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PEOPLE MANAGEMENT SKILLS, EMPLOYEE ATTRITION, AND MANAGER REWARDS: AN EMPIRICAL ANALYSIS

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ABSTRACT

How much do a manager's interpersonal skills with subordinates, which we call people management skills, affect employee outcomes? Are managers rewarded for having such skills? Using personnel data from a large, high-tech firm, we show that survey-measured people management skills have a strong negative relation to employee turnover. A causal interpretation is reinforced by research designs exploiting new workers joining the firm and manager moves. However, people management skills do not consistently improve most observed non-attrition outcomes. Better people managers themselves receive higher subjective performance ratings, higher promotion rates, and larger salary increases.

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An online appendix is available at http://www.nber.org/data-appendix/w24360

1 Introduction

It is broadly accepted that even within narrow industries, there are large and persistent productivity differences across firms and countries (Syverson, 2011). While a growing literature shows that management practices affect firm performance and help explain these productivity differences (Ichniowski et al., 1997; Ichniowski and Shaw, 1999; Bloom and Van Reenen, 2007; Bloom et al., 2013, 2019), less attention has been devoted to managers themselves.

Of particular interest to understanding how managers affect outcomes are *people management skills*, which we take to be a manager's interpersonal (i.e., social) skills in relations with their subordinates. Non-cognitive skills like social skills play a key role in economic life (Heckman and Kautz, 2012) and social skills are increasingly rewarded in the labor market (Deming, 2017). How much do good people management skills matter for employee outcomes? Are good people management skills rewarded inside the firm?

We answer these questions using employee surveys conducted at a large, high-tech firm combined with rich personnel data that cover thousands of managers and tens of thousands of employees. We show that people management skills (1) reduce employee attrition, particularly attrition the firm wishes to avoid, and that the relation appears causal; (2) do not consistently improve most non-attrition outcomes; and (3) are rewarded by the firm. We explore how the impact of people management skills varies across levels of hierarchy, countries, and occupations, providing granular quantification of managerial differences within the same multinational firm.

Progress has been made in examining how much managers matter using a "value-added" (VA) approach. Bertrand and Schoar (2003) examine how much CEOs matter by regressing firm outcomes on CEO fixed effects, pioneering an approach used by a subsequent finance literature. Lazear et al. (2015) use data from one firm to show that frontline supervisors matter a great deal for productivity. Bender et al. (2018) analyze interactions between employees/managers and management practices in Germany.

Despite its strengths, the VA approach faces two main limitations when applied to our research questions. First, VA estimates measure the overall impact of a given manager on individual outcomes, not the separate impact of people management skills. A manager's traits may include the ability to bring in high-value clients, or better problem-solving skills. Second, VA studies require good objective data on worker productivity. However, in many firms, direct data on individual worker productivity is scarce, and sometimes impossible to measure, particularly in high-skill, collaborative environments.

We take a different yet complementary approach to VA by measuring people management skills using employees' responses to questions about their manager (e.g., whether their manager is trustworthy or provides adequate coaching).[1] We explore the extent to which people management skills relate to employee outcomes, with the greatest focus on employee attrition. Conventional wisdom, especially in high-tech firms, is that employee attrition is a key way by which worker knowledge and skills are lost. As such, high-tech firms are deeply interested in what can be done to reduce turnover, particularly that of high performers.

Our data are well-suited to address possible concerns about measuring people management skills using surveys. First, the response rate at our firm is about 95%. Second, while employees may fear their manager will know how they evaluated them, this concern is mitigated due to the confidential nature of the survey. Workers are truthfully told that their individual responses will never be observed by the firm. Instead, managers receive aggregated results, and only for managers with a minimum number of employees responding. Third, survey responses may contain measurement error due to inattentiveness, sampling error, or different employees treating questions differently. We address this using an instrumental variable (IV) strategy where a manager's score in one wave is instrumented using his or her score in the other wave. Section 2 further describes the data. Section 3 covers our empirical strategy.

Our first main finding, presented in Section 4, is that people management skills have a strong negative relation to employee attrition. Increasing a manager's people management skills from the 10th to the 90th percentile predicts a 60% reduction in turnover. These results are quite strong in terms of retaining the firm's high performers, both defined in terms of classifying employees based on subjective performance scores and using the firm's definition of "regretted" voluntary turnover. Beyond classical measurement error, the IV strategy addresses survey measurement error that is contemporaneously correlated with attrition.

Still, the question remains whether these results are causal. Even for our IV estimates, there are concerns about non-contemporaneous measurement error in people management skills that is correlated with employee attrition, as well as concerns that the firm optimally sorts managers and employees together. We address these using multiple identification strategies, some similar to those in the teacher VA literature (Chetty et al., 2014). Our first strategy analyzes outcomes of employees who join mid-way through our sample, using a manager's quality measured before an employee joins the firm as an IV. This addresses concern about non-permanent, unobservable shocks affecting turnover and manager ratings, and reduces concern that the results are driven by the firm sorting managers and employees based on long-time information about the employee. Our second strategy additionally analyzes instances of workers switching managers, allowing us to test for non-random assignment of

[1] Many organizations survey employees about their managers, including Google (Garvin et al., 2013), RBC (Shaw and Schifrin, 2015), Pepsi (Bracken et al., 2016), and the US federal government (Arrington and Dwyer, 2018). CultureAmp, a firm survey vendor, informed us that about 15-20% of its 1,000+ clients use upward feedback surveys (i.e., surveys of employees about their managers) focused on giving managers feedback.

managers and workers, and to analyze how the impact of people management skills varies based on time together between a manager and employee. Our third strategy exploits managers moving across locations or job functions within the firm, allowing us to address more permanent unobservables as well as to rule out assignment bias. All strategies point to people management skills having a strong, causal effect on attrition. Section 5 discusses heterogeneity in MOR effects.

Section 6 shows that people management skills do not have a consistent positive relation to most observed non-attrition employee outcomes. We find no evidence of a positive relation between people management skills and employee salary growth, probability of promotion, or patenting. There is some evidence of a positive relation for employee subjective performance, but results are not robust across analyses. These findings suggest that better people management skills do not reduce attrition by making workers more "productive" (though we freely admit that it is hard to measure knowledge worker productivity), but may instead cause workers to better enjoy their jobs. We caveat that most non-attrition outcomes are not observed in our data and that effects could be observed over a longer horizon.

Section 7 establishes our secondary result that managers with better people management skills get rewarded by the firm. Better people managers attain substantially higher subjective performance scores, are more likely to be promoted, and receive larger salary increases. Such results are consistent with the firm valuing the role of good people management skills in reducing employee turnover.

Our paper contributes to several literatures. First, it relates to work on individual managers, from which Lazear et al. (2015), who study the impact of front-line supervisors in a low-skill setting, is most related.[2] Aside from focusing on a large high-tech firm, our paper differs from theirs in several other ways. Most notably, their VA approach shows that managers matter, but this may be through "hard" skills, like problem-solving, and soft skills, which is our focus. Also, unlike Lazear et al. (2015), we are able to test whether managers are rewarded for skills. Our paper also differs by analyzing quasi-experiments from managers switching locations or functions, and by characterizing heterogeneity in estimates by hierarchy, geography, and occupation. Beyond the broader VA literature, our paper relates to other work that tries to open the black-box of what managers do. For example, Bandiera et al. (2019) use CEO diary data and find that CEOs who focus on high-level agendas outperform those who focus on functional management. We instead explore managerial differences by using employee surveys as a measure of manager skills. Like prior work showing that management practices can be usefully measured using surveys (Ichniowski et al., 1997; Bloom and Van

[2] Also, Bandiera et al. (2007) show that manager bonuses boost productivity. Bandiera et al. (2015) study matching of top managers and firms. Friebel et al. (2018) show that store managers change behavior in response to letters.

Reenen, 2007), we show the same is true of particular managerial skills.

Second, it relates to studies of compensation in firms (e.g., Baker et al., 1994a), providing novel evidence that people management skills are rewarded within the firm. We find that people management skills are a much stronger predictor of rewards by the firm than attrition VA; hence, a researcher using attrition VA who lacked data on people management skills would reach different conclusions than ours regarding manager rewards.

Third, we contribute to the growing literature on social skills in the workplace. People management skills are one variety of "social" or "people" skills. Social skills are generally considered to be a person's ability to communicate effectively with others (Borghans et al., 2014), and we define people management skills as a manager's ability to effectively interact with their subordinates. Deming (2017) emphasizes that social skills are important for teamwork (horizontal production). We provide novel evidence that social skills are also important in management (vertical production). Relatedly, Schoar (2016) shows that an intervention in garment factories aimed at improving supervisors' communication skills and treatment of workers improved productivity. Weinberger (2014) and Deming (2017) show that a worker's social and cognitive skills are complementary. As we observe a stronger relation between people management skills and attrition for workers in more cognitively demanding jobs (namely, ones higher up in the firm's hierarchy), our results suggest complementarity between a manager's people management skills and a worker's cognitive skills. This is noteworthy as it occurs even within the firm's relatively high-skilled workforce.

Fourth, it expands the literature on knowledge-based employees (e.g., Baker et al., 1994a; Bartel et al., 2017; Kuhnen and Oyer, 2016; Brown et al., 2016). Much of empirical personnel economics focuses on lower-skilled jobs (e.g., truckers, retail, and farm-workers), partially because it is relatively simple to measure individual productivity. In contrast, for knowledge-based jobs, production is often complex, multi-faceted, and involves teamwork. Our analysis sheds light on the managerial production function in these settings.

Finally, it relates to work on subjective performance evaluation and workplace feedback. Like subjective performance evaluations, employee surveys help account for difficult-to-measure aspects of performance (Baker et al., 1994b). Our paper highlights a version of performance evaluation, namely upward feedback surveys, that has received much less attention than subjective performance ratings, particularly by economists.[3]

[3]Surveys explored in psychology and management seem focused on different issues like consistency of ratings (e.g., Atkins and Wood, 2002) or rating system design (e.g., Bracken et al., 2016) rather than the causal impact of people management skills on outcomes. Also, to our knowledge, this work does not use our IV approach or quasi-experimental designs.

2 Data and Institutional Setting

Our data, obtained from a U.S.-headquartered high-tech firm, cover a period of two years and three months, some time during 2011-2015, and are organized as a worker-month panel. To preserve firm confidentiality, certain details regarding the firm and exact time period cannot be provided. We refer to the three years of the data as Y_1, Y_2, and Y_3.

The firm is divided into several broad business units and workers are classified by job function. A core job function is engineering, comprising 36% of worker-months in our sample. Engineers build and debug code, often working in teams. Many workers are in non-engineering functions (e.g., marketing, finance, sales). As in many high-tech firms, the firm also has a large number of lower-skilled workers in customer service/operations, but we exclude them from our analysis, given our broad motivation of better understanding high-skilled workplaces.

About 21% of observations (worker-months) are for individuals in manager roles, so the majority of observations are for non-managers, often referred to in industry as individual contributors. While our data begin in January Y_1, 71% of workers in our sample are hired before that date. Further details describing the data are in Appendix B.

2.1 Employee Surveys

Every year, employees are given a detailed survey used by the firm's Human Resource (HR) department and executives to gain an accurate sense of employee opinions. To ensure the anonymity of responses, survey information about one's manager is only collected on managers who manage a minimum number of individuals.[4]

Surveys of this type are typically administered before year-end, and ours were performed around September in Y_1 and Y_2. The survey had the same manager questions and format in Y_1 and Y_2, and the response rate was about 95%. To match outcomes with their associated survey, we follow what the firm's HR analysts did for internal reporting. Observations from January Y_1-September Y_1 are assigned the survey information from the Y_1 survey, whereas other observations are assigned the survey information from the Y_2 survey.

Manager questions. Every year employees are asked six key questions about their manager. For each question, employees are asked whether they Strongly Disagree, Disagree,

[4] In the Y_1 survey, the threshold was 3 employees, whereas in Y_2, the threshold was 5 employees. We do not know the number of people who responded for each manager. To check that measurement error from surveys on small teams does not drive the results, we verify that our main results are robust to restricting to workers where the manager has an at or above the median (i.e., at least 8 or 9) number of direct reports. The survey is "third-party confidential" meaning that the survey vendor, a third-party independent firm, has access to responses so it can tie them to employee attributes to generate statistical information.

