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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 managers moving jobs. However, people management skills do not consistently improve non-attrition outcomes. Better people managers are themselves more likely to receive higher subjective performance ratings and to be promoted.

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1 Introduction

The relationship between managers and employees is fundamental to the success of firms. While a growing body of research in labor and organizational economics examines the role of management practices in understanding large productivity differences across firms and countries (Ichniowski et al., 1997; Bloom and Reenen, 2007; Bloom and Van Reenen, 2011; Bloom et al., 2017), less attention has been devoted to managers themselves. It seems evident that good managers matter. Many people pay handsomely to attend business school to become better managers, and scores of books are written every year on how to become a better manager. Little empirical evidence, however, exists regarding the managerial production function and the influence of managers on their employees, particularly regarding the influence of interpersonal skills (or what are sometimes called "people management skills").

How much do good people management skills matter? Are good people management skills rewarded inside the firm? We answer these questions using rich employee surveys conducted at a large, high-tech firm. Employees in the firm are asked to evaluate their managers on a number of dimensions, e.g., whether they are trustworthy or whether they provide adequate coaching. We use these surveys to measure each manager's people management skills.

Progress has been made recently in examining how much managers matter using a "value-added" (VA) approach. Bertrand and Schoar (2003) examine how much CEOs matter for various decisions in firms by regressing various firm outcomes on CEO fixed effects, pioneering an approach that has been pursued by a large subsequent literature in finance. In a recent paper, Lazear et al. (2015) use data from one firm to examine to what extent low-level managers (specifically, frontline supervisors) matter for productivity, finding that they matter a great deal. Bender et al. (2018) analyze interactions between employees/managers and management practices in Germany.

While these studies are of great interest, the VA approach faces two main limitations. First, VA studies require good objective data on worker productivity. However, in many firms, direct data on individual worker productivity is often scarce, and sometimes impossible to measure, particularly in high-skill, collaborative environments. When data are available, productivity metrics may be subject to various shocks (e.g., business generated by a law firm partner could be adversely affected by the exit of a single prominent client, who decided to leave the firm for reasons having nothing to do with the partner). Second, VA estimates provide researchers with the overall impact of a given manager on individual outcomes, not the separate impact of people management skills. A manager may appear to have desirable fixed effects for various reasons separate from subordinate-related interpersonal skills, such as ability to bring in high-value clients, or have better problem-solving skills, thereby making his or her employees look more productive.

Rather than estimate VA, we take a different yet complementary approach by measuring people management skills using employees' survey responses about their manager. We explore the extent to which people management skills relate to employee outcomes, with the greatest focus on employee attrition. The data from the firm cover thousands of managers and tens of thousands of employees. In high-tech firms, employee turnover is believed to be a key way by which knowledge and ideas are acquired and lost (e.g., Shankar and Ghosh, 2013; Stoyanov and Zubanov, 2012). As such, many high-tech firms, including ours, are deeply interested in what can be done to reduce turnover, particularly turnover of their high performers.

A central task for managers is to enhance the productivity of their employees and to help them succeed in their jobs. Asking employees about what managers do to improve their performance thus seems like a natural way to measure people management skills, and is one that has been pursued by many firms. Brutus et al. (2006) report that over 1/3 of U.S. and Canadian organizations in their survey use "multi-source assessments" (as opposed to assessing individuals based on their managers) and Pfau et al. (2002) report that 65% of firms use 360-degree performance evaluations.[1] Hence, our approach of analyzing managers using employee surveys thus appears to align closely to the data practices of many firms.

[1]Prominent examples of firms using such surveys include Google (Garvin et al., 2013) and Royal Bank of Canada (Shaw and Schifrin, 2015), as documented in business case studies.

There are several challenges in using employee surveys to measure people management. First, there is concern about non-response bias, but the response rate at our firm is over 95%. Second, one may worry that employees may not be truthful, e.g., workers may not want their manager to know that they evaluated them negatively. This concern is mitigated due to the confidential nature of the survey. Workers are told truthfully 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. Our data is thus limited to manager-year averages for various qualities ascribed to them by their employees. This feature protects worker confidentiality, but does not limit our analysis, given our focus on understanding behavior at the manager level. Third, survey responses may contain measurement error for several reasons, e.g., inattentiveness, sampling error, or different employees treating different 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.

