker tenure at the branch	-0.124	-0.306	-0.339
	(0.784)	(0.741)	(0.727)
Evaluation=s		42.543	44.928
		(57.726)	(58.517)
Evaluation=a		16.238 *	18.784 **
		(8.463)	(8.005)
Evaluation=b		8.665	11.122
		(7.146)	(6.792)
Evaluation=c		-1.312	2.386
		(7.021)	(7.239)
Inverse Mill's Ratio	20.008 *	7.908	8.403
	(12.117)	(10.545)	(10.183)
Constant	22.499	36.433	28.431
	(35.838)	(34.831)	(33.975)
Puseud R squared	0.02	0.02	0.03
# Obs	241	241	241

Bootstrapped sd errors are in the parentheses. For the first stage regression to compute the bias measures, we used the full 2148 observations (In STATA's bootstrap routine, nodrop option was used.). Tobit regressions use zero as the lower bound and 100 as the upper bound. *, **, ***, significant at 10, 5, 1 percent. Residuals are computed from Table 4 Model 4.