Neither Agree nor Disagree, Agree, or Strongly Agree. Specifically, we observe answers to the following survey items about one's immediate manager:[5]

1. communicates a clear understanding of the expectations from me for my job.
2. provides continuous coaching and guidance on how I can improve my performance.
3. actively supports my professional/career development.
4. consults with people for decision making when appropriate.
5. generates a positive attitude in the team, even when conditions are difficult.
6. is someone whom I can trust.

The questions measure broad aspects of a manager's effectiveness in interacting well with subordinates, which is presumably enhanced by clear expectations, appropriate trust, and positivity. Thus, we use the responses to construct our measure of people management skills.

In the data provided to us, a manager's rating on an item is the share of employees who marked Agree or Strongly Agree. For example, if a manager has 8 direct reports, and 6 of them marked Agree or Strongly Agree for one of the items, the manager's score on that item would be 75 out of 100.[6] A manager's overall rating (MOR) is the average of scores on the 6 items, e.g., if a manager had a score of 100 on the first 3 items and a score of 50 on the second 3 items, MOR is 75. The MOR is easy to compute and is used by the firm in its internal reporting and communications. We use MOR as our main measure of people management skills. If employees experience multiple managers over the survey period, they only rate their most recent manager.

Persistence of MOR. Panel (a) of Figure 1 shows that manager scores are somewhat persistent over time using a binned scatter plot with no controls. The coefficient of 0.37 means that a manager who scores 10 points higher in the Y_1 survey in MOR is scored 3.7 points higher on average in the Y_2 survey. Table 2 shows a similar pattern of some persistence while applying control variables and using normalized survey scores. Each column regresses MOR or one of the six manager questions from the Y_2 survey on the same variable in the Y_1 survey. In column 1, a manager who performs 1 standard deviation (σ) higher in Y_1 scores 0.37σ higher in Y_2.

Though sizable, the predictiveness of the scores over time is perhaps not as high as one might expect. We believe that the main reason is measurement error in the surveys. Even though the firm strongly encouraged workers to take the surveys quite seriously (reflected in the high response rate), measurement error often occurs when respondents are asked to answer

[5]The manager questions are part of a longer survey covering other topics (e.g., firm decision-making, worker engagement). To preserve firm confidentiality, the wording may be slightly modified from the original.

[6]It is common practice to break the 5-answer scale into 2 or 3 parts (Garvin et al., 2013). Using the share of workers marking 4 or 5 (as we do) is consistent with how other firms measure managers on similar surveys.

many questions, particularly subjective ones (Bound et al., 2001). Measurement error could arise from inattention, survey fatigue, short-term mood, or from MOR being created by taking the share of individuals marking Agree or Strongly Agree to a question (thereby introducing noise from an average over a discrete categorization).[7] Observed persistence increases when restricting attention to workers on larger teams, consistent with classical measurement error shrinking persistence. Section 3 details how our empirical strategy addresses measurement error.

2.2 Employee Outcomes

In knowledge-based firms such as the one we study, employee performance has multiple dimensions and is not measured by a single metric. Our data's core employee outcomes are:

Turnover. Many firms consider turnover to be a significant problem, especially high-tech firms, because employee knowledge is a key asset and turnover is a loss of knowledge. In fact, as is common in many high-tech firms, our firm has analysts who try to predict and reduce turnover. We separately observe dates of voluntary attrition ("quits") and involuntary attrition ("fires"). We also observe whether the firm classified quits as "regretted" (highly-valued employee) or not, allowing us to go much further than is typical in analyzing turnover. To our knowledge, we are the first paper in economics to analyze data on whether a worker's attrition was regretted by the firm, which is valuable because it sheds light on the issue of "good" vs. "bad" attrition.

Subjective performance. The firm's subjective performance score ratings of each employee are on a discrete 1-5 scale, which is common (e.g., Frederiksen et al., 2017), and are set biannually by an employee's immediate and higher-up managers. While there are some broad guidelines for the distribution of these scores across various units within the firm, there is not a fixed "curve" across managers regarding subjective performance.

Salary increases. One way of proxying an employee's productivity improvements is the extent to which her salary increases. Because salaries are listed in local currency, we restrict analysis of salary to employees paid in U.S. dollars following Baker et al. (1994a). Salaries are set by the compensation department, though managers play a role in affecting worker raises.

Promotions. Promotions are pre-defined in the data provided by the firm, and correspond roughly to an increase in a person's salary grade. Promotions are a common proxy of productivity for higher skilled workers (Brown et al., 2016; Lyle and Smith, 2014).

[7]Other factors may limit persistence of MOR. First, managers may provide better supervision for certain teams or projects. Second, managers may change behavior or invest in manager effectiveness. Panels (b) and (c) of Figure 1 shows histograms for MOR in both periods.

Patents. Patents are commonly used to measure innovation (Jaffe et al., 1993). We analyze workers' patent applications because it takes years for the patent office to approve applications.[8] Since patents vary widely in value, we also analyze citation-weighted patents (Jaffe et al., 1993). Patents are most common among engineers, but non-engineers also patent.

While these outcomes all capture valuable aspects of worker performance, we do not claim to fully measure "productivity" in our setting. Certain aspects of productivity, such as the value of an engineer's computer code or the contributions of a businessperson to a new marketing strategy, seem impossible to quantify. This increases the usefulness of using surveys to study managers relative to using VA.

Different employee outcomes are available at different frequencies, but are coded in our data at the monthly level. Attrition events, promotions, and patent applications are based on exact dates. Subjective performance reviews occur twice per year, but scores are coded month-by-month. Annual salary is tracked at the monthly level, though salary increases are more likely to occur in spring and fall.

2.3 Assignment of Managers and Employees

The initial assignment of employees to managers reflects the projects and functions that require employees at any given time. Geographic needs also dictate circumstances in which employees experience a change of manager. The firm has an online portal where managers post internal workforce needs, and new employee-manager matches can form based on these postings. Managers play a key role in hiring and are also involved with dismissals. Thus, it is clear that employees are not being randomly assigned to managers. We further discuss manager assignment in Sections 3 and 4. Manager changes are observed month-by-month.

As described in Sections 3 and 4.4, we adopt several identification strategies to deal with the possibilities that MOR and outcomes are both correlated with unobservable project characteristics, e.g., some projects being exciting, leading to high scores in MOR and good outcome measures. A remaining concern may be that managers are selected into projects based on MOR. Conversations with several executives confirm that this is not done in practice; projects are assigned based on business needs and to some extent on domain expertise, but not based on MOR.

In our sample, managers manage an average of 9.35 employees at once. Weighted by worker tenure, employees experience an average of 1.52 managers in our sample.

[8] Workers disclose inventions to the legal department, who decides whether to file a patent application. Workers get a bonus when an application is filed, plus another bonus after a patent grant, so there is a significant incentive to disclose useful inventions.

2.4 Sample Creation and Summary Statistics

To create our sample, we restrict attention to worker-months where an employee has a manager with a non-missing MOR for both survey waves. This sample restriction is required for our IV analysis, where we instrument manager MOR in the current period using MOR in the other period. Our sample runs from January Y_1-March Y_3.

Table 1 provides summary statistics. While sample size cannot be shown to preserve firm confidentiality, our final sample contains well over 1,000 managers, 10,000 workers, and 100,000 worker-months. Mean MOR is 81 out of 100. 81% of employees are co-located with their manager and the rest are managed remotely. Mean attrition is 1.37% per month (∼15% per year). Most separations are quits, but there are still many fires.

3 Empirical Strategy

We wish to estimate how much manager j's underlying people management skill, m_j, affects an outcome, y_{it}, of employee i:

$$y_{it} = \beta m_{j(i,t)} + \varepsilon_{it} \tag{1}$$

where $j(i,t)$ means that j manages employee i at time t, though we will henceforth abbreviate $j(i,t)$ simply by j. Measurement error implies that instead of true people management skills, we only observe the noisy survey measure, $\widetilde{m}_j$. In our data, we have the two waves of the survey, giving us two manager scores $\widetilde{m}_{j,1}$ and $\widetilde{m}_{j,2}$, with $\widetilde{m}_{j,\tau} = m_j + u_{j,\tau}$, $\tau \in \{1,2\}$. t is at the monthly level, whereas there are two values of τ, which we call the *period*.

Perhaps the simplest approach to analyzing the impact of people management skills is to estimate OLS regressions of the form:

$$y_{i,t} = b\widetilde{m}_{j,\tau(t)} + \theta_{i,t} \tag{2}$$

where $\theta_{i,t}$ is an error term; and where $\tau(t) = 1$ if $t \leq$ month 9 of Y_1 and $\tau(t) = 2$ otherwise. However, OLS models may be biased by measurement error. An alternative approach (e.g., Ashenfelter and Krueger, 1994) is to instrument one survey measure with the other one:

$$\begin{aligned} y_{i,t} &= b\widetilde{m}_{j,\tau} + \theta_{i,t} \\ \widetilde{m}_{j,\tau} &= c\widetilde{m}_{j,-\tau} + \eta_{j,t} \end{aligned} \tag{3}$$

where $\widetilde{m}_{j,-\tau}$ is the measured people management score of manager j in the period other than the current one, and $\theta_{i,t}$ and $\eta_{j,t}$ are error terms.

Instead of assuming that the measurement error is classical, we will consider the possibility that the measurement error could be correlated with unobserved determinants of employee outcomes, e.g., that being on a good project could affect how an employee rates their manager, as well as whether that employee attrites. Hence, we make fewer assumptions than most empirical studies with measurement error. We still assume, though, that measurement error is uncorrelated with a manager's true people management skill:

Assumption 1 *$cov\left(m_j, u_{j,\tau}\right) = 0$ for $\tau \in \{1, 2\}$.*

While we do not expect Assumption 1 to be literally true (given that MOR is capped at 0 and 100), we believe that it is approximately true in our setting, particularly because managers are not selecting their own survey score.[9] For simplicity, we also assume that $var(u_{j,\tau}) = \sigma_u^2$ for all j and τ, i.e., the variance of the measurement error is the same across managers and periods.

We now compare OLS and IV estimators for this setting. For ease of exposition, we suppress i and j subscripts. For OLS, we use $\text{plim}(\widehat{b}_{OLS}) = \frac{cov(y_t, \widetilde{m}_\tau)}{var(\widetilde{m}_\tau)}$ plus Assumption 1 to get an equation for the inconsistency from OLS (derived in Appendix A.2):

$$\text{plim}(\widehat{b}_{OLS} - \beta) = \underbrace{-\frac{\sigma_u^2}{\sigma_m^2 + \sigma_u^2}\beta}_{\text{Attenuation Bias}} + \underbrace{\frac{cov(\varepsilon_t, u_\tau)}{\sigma_m^2 + \sigma_u^2}}_{\text{Contemp. Corr. ME}} + \underbrace{\frac{cov(\varepsilon_t, m)}{\sigma_m^2 + \sigma_u^2}}_{\text{Assignment Bias}} \tag{4}$$

In (4), the first term, *Attenuation Bias*, is standard under OLS with classical measurement error. In the second term, *Contemporaneously Correlated Measurement Error*, the numerator, $cov\left(\varepsilon_t, u_\tau\right)$, is the covariance between unobservables that affect worker outcomes and survey measurement error. We believe that such covariance is likely to be positive, but that is not necessarily the case. For example, one issue for analyzing attrition as an outcome is that there are individuals who quit before they get to take the survey. A manager may appear to have a better score on the survey than if departed workers were allowed to take part in the survey. In the third term, *Assignment Bias*, the numerator, $cov(\varepsilon_t, m)$, is the covariance of people management skills with worker outcome unobservables. This could be positive or negative.

Next, consider the IV estimator from (3). Note that different workers may evaluate the same manager during different periods. Using $\text{plim}(\widehat{b}_{IV}) = \frac{cov(y_t, \widetilde{m}_{-\tau})}{cov(\widetilde{m}_\tau, \widetilde{m}_{-\tau})}$, we get:

$$\text{plim}(\widehat{b}_{IV} - \beta) = \underbrace{-\frac{cov\left(u_\tau, u_{-\tau}\right)}{\sigma_m^2 + cov\left(u_\tau, u_{-\tau}\right)}\beta}_{\text{Attenuation Bias}} + \underbrace{\frac{cov\left(\varepsilon_t, u_{-\tau}\right)}{\sigma_m^2 + cov\left(u_\tau, u_{-\tau}\right)}}_{\text{Asynchronously Corr. ME}} + \underbrace{\frac{cov\left(\varepsilon_t, m\right)}{\sigma_m^2 + cov\left(u_\tau, u_{-\tau}\right)}}_{\text{Assignment Bias}} \tag{5}$$

In (5), the numerator of the first term has $cov\left(u_\tau, u_{-\tau}\right)$ in place of σ_u^2 in (4). Thus, if the measurement errors are uncorrelated across the two surveys, there is no attenuation bias. This assumption seems reasonable for certain types of measurement error, such as sampling

[9] People answering questions about themselves are often subject to desirability bias (Bound et al., 2001), but that is not the case here. A1 could still be violated if good or bad managers systematically engage in influence activities to boost their scores, or if there is peer pressure to systematically boost scores of good or bad managers. However, conversations with the firm indicate that influence activities are frowned upon and any such biases are likely to be minimal. Our analysis of measurement error draws heavily on Pischke (2007).

error due to small numbers of subjects or people being happy because the current project is going well. Other types of measurement error might be more persistent, e.g., there could be a persistently good long-term project or people on a manager's team have a general tendency to rate managers highly on surveys. As we discuss later, such correlations can be avoided by looking at managers who move across locations or job functions in the firm. In such circumstances, we would expect substantially less attenuation bias than in OLS.

The second term of (5) has $cov\,(\varepsilon_t, u_{-\tau})$ instead of $cov\,(\varepsilon_t, u_\tau)$ in the numerator. That is, it involves the covariance between unobserved determinants of performance in the current period and measurement error in the *other* period. For measurement error due to inattention or non-response, this correlation may be quite small or zero. For issues like being on a good project, this correlation may depend on how persistent the shock is over time.

The third term of (5) still has $cov\,(\varepsilon_t, m)$ in the numerator, but it is divided now by $\sigma_m^2 + cov\,(u_\tau, u_{-\tau})$ instead of $\sigma_m^2 + \sigma_u^2$. Thus, IV amplifies assignment bias if $cov\,(u_\tau, u_{-\tau}) < \sigma_u^2$.

We will also present reduced form results, i.e., OLS regressions of y_t on $\widetilde{m}_{-\tau}$:

$$\text{plim}(\widehat{b}_{RF} - \beta) = \underbrace{-\frac{\sigma_u^2}{\sigma_m^2 + \sigma_u^2}\beta}_{\text{Attenuation Bias}} + \underbrace{\frac{cov(\varepsilon_t, u_{-\tau})}{\sigma_m^2 + \sigma_u^2}}_{\text{Asynchronously Corr. ME}} + \underbrace{\frac{cov(\varepsilon_t, m)}{\sigma_m^2 + \sigma_u^2}}_{\text{Assignment Bias}}$$

Relative to the IV, a disadvantage of the reduced form is that there is still the same attenuation bias as for OLS. A potential advantage is that assignment bias is scaled by $\sigma_m^2 + \sigma_u^2$ in the denominator instead of $\sigma_m^2 + cov\,(u_\tau, u_{-\tau})$.

Except if noted otherwise, standard errors are clustered by manager in the empirical analysis, reflecting the main level of variation for our key regressor.

Comparison with studies on teachers. While teachers generally do not choose their students, managers at our high-tech firm play a critical role in selecting people for their team. Indeed, practitioners frequently argue that one of the most important parts of being a good people manager is selecting the right people (Harvard Management Update, 2008). Thus, randomly assigning employees to managers would not be informative of the overall impact of good people management skills since it would rule out better people managers selecting better people. Rather, differences in employee quality across managers might be viewed as a *mechanism* by which managers improve outcomes as opposed to a source of bias. A more informative hypothetical experiment for our setting, approximated in our design based on managers switching locations or job functions, would be to randomly assign managers to different parts of the firm and then observe employee outcomes. Still, even if we do not wish to rule out better managers selecting better people, we need to address the possibility of the firm optimally sorting managers and employees together ("assignment bias").

Control variables. Adding control variables helps address the possibility that MOR and employee outcomes may differ systematically. We control for the firm's different business units, as well as for job function (or occupation). We also control for year of hire, current year dummies, and a 5th order polynomial in employee tenure. We control for location, employee salary grade, and an employee's manager's span of control. In tests of coefficient stability (Oster, 2019), Section 4.5 shows that our results are very similar when using even finer controls, including various interaction terms and year-month dummies.

4 Manager Quality and Employee Attrition

Section 4.1 shows baseline results on MOR and attrition, followed by our three research designs: new joiners (Section 4.2); new joiners or employees switching managers (Section 4.3); and exploiting managers switching locations or job functions (Section 4.4). Exploiting different variation and addressing different threats to identification, all three designs provide complementary evidence that people management skill substantially reduces attrition. Section 4.5 addresses further threats to identification, assesses the quantitative importance of our results, and estimates manager VA.

4.1 Baseline Results

Panel A of Table 3 shows our baseline results. Column 1 shows a strong first stage ($F > 100$). In column 2, the OLS coefficient of -0.156 means that increasing MOR by 1σ is associated with a monthly reduction in attrition of 0.156 percentage points (hereafter "pp"), which is an 11% reduction relative to the mean of 1.37pp per month. In column 3, the IV coefficient is substantially larger at -0.475, implying that increasing MOR by 1σ corresponds to a 35% reduction in turnover. By the difference of two Sargan-Hansen statistics, we reject that the IV and OLS estimates are the same ($p < 0.01$). That the IV estimate is three times larger in magnitude than the OLS estimate is consistent with OLS being significantly biased downward due to attenuation bias from measurement error.

Our IV estimate implies that moving from a manager in the 10th percentile of MOR to one in the 90th percentile is associated with a turnover drop of roughly 60%.[10] As a reference point, Bloom et al. (2014) show that randomly assigning call-center employees to work from home reduces turnover by 50%.

However, not all attrition is the same. It could be that good managers prevent quits, but are willing to fire individuals who are not contributing. Thus, Panels B and C perform

[10] p10 of a standard normal is 1.28σ below the mean, corresponding to a monthly attrition rate of 1.374-1.28*(-.475)=1.98pp. p90 corresponds to 0.77pp monthly, a reduction of slightly more than 60%.

the same analyses separately for quits and fires.[11] We observe highly significant negative IV results for both quits and fires. The coefficient is larger in absolute magnitude for quits, but is larger in percentage terms for fires, reflecting that fires are rarer than quits. Also, there is a larger percentage relation between MOR and regretted quits (Panel D) than there is between MOR and non-regretted quits (Panel E), suggesting that MOR might help reduce unwanted, or "bad" quits (from the firm's perspective) more than it reduces "good" quits. Later, we will shed further light on "good" vs. "bad" attrition by using subjective performance scores.

That high-MOR managers reduce both quits and fires is consistent with people management skill positively impacting worker motivation. More motivated workers should be less interested in outside opportunities and less prone to behave in ways that result in fires.[12] That the coefficient is larger for regretted than non-regretted quits is consistent with complementarity between a worker's skills and their manager's people management skills, a point we return to in Section 5, particularly in the context of attrition result heterogeneity according to worker position in the hierarchy.

Figure 2 shows binned scatter plots for the reduced form regressions, showing a clear negative relationship for all five attrition variables, plus whether a worker changes manager.

4.2 New Workers Joining the Firm

Restricting our IV analysis to new workers who join the firm in the second period, Table 4 also finds a strong negative relation between a worker's manager's MOR and turnover (our sample size here is 8% of that in Table 3). This "joiners" analysis allows us to address a couple of concerns. First, in the joiners analysis, the survey responses of the workers under analysis do not influence the instrument because they are new to the firm.

Second, our analysis reduces concerns about assignment bias. When an employee joins a very large firm, they are unlikely to have substantial information about differences in people management skills across managers that would enable them to choose their managers. Further, as argued in Lazear et al. (2015) who use joiners to estimate manager VA, the firm is unlikely to have substantial information about the new worker separate from the hiring manager. This reduces the concern that ε_t is correlated with m separate from the possible role of better managers in selecting better employees for their team.

Results. Despite having much less statistical power than in the full sample, the relationship between MOR and attrition in Table 4 is qualitatively similar. Looking at all attrition

[11] Turnover is labeled as voluntary, involuntary, or missing/not assigned. We don't use missing turnover events in Panels B and C (they are included in Panel A). Involuntary exits or "fires" include both true fires and layoffs. The "fires" results become even stronger if months with layoffs are excluded.

[12] It could also reflect high-MOR managers attracting less attrition-prone workers, though Section 4.3 shows little evidence of observably better workers sorting to high-MOR managers when workers change managers.

in Panel A, the IV coefficient of -0.550 is slightly larger in magnitude than that in Panel A of Table 3, though it just misses statistical significance at 10%. The coefficient implies that a 1σ increase in MOR corresponds to a 0.55pp (38%) reduction in monthly turnover. We also see broadly similar results for other attrition variables, with particularly strong results for quits (especially regretted quits), for which the IV estimate is statistically significant.

4.3 Workers Joining the Firm or Changing Managers

Despite its clear benefits, the joiners analysis has limitations. First, new workers may be impacted differentially by MOR. Second, the sample size is small relative to our full sample. Third, it is difficult to perform certain statistical tests regarding assignment bias. In this subsection, instead of just analyzing workers joining the firm in the second period, we additionally add instances of workers switching managers during the second period. These switches occur for many reasons, such as new projects, manager turnover, promotions, and several re-organizations ("re-orgs") that occurred for exogenous reasons. This addresses the three limitations of the joiners analysis, and we continue to find a strong negative relation between manager MOR and employee turnover outcomes.

Our analysis here is useful for several reasons. First, the sample is broader than only new joiners—it is about 1/4 the size of that in Table 3. Second, we can do a test for non-random sorting of existing employees to managers, following Rothstein (2010, 2017)—we happen to find little evidence of systematic sorting of better existing employees to better people managers. Third, we can plot "event study" graphs analyzing how impacts of MOR on turnover vary with how long a person has been with a manager; such a graph would be harder to interpret in the pure joiners design, where time since manager is collinear with tenure. Fourth, analyzing what happens to employees after changes in manager is generally useful for reducing concern about assignment bias. While matching of managers and employees is not random, matching occurring for reasons such as manager turnover or re-orgs reflects less active involvement by the firm, particularly when there are many individuals being moved at the same time. Fifth, as in the joiners analysis, the workers in this design do not influence the instrument.

When workers switch manager during period two, 4% of the time this is accompanied by a promotion in the same month, 4% of the time they experience a change in job function, and 8% of the time they experience a change in business unit. Thus, most changes in manager are not from worker promotions, or from worker changes in job function or business unit.

Results. Table 5 reproduces Table 3 combining joiners (as in Table 4) and switchers. In Panel A, the IV coefficient for overall attrition is -0.332 (a 22% drop in attrition per 1σ in MOR). This is a bit smaller in magnitude than our baseline attrition estimate and misses conventional statistical significance ($p = 0.16$). In contrast, the coefficient for quitting (panel

B) is slightly larger in magnitude than the one in Table 3. The negative quit coefficient is driven by regretted quits, where the MOR coefficient is statistically significantly negative. Interestingly, the MOR coefficient for non-regretted quits is positive (though not statistically significant). Overall, the results suggest that MOR sharply reduces undesirable attrition, whereas MOR may even increase "good" attrition, though the results on non-regretted quits have large standard errors.

Time path of MOR impacts. Figure 3 takes the IV regressions in Table 5, but interacts MOR with the quarter since receiving a new manager. Event time starts at 0 after each manager change, thus accommodating workers who experience multiple manager changes in the second period.[13] As seen in panel (a), the relation between MOR and turnover is negative, but is small in magnitude during the first six months after a manager change. Rather, the turnover benefit builds gradually, with much of the reduction in turnover occurring 7-12 months after a manager change. Other attrition variables show similar patters, particularly for regretted quits. Figure C4 shows similar patterns using half-years instead of quarters.

Figure 3 seems consistent with a causal impact of MOR on attrition. If the effect were driven by assignment bias (e.g., the firm decides to match unobservably better workers with better managers), one might imagine that turnover impacts would be seen immediately. Instead, it may take some time for a worker to get to know and be affected by their manager.

Testing for assignment bias. A concern with the switchers analysis is that the firm may be matching unobservably high-quality managers and workers together. To test for this, we can examine whether the MOR of an employee's *future* manager predicts employee non-attrition outcomes in the current period, following Rothstein (2010, 2017). As detailed further in Appendix A.6, we implement an IV procedure where we instrument the future manager's MOR during period 2 using MOR during period 1. We cannot use attrition for the test because workers who will experience a new manager in the future do not attrite before then.

Table 6 examines the relation between a future manager's MOR and key non-attrition outcomes (subjective performance, salary, salary increases, and promotion propensity), as well as two other important worker characteristics (restricted stock units granted to an employee and whether the firm has designated a worker as a "key individual" whom they strongly want to retain). These variables are discussed further in Section 7 when we discuss rewards for managers. In the IV analyses in Panel B, we see little evidence that better people managers receive teams with observably better characteristics. This suggests that assignment bias from

[13] After joining the firm or changing manager in period 2, 17% of workers experience one additional change in manager, 3.5% an additional two changes, and 0.5% an additional three changes. Results in Table 5 and Figure 3 are robust to restricting to workers' first post-join/switch manager spell. Results in Table 4 are also robust (and slightly stronger) when restricted to joiners' first manager spell.

sorting strong people managers with unobservably better employees is likely limited.[14]

4.4 Manager Moves across Locations or Job Functions

Our third research design exploits managers moving across locations or job function. In line with Chetty et al. (2014), we collapse our data to the job function-location-period level (e.g., engineers at Location X in period 1), and examine the relation between average MOR and average attrition. This serves two key purposes. First, it provides further evidence that assignment bias does not drive our findings because by aggregating, we focus on differences in MOR at an aggregate level, as opposed to MOR differences within location-job function.

Second, it helps address concerns about asynchronously correlated measurement error from persistent unobservables. In our earlier joiners analysis, a persistent unobservable of a good project could lead to employees rating their manager favorably in period 1, as well as making new employees less likely to attrite in period 2. However, by aggregating up to the location-job function-period level, we no longer exploit variation from some engineers at a location working on a good project and some working on a bad project.

Implementation. Let $Q_{l,f,\tau,\tau'}$ be the (employee-month-weighted) mean normalized MOR of managers at location l in job function f during period τ, and for which we use the measurement of the managers' MOR taken during period τ'. Let $y_{l,f,\tau}$ be the mean quit rate of employees at location l in job function f during period τ. We estimate:

$$y_{l,f,\tau} = bQ_{l,f,\tau,\tau'} + \delta_l + \delta_f + \delta_\tau + \theta_{l,f,\tau} \tag{6}$$

where δ_l, δ_f, δ_τ are location, job function, and period fixed effects, respectively; and $\theta_{l,f,\tau}$ is the error term. Following Chetty et al. (2014), we weight observations by the number of employee-months per location-job function-period cell. While τ will vary based on the cell, all cells will use the same τ'. That is, we measure all managers using the same survey wave to help ensure that differences across cells reflect differences in manager quality as opposed to different measurements. We restrict attention to location-job functions that are in the data for both periods (results are slightly stronger if we don't). Standard errors are clustered by location-job function.

Similar to Chetty et al. (2014), our key identifying assumption is that changes in average

[14] Observed matching of employees and managers reflects not only potential assignment bias by the firm, but also selection of employees by managers. Thus, the null results of the Rothstein test are also consistent with high-MOR managers not selecting better existing employees for their teams (or, e.g., for negative assignment bias by the firm and positive selection of employees by managers given the firm's policies). An additional caveat is that our setting differs from other applications of the Rothstein test because we observe a limited relation of MOR to non-attrition outcomes later in Section 6. Overall, one might view the Rothstein test as less critical for the validity of our results (relative to other settings) given that managers selecting better workers for their team (separate from assignment bias) is a potential mechanism of people management skills.

location-function people management skills are uncorrelated with average location-function unobserved determinants of attrition, conditional on controls.[15] To control for possible changes in worker quality over the two periods (due to worker sorting or workers moving with their managers), we include controls for average location-function worker characteristics. While the key identifying assumption is difficult to test, there were no evident efforts by the firm (outside of autonomous decisions by workers and managers) to optimally sort workers and managers over time across location-job functions based on unobservables.

In our sample, 7% of managers experience a change in location, 9% a change in job function, and 14% a change in location or job function. In the month of a location change, the promotion rate is 2%, whereas in the month of a job function change, the promotion rate is 16%. Thus, a higher share of job function changes seem to occur from promotions compared to location changes. We suspect that location changes involve a combination of business reasons (e.g., moving to a close location because of business needs) and personal reasons.

Results. We obtain the same broad conclusion that people management skills reduce attrition, even though we are exploiting a different source of variation in MOR than in our baseline analyses. Columns 1-2 of Table 7 show OLS results, one measuring all managers in the sample using their wave 1 score, and the other measuring all managers using their wave 2 score. To account for attenuation bias due to measurement error, columns 3-4 show IV results, where we instrument the mean MOR in the location-job function-period cell using the mean MOR of the managers for that cell but measured during the other period.

As in our main results in Table 3, IV estimates are larger in magnitude than OLS. For the overall attrition results in Panel A, the IV estimates imply that a 1σ increase in a manager's MOR decreases employee attrition by 0.53-0.66pp per month, which is a bit larger in magnitude than our benchmark estimate in Panel A of Table 3, though our IV confidence intervals here overlap with those in Panel A of Table 3. Outside of Panel A, we have less power (and often lose statistical significance), but observe broadly similar results as before.

4.5 Additional Analyses

Categorizing workers by subjective performance scores. Beyond the earlier results on different types of attrition events, another way to analyze turnover is to look separately at "high" and "low" productivity workers based on subjective performance scores (Appendix Table C4). We residualize worker subjective performance on the Table 3 controls and regress the residuals on worker fixed effects. Fixed effects above the median are classified as high-

[15] For even greater control, one might wish to control for location-function fixed effects instead of location fixed effects and function fixed effects. However, we do not have enough power to do so. Instead, we control for a rich set of worker characteristics collapsed to location-job function means.

productivity workers. Column 1 analyzes overall attrition. As in many studies (e.g., Hoffman and Burks, 2017), high-productivity workers have lower attrition. While the IV coefficient is larger in absolute magnitude for low-productivity workers, it is slightly larger in percentage terms for high-productivity workers. MOR has stronger percentage associations with quits and regretted quits for high-productivity workers than low-productivity ones: A 10th to 90th percentile move in MOR reduces quits and regretted quits by roughly 50% among low-productivity workers, and by roughly 70% for high-productivity workers.

Switching managers. Beyond leading people to exit the firm, poor people management skills could also produce other types of "exits." Instead of quitting the firm, a worker may demand to be moved to a new manager. Appendix Table C5 repeats our analysis using whether a worker changes manager as the outcome (instead of attrition). The Panel A IV estimate implies that moving from a manager in the 10th percentile of MOR to one in the 90th percentile predicts a 45% reduction in the chance of switching managers.

Adding richer controls. A concern is that there is a persistent unobservable whose effect may not be alleviated by the above research designs (e.g., a manager is rated highly because they oversee a good project and continue overseeing the project when they move locations or functions). To proxy for such unobservables, we add further controls. Tables C8-C11 show that our attrition estimates are quite similar when adding additional controls. For the 4 sets of tests (sections 4.1-4.4), we add two-way interactions between business unit, job function, and salary grade, plus current month-year dummies (instead of baseline year dummies). To assess coefficient sensitivity, we use the Oster (2019) test. Given our IV set-up, we analyze the reduced form. As detailed in Appendix A.8, selection on unobservables would need to be many times larger than selection on observables to reverse the sign of the results.

Is people skills the cause? Another concern may be that something else about managers with good people management skills could be driving the results. Absent random assignment of particular skills to managers, we check that the impact of MOR remains strong as other manager characteristics are controlled for (Glover et al., 2017). While our data contain relatively few non-MOR manager characteristics, our main results are robust to controlling for them. For brevity, details are in Appendix A.9. Thus, the apparent effects of MOR do not seem due to any of the small number of observed managerial characteristics.

Quantitative importance. How much does the firm save each year in hiring costs due to lower attrition from a manager at the 90th percentile of MOR relative to one at the 10th percentile? Using the IV estimate from Panel A of Table 3 and assuming a hiring cost of 4 months of worker salary (Blatter et al., 2012), the savings is 5% of worker salaries for each worker on his or her team, totalling almost half a worker's salary per year when added up over the members of a typical 9-person team. Focusing on hiring costs likely provides a lower bound on total costs saved, both given the importance of turnover in high-tech for transmitting ideas and given that MOR particularly reduces turnover that is "bad" from the firm's perspective. Appendix A.10 provides further detail on these calculations.

VA approach. We also perform a "value-added" analysis of managers on attrition:

$$y_{it} = \alpha + \gamma_j + X_{it}\delta + \epsilon_{it} \tag{7}$$

where y_{it} is whether worker i attrites in month t; γ_j is a manager effect; and X_{it} are controls. The estimated standard deviation of VA may be biased upward if VA is estimated using finite observations per manager (Lazear et al., 2015). We address this two ways. First, we weight standard deviations by observations per manager. Second, following Silver (2016), we split the data in two and estimate (7) for separate samples. We do this splitting employee-months randomly in two or splitting by period. If sampling error is uncorrelated with underlying VA and across samples, the covariance of estimated VA across the two samples is equal to the variance of underlying VA.[16] Appendix Table C14 shows substantial variation in VA. In the split sample approach, the std. dev. of attrition VA is 0.67 (splitting randomly or by period). The consequence of improving attrition VA by 1σ (0.67pp per month) is 40% larger than the impact of improving underlying people skills by 1σ in Table 3 (0.48pp per month).

5 Heterogeneity

We examine variation in MOR by different dimensions of heterogeneity, namely, hierarchy position, geography, and worker occupation. We then analyze heterogeneity in terms of the relation of MOR to attrition. For brevity, the results are discussed here, with relevant tables and more information in Appendices A.11 and A.12.

The firm often segments employees into three groupings of hierarchy (low, medium, and high) according to their salary grade. The share of employee-months in low, medium, and high hierarchy jobs is 57%, 35%, and 8%, respectively.[17] A worker's hierarchy position can change when a worker gets promoted.

Explaining variation in MOR. Managers in engineering have 0.22σ lower MOR than managers in non-engineering job functions (consistent with stereotypes that social skills are

[16] A manager's underlying VA is γ. Let $\hat{\gamma_1} = \gamma + u_1$ and $\hat{\gamma_2} = \gamma + u_2$ be estimated VA in two split samples, where u_1 and u_2 are errors. Under the stated assumptions, $cov(\hat{\gamma_1}, \hat{\gamma_2}) = var(\gamma)$ (see also Silver (2016)).

[17] High hierarchy jobs include those such as Senior Director, Principal Design Engineer, and Distinguished Research Scientist (our sample does not include the very small share of people at the top of the firm hierarchy). Medium hierarchy jobs include those such as Senior Product Manager, Research Scientist, and Lead Designer. Within the engineering job function, the share of employee-months in low, medium, and high hierarchy jobs is 45%, 50%, and 5%, respectively. Outside of engineering, the shares are 64%, 26%, and 10%, respectively.

more scarce in engineers), and U.S. managers obtain higher scores than non-U.S. managers in creating a positive atmosphere and involving people in decisions. However, country, location, occupation, and firm rank explain only a modest portion of the variation in MOR scores, even after correcting for measurement error in MOR. About 90% of MOR variation is within-country and 80% of MOR variation is within-location. Appendix A.11 includes more details.

Heterogeneity in MOR effects. Appendix Tables C15-C17 show that the negative IV relation between MOR and the attrition variables is robust across hierarchy, geography, and occupation. Interestingly, however, the attrition results are significantly larger at higher levels of the firm hierarchy, and are suggestively larger in U.S. locations of the firm. For brevity, here in the main text, we focus primarily on heterogeneity by hierarchy.

Figure 4 compares the MOR-attrition relation for workers at lower positions in the hierarchy with that for workers at medium or high positions in the firm hierarchy. We perform an IV regression of different worker attrition outcomes on manager MOR in the current period and manager MOR in the current period interacted with whether the worker is currently at a medium or high position in the firm hierarchy. The instruments are manager MOR in the other period and manager MOR in the other period interacted with whether the worker is currently at a medium or high position in the firm hierarchy. For overall attrition, we observe that the MOR-attrition coefficient is substantially larger for workers at medium or high positions in the hierarchy compared to workers at low positions ($p = 0.04$ on the difference). This difference is particularly strong for quits ($p = 0.02$) instead of fires ($p = 0.36$).

Given that jobs higher in the hierarchy tend to involve higher cognitive skill than those lower in the hierarchy, our results point to complementarity between an employee's skills and *their manager's* people management skills. This mirrors the observed complementarity between employee cognitive skills and *employee* social skills (Weinberger, 2014; Deming, 2017).

These results seem sensible. Workers performing relatively less cognitively demanding tasks require managers to ensure that they are doing what they are supposed to, and people management skills help ensure that workers feel satisfied in doing the task at hand. However, in highly cognitively demanding jobs, there is frequently not a single answer to problems. It may not even be clear what problems an employee should be working on. A manager's people management skills may play an important role in helping employees feel challenged and supported in more ambiguous environments. Those in more cognitively demanding jobs may also be more accustomed to having positive and supportive relationships with their managers.

Other explanations are also possible, but seem less likely. One possibility is that the results reflect that employees who are higher in the hierarchy tend to have higher tenure than workers lower in the hierarchy. However, the findings are very similar (and slightly stronger) when controlling for the interaction of MOR and a dummy for the worker having above median tenure, as seen in panel (a) of Appendix Figure C5. Another possibility is that the results reflect that employees who are higher in the hierarchy tend to be managers themselves, and people management skills could have larger impacts on workers who are also managers. The chance of an employee being a manager is 4%, 35%, and 85%, when the employee is at a low, medium, or high position in the firm's hierarchy, respectively.[18] However, Figure 4 is similar when excluding employees who are managers themselves, as seen in panel (b) of Appendix Figure C5. A further explanation is that the composition of engineers/non-engineers differs by position in the hierarchy (e.g., the share of non-engineers is 71% for workers low in the hierarchy compared to 54% for workers medium or high in the hierarchy), but Figure 4 is similar when controlling for the interaction of MOR and a dummy for the worker being an engineer, as seen in panel (c) of Appendix Figure C5.[19]

On geography, Appendix Table C16 provides some evidence that attrition results are stronger for U.S. workers compared to abroad. In our firm, workers abroad have similar aggregate positions in the firm hierarchy relative to U.S. workers. Still, our general sense is that tasks done by U.S. workers often tend to involve greater skill than those done overseas, consistent with complementarity between employee skills and manager people management skills. In addition, research on global management (Hofstede, 2001; Bloom et al., 2012) emphasizes that there are key differences across countries and cultures in attitudes toward authority and managers. For example, it could be that the survey uncovers people management skills that are social and interpersonal, as opposed to distant and authoritarian, and that U.S. workers react more positively to the former style than workers in other countries.[20]

6 Manager Quality and Non-attrition Outcomes

This section shows that manager MOR does not have a consistent positive relation to most non-attrition outcomes of subordinate employees. Baseline IV results show a moderate positive

[18] As we believe is common at other high-tech firms, there are some individuals who are high up in the firm's hierarchy, but who do not manage other people; for example, some engineers specialize in addressing difficult engineering problems, but do not have direct reports.

[19] Another explanation is that being higher up in the hierarchy reflects geography, as we also tend to see some evidence that MOR coefficients are larger for U.S.-based workers (compared to workers abroad). However, Figure 4 is robust to restricting attention to U.S.-based workers (panel (d) of Figure C5). Last, results are similar when controlling for the interaction of MOR and whether a worker is co-located with their manager.

[20] There are also alternative explanations. First, the result could additionally reflect complementarity with some other feature of the U.S. Second, U.S. workers have a higher rate of being co-located with their manager than foreign workers (83% to 76%), but results are similar if one controls for MOR x (whether a worker is co-located with their manager). Third, MOR is based on surveys in English—though a simple explanation is not immediate (as we use IV to address measurement error), MOR might seem to matter less when questions are not in workers' native language. However, results from Panels D and E of Table C16 (where we see statistical significance) are robust to restricting to English-speaking countries. These explanations should not be over-interpreted, as we regard our geographic heterogeneity findings as more suggestive.

relation between MOR and subjective performance, but this relation is not consistently robust to our research designs.

Baseline results. Column 1 of Table 8 shows that MOR appears to have only a modest positive relation to employee performance as measured by normalized subjective performance scores. A 1σ increase in MOR is associated with a 0.05σ increase in employee subjective performance under OLS, and a 0.09σ increase under IV. As for the earlier attrition results, OLS is likely biased downward due to attenuation bias.

In column 2, the outcome is the increase in log salary between now and 12 months into the future. A 1σ increase in MOR predicts a 0.12% increase in worker salary in OLS and a 0.06% increase in IV, both insignificant. The mean salary increase per year is confidential, but is between 4% and 8%. With 95% confidence, our IV coefficients rule out that a 1σ increase in MOR predicts an additional annual raise of more than 0.47%.[21]

Column 3 of Table 8 examines employee promotions. Panel A shows an insignificant positive relation in the OLS that turns negative in the IV in Panel B. The top of the IV 95% confidence interval is 0.24. With a mean monthly promotion rate in our sample of 1.5pp, we can rule out that a 1σ increase in MOR would increase the promotion probability by about 15%. Recall that we find 1σ decreases attrition by 28%, so the top of the confidence interval for promotion is about half as large.

Column 4 shows no relation between MOR and patent applications. The mean patent rate in-sample is roughly 0.002 patents per worker-month, or about 3 times the size of the IV standard error. To address the fact that many patents are not valuable, column 5 analyzes citation-weighted patents, defined as a worker's patent applications in a month, plus log(1+citations to those patents), and we still observe no relation. For citation-weighted patents, the mean rate is also about 3 times the IV standard error. To focus on new inventions (as opposed to revisions of past applications), we conservatively restrict attention in Table 8 to patent applications where the priority date equals the application date. If we don't make this restriction, our precision increases, with a coefficient very close to 0 and an IV standard error 4 times smaller than the mean patent rate.

Research designs. Appendix Table C18 performs our different research designs for non-attrition outcomes. While the different designs provide consistent evidence that MOR reduces attrition, they do not support that MOR affects non-attrition outcomes.

Differential attrition. That MOR reduces turnover could potentially bias estimation for non-attrition outcomes. To address this, we repeated our analysis in Table 8 while re-

[21] A null result also occurs if the outcome is log salary or log restricted stock holdings. These results suggest that MOR's impact on attrition is unlikely to be driven by workers of high-MOR managers receiving greater pay. We also examined whether subordinates of high-MOR managers have pay respond more to subjective performance (contemporaneous or average) than subordinates of low-MOR managers, and saw no difference.

stricting to worker-months where a worker's manager experiences zero subordinate attrition events in the analysis sample (Panel A) or no more than 1 or 2 of such events (Panels B and C). Our conclusions are substantively unchanged in such analyses.

Why do we see large impacts of people management on attrition, but not on other outcomes? There are several possibilities. First, good people management may matter most for attrition, which may reflect whether an employee feels respected and motivated. People management skills may matter less for subjective performance, salary growth, promotions, or patents, for which technical talent and knowledge may be more important. Second, it may be easier for a manager to reduce attrition (e.g., by making someone feel respected and motivated), but harder to affect subjective performance. Third, it could be that certain outcomes take more time and interaction to be affected, and that our data's time-frame is too short to observe such effects. It is hard to distinguish these possibilities in our data.

While we do not see positive results for most non-attrition outcomes, we also do not see negative ones. If MOR reduced attrition via giving employees lower workload or other amenities that were against the firm's interests, this would likely show up in negative non-attrition results, but the results do not support such an interpretation.

7 How does the Firm Reward Good Managers?

We now examine whether MOR is "rewarded" by the firm in terms of how it evaluates, compensates, and promotes its managers. In large high-skill firms such as the one we study, the concept of *reward* is complex and multi-faceted. Individuals can be rewarded through promotions, salary increases, or stock grants. The firm could also respond by changing span of control, e.g., so that better people managers get to manage more people (Garicano, 2000). We estimate regressions similar to those in Section 3 except the dependent variable is manager rewards instead of employee outcomes. For OLS, this would be:

$$R_{j,t} = b\widetilde{m}_{j,\tau(t)} + \theta_{j,t} \tag{8}$$

where $R_{j,t}$ is a reward (e.g., subjective performance score, or stock grants) achieved by manager j in month t. We include the same controls as for our analysis of worker outcomes. Relevant robustness checks and calculation details are provided in Appendix A.14.

Manager subjective performance. Subjective performance is a critical measure of reward at our firm, as the subjective score is a key determinant of financial rewards. As seen in column 1 of Table 9, a 1σ increase in MOR predicts a 0.09σ increase in subjective performance in OLS, but a 0.40σ increase in subjective performance in IV. The IV estimate is substantial, both statistically and economically. OLS is likely attenuated due to measurement error.

Promotions. Column 2 of Table 9 shows that a 1σ increase in MOR predicts a 0.67pp increase in promotion probability each month. Given the average monthly promotion rate of roughly 1.5pp, a manager at the 90th percentile of MOR is over 3 times more likely to be promoted than one at the 10th percentile. This suggests that the firm promotes good people managers to higher levels of the firm where they may have greater impact, consistent with the complementarity results shown in Section 5.

Compensation. A 1σ increase in MOR is associated with a 1.4% larger increase in salary over a 12-month period. The average annual increase in salary for managers is confidential, but is between 4-8%, so the estimate is economically meaningful. Using 1-month increase in log salary, there is also a significantly positive IV relationship. Another means of compensation, particularly in high-tech firms, is restricted stock grants, which are given to reward and retain valued employees. Table 9 shows that there is no relation between MOR and the level of an employee's stock grant holdings, or between MOR and disbursement of new stock grants. Thus, higher people management skills predict high salary growth, but not stock grants.

We also analyzed the level of salary (instead of increases in salary) and saw no significant relation with MOR. We do not combine salary and stock grants because stock grants take time to vest and are hard to value. The firm also pays non-stock cash bonuses, but much of these are based on firm performance, and are not included in our data.

Span of control. Column 6 shows that an increase in MOR by 1σ predicts an increase in span of control by 0.3 individuals, but it is not statistically significant (though the standard error of 0.3 also seems relatively large).

Key individual designation. Individuals at the firm who are believed to be especially important can be designated by the firm as "key individuals." The designation is not permanent. A 1σ increase in MOR predicts a 2.1pp increase in the probability of being designated a key individual, though the relation is not statistically significant. Roughly 20% of managers are key individuals.

Manager VA as a regressor. Appendix Table C23 shows that attrition VA is not a consistent predictor of rewards. We use split sample IV to address sampling error in VA. We also observe that our MOR reward results from Table 9 are qualitatively robust to controlling for attrition VA, i.e., a manager with higher MOR is more likely to receive rewards, even controlling for the manager's ability to retain workers. This suggests, as one might expect, that the firm values MOR beyond its impact on attrition.

Discussion. Table 9 shows that better people managers receive significant rewards from the firm in some important dimensions. Even though the firm has a strong engineering culture and values technical skills, it still rewards people management skills. One important caveat regarding the reward results is that we only observe a relatively short panel. Given that subjective performance and promotions are often the gateway to future rewards, we speculate that MOR might be rewarded to an even greater extent in the longer-run than seen here.

To get a sense of the long-run importance of the estimates, note that increasing MOR from the 10th to 90th percentile increases the annual chance of promotion by 21pp. Thus, after a 10-year period, a manager in the 90th percentile of MOR will have experienced two more promotions on average than a manager in the 10th percentile, which represents a large difference in the firm's hierarchy.

Another question is, for high MOR managers, how much cost does the firm incur in higher manager salaries relative to the benefits of lower worker turnover? Using (1) that managers at p90 of MOR receive an additional 3.6% salary raise each year relative to managers at p10 of MOR, (2) that average manager duration at the firm is roughly 6 years, and (3) that managers earn roughly 50% more than their workers, we estimate that the firm pays out roughly $0.27 in higher salaries for each $1 in benefit from lower turnover. Hence, the extra pay that high MOR managers receive are well worth the return to the firm from lower attrition of employees.

8 Conclusion

Managers are at the heart of organizations, but measuring what managers do is challenging. An approach taken in studying CEOs and managers in lower-skill firms has been to calculate a manager's VA using performance metrics. However, such an approach is hard to take to knowledge-based firms and other firm contexts where objectively measuring productivity is challenging. We pursue an alternative approach using employee surveys. Employee surveys also help us address a more refined research question. VA papers answer: how much do managers matter overall? We answer: how much do *people management skills*, or interpersonal skills for dealing with one's subordinates, matter? Upward feedback surveys are used by many firms, but we have little hard evidence on the importance of people management skills.

We find a strong, positive relationship between people management skills and employee retention, a critical outcome in high-skill firms. Results are particularly strong for attrition that is "bad" from the firm's perspective. A causal interpretation is strengthened using several complementary research designs. The results imply that replacing a manager at the 10th percentile of people management skills with one at the 90th percentile reduces the total subordinate labor costs by 5% solely from lower hiring costs due to less attrition. Moreover, managers with better people management skills receive higher subjective performance scores, are more likely to be promoted, and receive larger salary increases, consistent with the firm attaching significant value to these skills. Interestingly, we find little relationship between people management skills and most observed non-attrition employee outcomes, though this could occur because many aspects of worker behavior are not observed or because the time-frame of our dataset is too short.

While our conclusions are specific to one firm, our main findings on the importance of people management skills are robust across different hierarchy levels, geographies, and occupations within the large, multinational firm we study. This strengthens the case for external validity, and suggests that our conclusions may hold in other contexts. People management skills are particularly important for attrition in higher-level jobs and (more suggestively) for U.S. workers. These results, together with people management skills mattering more for reducing "bad" attrition, suggest complementarity between a manager's people management skills and a worker's cognitive skills (provided that cognitive skill is higher for high-level jobs, U.S. jobs, and workers where the firm regards attrition as bad instead of good). Methodologically, we illustrate the value of analyzing personnel data from a large firm, giving the researcher significant information and detail about the type of jobs that workers are doing, while still accessing a large sample of workers across various dimensions of heterogeneity.

Our results help open the black box of managerial production. One open question is, what are the precise managerial behaviors by which people management skills matter? While unavailable to us, email or calendar data might further elucidate manager behavior. Also, what interventions (if any) can improve people management skills in high-skill workplaces? Future work can help further open the black box by answering these questions.

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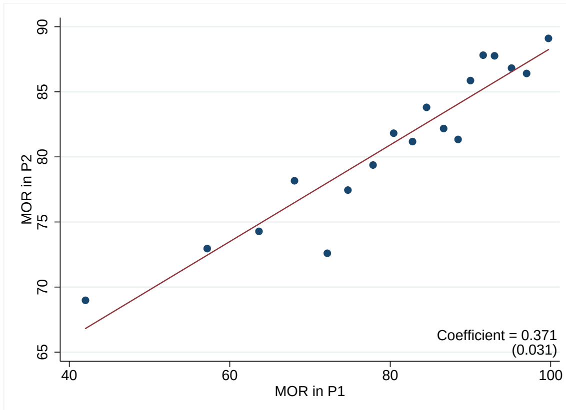
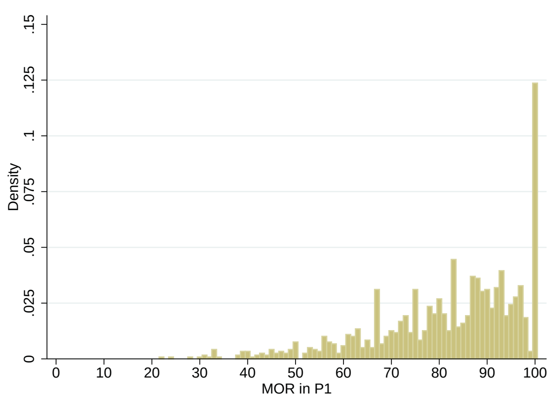
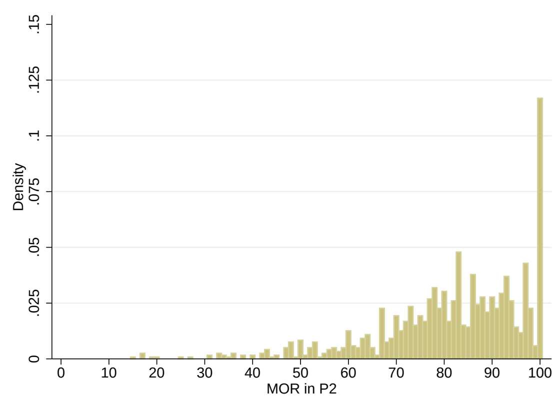
Figure 1: Manager Overall Rating (MOR): Correlation across Surveys and Histograms



(a) Correlation of MOR across Two Surveys



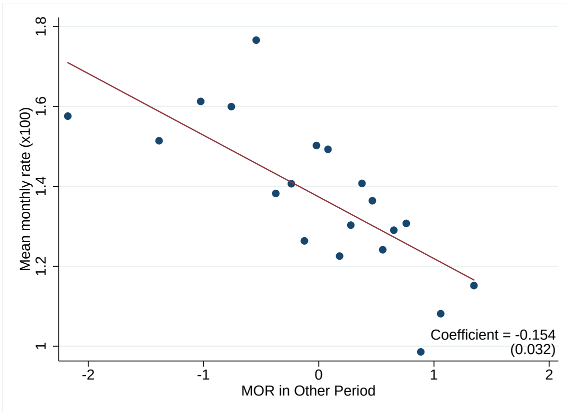
(b) MOR in Period 1



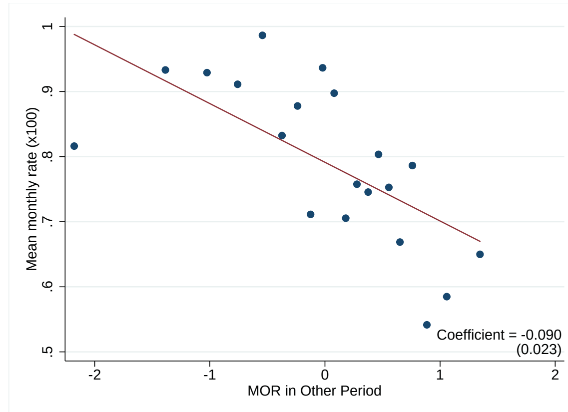
(c) MOR in Period 2

Notes: Panel (a) presents a binned scatter plot of MOR in period 2 on MOR in period 1 with no control variables. We use "binscatter" in Stata. An observation is a manager. In the lower-right of the figure, we list the regression coefficient (with a robust standard error in parentheses) for a manager-level regression of MOR in period 2 on MOR in period 1. Panels (b) and (c) present histograms of MOR. In each period, 12% of managers receive the highest score of 100. As discussed in Section 4.5, our main results are robust to using MOR in quintiles or percentiles, or to excluding cases where MOR equals 100. We restrict attention in this figure to managers who also appear in our cleaned data (i.e., after dropping duplicates, customer service workers, and months in April-May Y_3, but before requiring workers to have MOR non-missing in both periods) as workers.

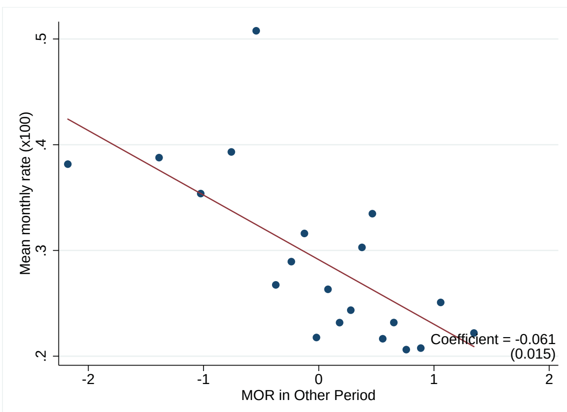
Figure 2: Reduced Form Binned Scatter Plots: Regressing Attrition Variables on Current Manager MOR in Other Period



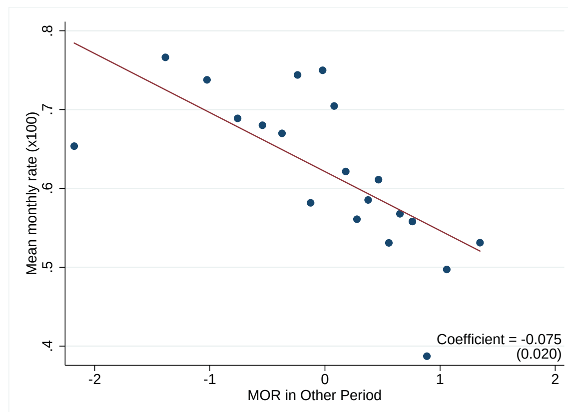
(a) Attrition



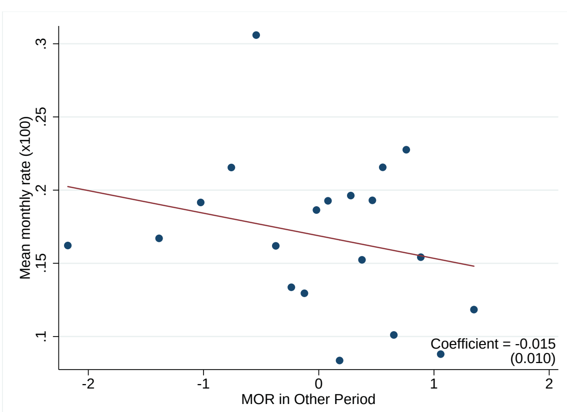
(b) Quits



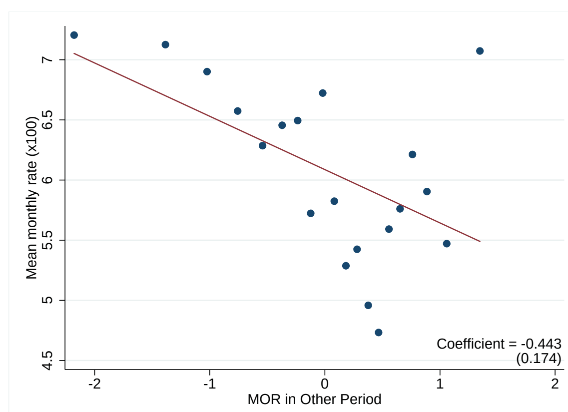
(c) Fires



(d) Regretted Quits



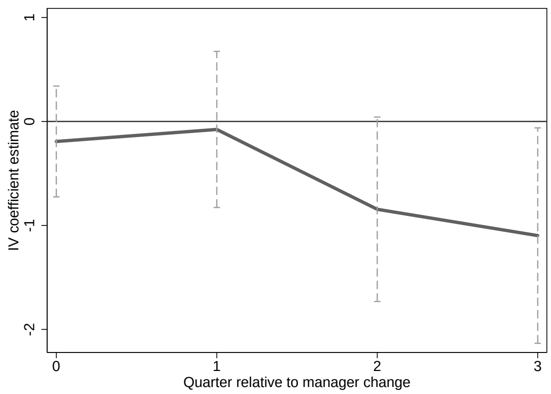
(e) Non-regretted Quits



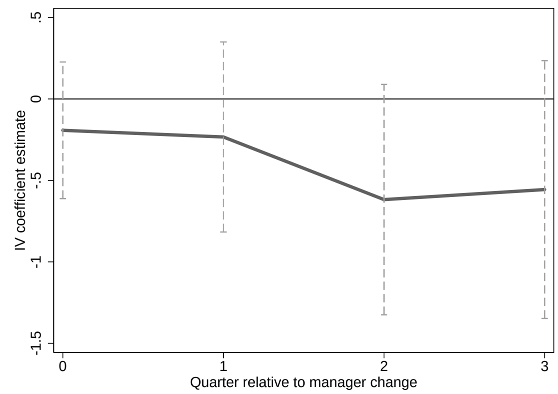
(f) Worker changes manager

Notes: This figure presents binned scatter plots corresponding to the reduced form regressions in Table 3. We use "binscatter" in Stata with 20 bins. Controls are the same as in Table 3.

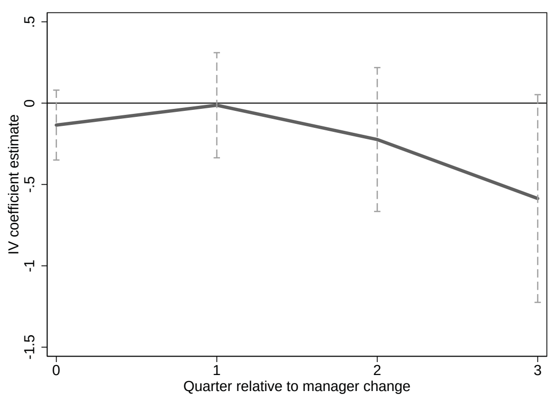
Figure 3: Impacts of MOR on Attrition Outcomes by Quarter Since Getting a New Manager



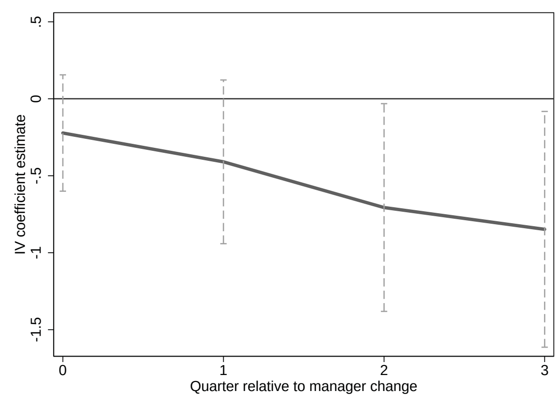
(a) Attrition



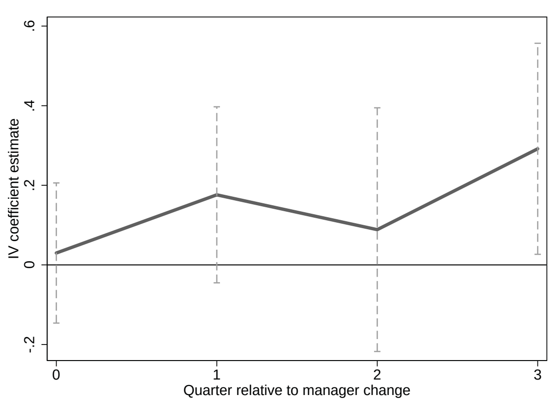
(b) Quits



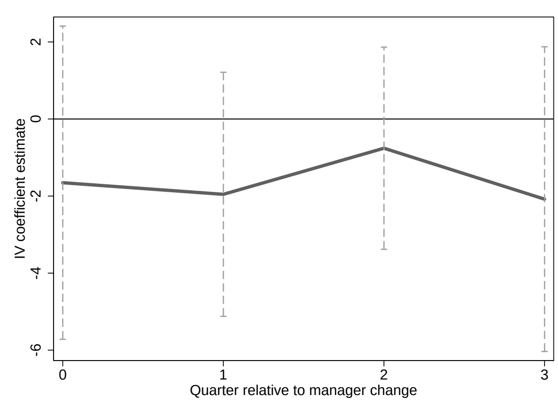
(c) Fires



(d) Regretted Quits



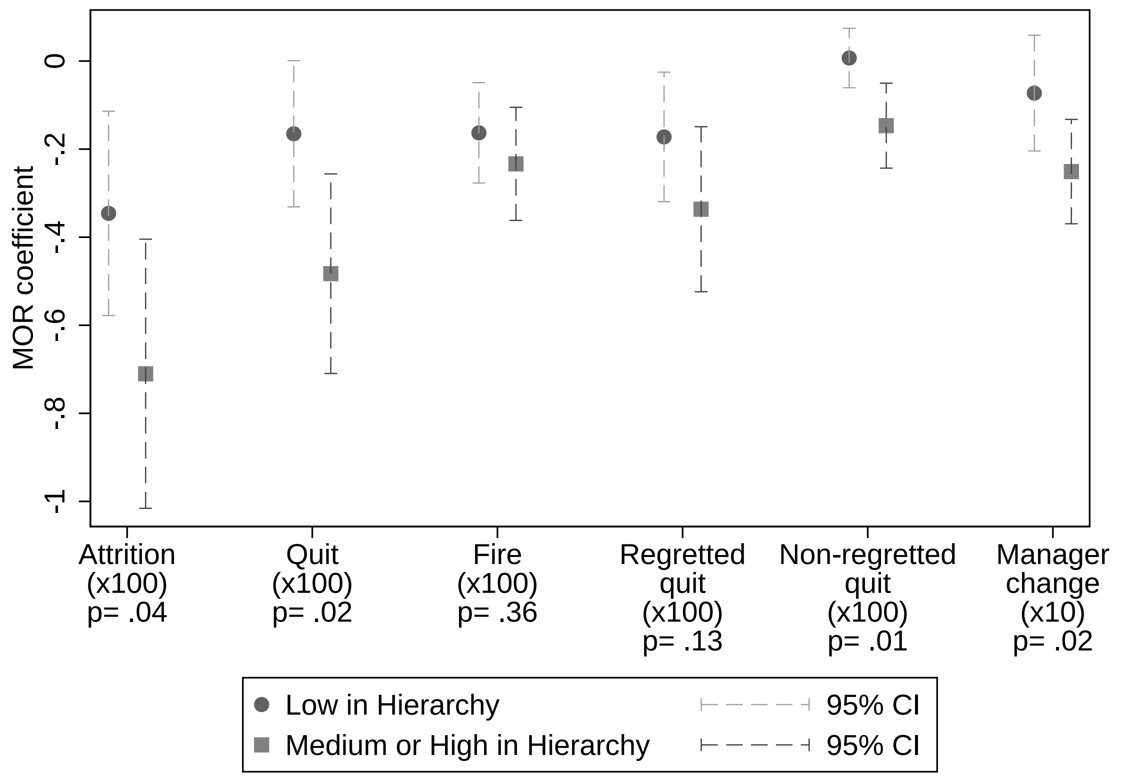
(e) Non-regretted Quits



(f) Worker changes manager

Notes: The dotted lines show 95% confidence intervals on coefficients, with standard errors clustered by manager. This figure comes from an IV regression similar to that in Table 5, with one main difference. The main difference is that instead of using MOR, we use MOR interacted with quarters since getting a new manager. In addition, we include dummies for the quarter since getting a new manager. "Quarter 0" includes the month during which a worker gets a new manager, followed by the two months after (i.e., months 2 and 3). "Quarter 3" includes months 10, 11, and 12. Beyond quarters 0-3 shown here, we also include a single dummy for being in quarters 4 or 5 (this is a small bin, including about 5% of observations in the analysis, whereas the other bins each include about 10% or more). Both current period MOR (the regressor) and other period MOR (the instrument) are interacted with quarters since getting a new manager.

Figure 4: Heterogeneity in the MOR-Attrition Results by a Worker's Position in the Firm Hierarchy



Notes: This figure shows how the relationship between normalized manager MOR and worker attrition varies by a worker's position in the firm hierarchy. We show results from an IV regression of different worker attrition outcomes on manager MOR in the current period and manager MOR in the current period interacted with whether the worker is currently at a medium or high position in the firm hierarchy. The instruments are manager MOR in the other period and manager MOR in the other period interacted with whether the worker is currently at a medium or high position in the firm hierarchy. The full specification appears in Panel E of Appendix Table C15. In the figure here, the coefficients on "Low in Hierarchy" correspond to those on MOR, whereas the coefficient on "Medium or High in Hierarchy" correspond to the sum of the coefficients on MOR and MOR x (Medium or High in Hierarchy). The standard error for "Medium or High in Hierarchy" is computed in Stata using "lincom" (i.e., using the Delta Method). The 6 p-values shown are tests of whether the coefficient on MOR x (Medium or High in Hierarchy) equals 0, i.e., whether there is a differential effect of MOR by hierarchy. The whiskers show 95% confidence intervals, with standard errors clustered by manager.

Table 1: Summary Statistics

Panel A: Overall numbers

Share of records, employee in US	0.70
Share of records from managers	0.21
Share of records for engineers	0.36
Worker is co-located with their manager	0.81
Worker has same function as their manager	0.86
Average manager span (employees/mgr)	9.35
Managers per employee in the sample	1.39
Managers per employee (weighted by tenure)	1.52
Worker was hired during the sample period	0.29
Low level in the firm hierarchy	0.57
Medium level in the firm hierarchy	0.35
High level in the firm hierarchy	0.08

Panel B: Summary statistics for outcomes and regressors

Variable:	mean	sd	min	max
Attrition probability (monthly) x100	1.37	11.64	0	100
Quit probability (monthly) x100	0.79	8.86	0	100
Fire probability (monthly) x100	0.29	5.39	0	100
Regretted quit prob (monthly) x100	0.62	7.86	0	100
Non-regretted quit prob (monthly) x100	0.17	4.11	0	100
Subjective performance rating	3.32	0.81	1	5
Log salary	Confidential			
Promotion probability (monthly)	Confidential			
Patents (monthly)	Confidential			
Manager overall rating (MOR)	81	15	15	100
Manager gives clear expectations	84	16	0	100
Manager provides coaching	75	21	0	100
Manager supports career dev	77	19	0	100
Manager involves people in decisions	84	17	0	100
Manager instills positive attitude	83	18	0	100
Manager is someone I trust	83	17	0	100

Notes: This table presents important summary statistics regarding our analysis sample. The data are at the employee-month level. While exact sample size cannot be shown to preserve firm confidentiality, our sample contains well over 1,000 managers, 10,000 workers, and 100,000 worker-months. Observation counts vary slightly by variable, reflecting that our dataset is created by linking multiple firm data files. We also cannot disclose the exact time frame of the sample, but the sample is for a 27-month period between January Y_1 and March Y_3 in 2011-2015. Thus, Y_1 corresponds to 2011, 2012, or 2013, but we cannot disclose which year it is. In Panel A, "Share of records, employee in US" refers to the share of employee-months in the dataset where the employee is working at a US location. "Co-located with manager" refers to the share of employee-months where the employee and manager are working at the same location. For several non-attrition outcomes, we cannot give exact summary statistics. However, as part of Sections 6 and 7, we provide approximate mean values in-text as appropriate to help interpret the regression coefficients. The overall attrition probability is greater than the sum of the quit and fire probabilities because there are a number of exits which are not classified in the data as voluntary or involuntary.

Table 2: Managerial Characteristics are Persistent: Predicting Manager Ratings on Different Dimensions in the Y_2 Survey using Ratings from the Y_1 Survey

Dep. Variables:	(1) Overall MOR	(2) Clear expectations	(3) Coaching	(4) Career dev	(5) Involves people in decisions	(6) Positive attitude	(7) Someone I trust
Characteristic in Y_1	0.37***	0.25***	0.29***	0.31***	0.29***	0.43***	0.35***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
R-squared	0.23	0.18	0.21	0.22	0.18	0.25	0.20

Notes: Robust standard errors in parentheses. An observation is a manager. Each column regresses a normalized managerial score variable in Y_2 on the same variable in Y_1. For example, column 1 regresses a manager's overall rating (MOR) in Y_2 on a manager's MOR in Y_1 as well as control variables. The sample is restricted to managers for whom we have manager scores for both waves of the employee surveys. We include control variables corresponding to a manager's first observation in the data as a manager. All regressions include business unit dummies, job function dummies (8 categories), salary grade dummies, dummies for year of hire (observations before 2001 are lumped in one category), and location dummies. Locations with less than 2,000 employee-months are lumped into a separate location category, and we also include a separate dummy variable for a location being in the US. The questions from the survey are listed in the main text in Section 2.1. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: MOR and Employee Attrition: Baseline Results

Specification:	1st Stg	OLS	IV	Reduced Form
Panel A: Attrition				
MOR in other period	0.325***			-0.154***
	(0.029)			(0.032)
MOR in current period		-0.156***	-0.475***	
		(0.031)	(0.103)	
Mean dep. var.		1.374	1.374	1.374
F-stat on excl instrument			124.6	
Panel B: Quits				
MOR in other period	0.325***			-0.090***
	(0.029)			(0.023)
MOR in current period		-0.103***	-0.278***	
		(0.023)	(0.074)	
Mean dep. var.		0.791	0.791	0.791
F-stat on excl instrument			124.6	
Panel C: Fires				
MOR in other period	0.325***			-0.061***
	(0.029)			(0.015)
MOR in current period		-0.033**	-0.188***	
		(0.014)	(0.048)	
Mean dep. var.		0.291	0.291	0.291
F-stat on excl instrument			124.6	
Panel D: Regretted Quits				
MOR in other period	0.325***			-0.075***
	(0.029)			(0.020)
MOR in current period		-0.084***	-0.230***	
		(0.021)	(0.065)	
Mean dep. var.		0.621	0.621	0.621
F-stat on excl instrument			124.6	
Panel E: Non-regretted Quits				
MOR in other period	0.325***			-0.015
	(0.029)			(0.010)
MOR in current period		-0.019**	-0.048	
		(0.010)	(0.030)	
Mean dep. var.		0.169	0.169	0.169
F-stat on excl instrument			124.6	

Notes: Standard errors clustered by manager in parentheses. An observation is an employee-month. In Panel A, the dependent variable is a dummy for whether an employee attrites in a given month. In the other panels, the dependent variable is a dummy for whether an employee experiences a particular type of attrition event in a given month. In all regressions, the dependent variable is multiplied by 100 for readability. All regressions include the same controls as in Table 2, plus current year dummies, the span of control for an employee's manager (plus a dummy for span being missing), and a 5th order polynomial in employee tenure. Also, unlike Table 2, the controls are over time instead of for one month. While exact sample size cannot be shown to preserve firm confidentiality, our sample contains well over 1,000 managers, 10,000 workers, and 100,000 worker-months. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: MOR and Employee Attrition: Exploiting New Joiners

Specification:	1st Stg	OLS	IV	Reduced Form
Panel A: Attrition				
MOR in other period	0.296***			-0.163
	(0.043)			(0.114)
MOR in current period		-0.252**	-0.550	
		(0.100)	(0.370)	
Mean dep. var.		1.446	1.446	1.446
F-stat on excl instrument			47.19	
Panel B: Quits				
MOR in other period	0.296***			-0.190**
	(0.043)			(0.093)
MOR in current period		-0.212**	-0.643**	
		(0.086)	(0.308)	
Mean dep. var.		0.864	0.864	0.864
F-stat on excl instrument			47.19	
Panel C: Fires				
MOR in other period	0.296***			-0.052
	(0.043)			(0.054)
MOR in current period		-0.028	-0.175	
		(0.045)	(0.180)	
Mean dep. var.		0.362	0.362	0.362
F-stat on excl instrument			47.19	
Panel D: Regretted Quits				
MOR in other period	0.296***			-0.181**
	(0.043)			(0.088)
MOR in current period		-0.190**	-0.613**	
		(0.083)	(0.292)	
Mean dep. var.		0.798	0.798	0.798
F-stat on excl instrument			47.19	
Panel E: Non-regretted Quits				
MOR in other period	0.296***			-0.016
	(0.043)			(0.017)
MOR in current period		-0.024*	-0.053	
		(0.015)	(0.057)	
Mean dep. var.		0.0603	0.0603	0.0603
F-stat on excl instrument			47.19	

Notes: This table is similar to Table 3, but restricts to new employees joining the firm after the administration of the second survey (i.e., during period 2). The sample size is 8% of that in Table 3. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: MOR and Employee Attrition: Exploiting New Joiners and People Switching Managers

Specification:	1st Stg	OLS	IV	Reduced Form
Panel A: Attrition				
MOR in other period	0.280***			-0.093
	(0.037)			(0.069)
MOR in current period		-0.157***	-0.332	
		(0.061)	(0.236)	
Mean dep. var.		1.512	1.512	1.512
F-stat on excl instrument			57.94	
Panel B: Quits				
MOR in other period	0.280***			-0.086
	(0.037)			(0.053)
MOR in current period		-0.147***	-0.308*	
		(0.047)	(0.185)	
Mean dep. var.		0.880	0.880	0.880
F-stat on excl instrument			57.94	
Panel C: Fires				
MOR in other period	0.280***			-0.043
	(0.037)			(0.028)
MOR in current period		-0.015	-0.153	
		(0.024)	(0.097)	
Mean dep. var.		0.327	0.327	0.327
F-stat on excl instrument			57.94	
Panel D: Regretted Quits				
MOR in other period	0.280***			-0.116**
	(0.037)			(0.047)
MOR in current period		-0.127***	-0.412**	
		(0.045)	(0.166)	
Mean dep. var.		0.722	0.722	0.722
F-stat on excl instrument			57.94	
Panel E: Non-regretted Quits				
MOR in other period	0.280***			0.029
	(0.037)			(0.019)
MOR in current period		-0.020	0.105	
		(0.015)	(0.073)	
Mean dep. var.		0.158	0.158	0.158
F-stat on excl instrument			57.94	

Notes: This table is similar to Table 3, but restricts to new employees joining the firm after the administration of the second survey (i.e., during period 2) or to observations following a change in manager during the second period (more precisely, to observations where a worker's manager differs from the manager they had during September Y_1 when the first survey was administered). The sample size is about 1/4 the size of that in Table 3. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Testing for Assignment Bias: Predicting Employe Outcomes Before Manager Switch as a Function of MOR of Future Manager

Dep. Var.	Subjective performance (normalized)	Log salary x100	Log salary growth x100	Promoted x100	Log stock grant holdings x100	Key individual (x100)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
MOR of future manager	0.028	-0.776**	0.294*	0.170	2.326	0.785
measured in 2nd period	(0.026)	(0.394)	(0.155)	(0.149)	(1.589)	(0.640)
Panel B: IV						
MOR of future manager	0.041	-0.574	0.795	0.176	8.495	-0.450
measured in 2nd period	(0.106)	(1.209)	(0.567)	(0.471)	(9.357)	(1.694)
F-stat on excl instrument	25.19	30.08	29.22	28.10	9.29	28.10
Panel C: Red. Form						
MOR of future manager	0.011	-0.178	0.243	0.049	1.571	-0.126
measured in 1st period	(0.029)	(0.378)	(0.175)	(0.134)	(1.667)	(0.476)

Notes: Standard errors clustered by future manager in parentheses. The controls are the same as in Table 3. The table presents regressions of employee outcomes at the start of the sample as a function of the MOR of the employee's new manager. The sample is restricted to switchers from the Table 5 sample (i.e., people switching managers in the second period and no new joiners) and restricts attention to the new manager after a worker's first change in manager during the second period. An observation is an employee-month occurring during period 1 (January Y_1-September Y_1). Panel A presents regressions of employee outcomes on the new manager's MOR as measured during period 2. Panel B presents IV regressions of employee outcomes on the new manager's MOR as measured during period 2, while instrumenting using the new manager's MOR as measured during period 1. The F-statistic on the excluded instrument varies across columns due to variation in the number of observations per column. Results in column 5 are robust to using a confidence interval based on Moreira's conditional likelihood ratio test. Panel C presents the reduced form regression of employee outcomes on the new manager's MOR as measured during period 2. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: MOR and Employee Attrition: Exploiting Managers Moving Across Locations and Job Functions

Specification:	OLS	OLS	IV	IV
Panel A: Attrition				
MOR of current manager in 1st period	-0.227**		-0.661**	
	(0.108)		(0.266)	
MOR of current manager in 2nd period		-0.234**		-0.525**
		(0.097)		(0.222)
Mean dep. var.	1.458	1.458	1.458	1.458
F-stat on excl instrument			24.24	27.93
Panel B: Quits				
MOR of current manager in 1st period	-0.086		-0.181	
	(0.063)		(0.151)	
MOR of current manager in 2nd period		-0.064		-0.199
		(0.060)		(0.131)
Mean dep. var.	0.722	0.722	0.722	0.722
F-stat on excl instrument			24.24	27.93
Panel C: Fires				
MOR of current manager in 1st period	-0.110*		-0.246*	
	(0.059)		(0.128)	
MOR of current manager in 2nd period		-0.087*		-0.254**
		(0.045)		(0.118)
Mean dep. var.	0.238	0.238	0.238	0.238
F-stat on excl instrument			24.24	27.93
Panel D: Regretted Quits				
MOR of current manager in 1st period	-0.047		-0.224	
	(0.059)		(0.142)	
MOR of current manager in 2nd period		-0.079		-0.108
		(0.054)		(0.120)
Mean dep. var.	0.606	0.606	0.606	0.606
F-stat on excl instrument			24.24	27.93
Panel E: Non-regretted Quits				
MOR of current manager in 1st period	-0.041		0.032	
	(0.031)		(0.080)	
MOR of current manager in 2nd period		0.011		-0.095
		(0.031)		(0.067)
Mean dep. var.	0.116	0.116	0.116	0.116
F-stat on excl instrument			24.24	27.93

Notes: Standard errors clustered by location-job function in parentheses. This table presents regressions as in equation (6). An observation is a location-job function-period. We use the raw locations with no groupings, and we exclude locations that have less than 10 worker-month observations in the data provided before sample restrictions. The dependent variable is average attrition in that cell. The regressor is the average MOR for managers in that cell, *while measuring that manager's MOR in a particular period.* All regressions include collapsed forms of the controls in Table 3, but instead of controlling for current year, we control for period. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: MOR and Employee Non-Attrition Outcomes

Dep. Var.	Subjective performance (normalized) (1)	Log Salary Growth (x100) (2)	Promotion (x100) (3)	Patents (x100) (4)	Citation-weighted patents (x100) (5)
Panel A: OLS					
MOR in current period	0.053***	0.123	0.072	0.029	0.048
	(0.007)	(0.079)	(0.048)	(0.025)	(0.042)
Panel B: IV					
MOR in current period	0.090***	0.064	-0.020	0.011	0.038
	(0.022)	(0.205)	(0.135)	(0.065)	(0.110)
F-stat on excl instrument	129.0	112.6	124.6	124.6	124.6
Panel C: Red. Form					
MOR in other period	0.029***	0.022	-0.006	0.004	0.012
	(0.007)	(0.071)	(0.044)	(0.021)	(0.036)

Notes: Standard errors clustered by manager in parentheses. The controls are the same as in Table 3. Due to confidentiality, we cannot show the means of most of the variables here, but we discuss approximate information about their levels as appropriate in Section 6 when we discuss the results. In column 1, "subjective performance" is an employee's subjective performance on a 1-5 scale. We then normalize scores across the full sample. In column 2, "log salary growth" represents the change in a worker's log salary from the present month to one year ahead, with coefficients multiplied by 100 for readability. That is, for an employee in May Y_1, the outcome variable is log(salary) in May Y_2 minus log(salary) in May Y_1. In column 3, the outcome is whether an employee receives a promotion, with coefficients multiplied by 100 for readability. In column 4, the dependent variable is patent applications per month, with coefficients multiplied by 100 for readability. In column 5, "citation-weighted patents" is defined as a worker's patent applications in a month, plus log(1+citations to those patents), with coefficients multiplied by 100 for readability. For the analysis of patents and citation-weighted patents, to ensure we restrict to new inventions (as opposed to revisions of past patent applications), we restrict attention to patent applications where the priority date equals the application date. The score is from 0-100, and is normalized across the full sample. In Panel B, the F-statistic on the excluded instrument varies across columns due to variation in number of observations per column. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Manager Rewards

Dep var:	Subjective performance (normalized)	Promoted (x100)	Log salary growth (x100)	Log stock grant holdings (x100)	Log change in stock grants (x100)	Change in span of control	Key individual (x100)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
MOR in current period	0.0868***	0.101	0.135	-2.685*	0.277	0.0637	-0.895
	(0.0231)	(0.0899)	(0.162)	(1.543)	(3.190)	(0.0937)	(0.863)
Panel B: IV							
MOR in current period	0.397***	0.673**	1.405**	-0.670	5.874	0.260	2.078
	(0.083)	(0.311)	(0.627)	(5.252)	(9.943)	(0.303)	(2.696)
F-stat on excl instrument	67.44	67.46	46.16	55.97	54.56	49.83	67.46
Panel C: Red. Form							
MOR in other period	0.131***	0.221**	0.462**	-0.216	1.882	0.0843	0.682
	(0.0234)	(0.0950)	(0.193)	(1.700)	(3.173)	(0.0986)	(0.870)

Notes: Standard errors clustered by manager in parentheses. An observation is a manager-month. The controls are the same as in Table 3. Due to confidentiality, we cannot show the means of most of the variables here, but we discuss approximate information about their levels as appropriate in Section 7 when we discuss the results. “Subjective performance” is a manager’s subjective performance on a 1-5 scale, and then normalized. “Promoted” is whether a manager receives a promotion in a given month, with coefficients multiplied by 100 for readability. “Log salary growth” represents the change in a manager’s log salary from the present month to one year ahead. “Stock grant holdings” measure the value of a person’s unvested stock grants. “Log change in stock grants” uses the data field from the firm on the value of new stock grants issued by the firm in the last year, and takes the log. “Change in span of control” represents the change in a manager’s span of control from the present month to one year ahead. The “key individual” designation by the firm to individuals who are deemed to especially important. In Panel B, the F-statistic on the excluded instrument varies across columns due to variation in number of observations per column. Compensation variables are in nominal terms. * significant at 10%; ** significant at 5%; *** significant at 1%