Our main finding is that people management has a strong negative relation to employee attrition. Our main IV results imply that increasing a manager's people management skills from the 10th to 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 persistent subjective performance score differences and using the firm's definition of regretted voluntary turnover. Beyond classical measurement error, IV addresses measurement error in people management 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 measured people management skills that is correlated with employee attrition, as well as concern that the firm is optimally sorting managers and employees together. We address this concern using multiple identification strategies, some of which are in the spirit of 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 instrumental variable. 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 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 (beyond what are already measured using our rich, baseline controls), as well as to rule out assignment bias. All strategies support people management skills having a strong, causal effect on attrition.

Having examined attrition, we also examine the relation between people management and other employee outcomes. Interestingly, we do not find a consistent relation between people management skills and employee subjective performance, salary growth, or probability of promotion, at least once employee fixed effects are controlled for. Thus, good people management affects some outcomes (namely different attrition variables), but not others.

Our secondary result is that managers with better people management skills get "rewarded" by the firm. Better people managers attain substantially higher subjective performance scores and are more likely to be promoted. 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 is related to other work on the importance of individual managers. In addition to the work on VA mentioned above, Bandiera et al. (2017) classify CEOs into two types using machine learning techniques and time use data, finding that one type (representing a higher tendency to delegate) outperforms the other. Friebel et al. (2018) conduct a field experiment where they send a letter from the CEO to store managers explaining that reducing turnover is important; they find that this intervention reduced turnover by 1/4.[2] Most related to our paper is the above-cited work

[2]Also, Glover et al. (2017) show that workers perform worse when paired with more ethnically biased managers. Frederiksen et al. (2017) show that workers benefit financially from having a supervisor who is

of Lazear et al. (2015), who study the impact of lower-level managers in a low-skill setting. Relative to existing work, our paper contributes by asking a different question (i.e., what is the impact of people management skills as opposed to the impact of managers overall) and using a different methodology (i.e., to measure manager skills using employee surveys).

Second, it expands the literature on knowledge-based employees.[3] Much of empirical personnel economics focuses on relatively low-skilled jobs (e.g., truckers, retail, and farm-workers), partially because it is relatively simple to measure individual productivity. In contrast, for high-skilled and knowledge-based jobs, production is often complex, multi-faceted, and involves teamwork. Our analysis sheds light on the managerial production function in such a high-skilled setting.

Third, 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 brings forward a new aspect of performance evaluation, namely reports from a manager's employees, that has not been previously explored in economics.[4] Fourth, it relates to studies of compensation and reward within organizations (e.g., Baker et al., 1994a), providing novel evidence that people management skills are rewarded within the firm.

Section 2 describes the data. Section 3 describes our empirical strategy. Section 4 provides our main analyses on how managers' people management skills affect employee attrition. Section 5 analyzes how people management skills affect outcomes other than attrition. Section 6 analyzes to what extent people management is rewarded. Section 7 concludes.

lenient in performance evaluations. Queiro (2016) shows that firms with educated top managers grow faster.

[3]See, e.g., Bartel et al. (2017); Kuhnen and Oyer (2016); Brown et al. (2016); Burks et al. (2015).

[4]Some economic research uses satisfaction surveys to show that more satisfied workers are less likely to attrite (e.g., Clark, 2001; Frederiksen, 2017). We instead focus specifically on measuring the impact of underlying people management skills, as opposed to a person's individual perception of their job. There is also economics work on student evaluations of teachers (e.g., Beleche et al., 2012). Carrell and West (2010) show that teacher evaluations positively correlate with contemporaneous student VA, but negatively correlate with later achievement. There is also a vast psychology literature on leadership (Yukl, 2010) that includes 360 degree evaluations (e.g., Atkins and Wood, 2002). However, this work seems primarily focused on different issues (e.g., consistency of ratings) as opposed to the causal impact of people management skills on employee outcomes.

2 Data and Institutional Setting

Our data, obtained from a high-tech firm, cover a period of two years and five months, some time during 2011-2015. 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 data are organized as a worker-month panel. Between January Y_1 and May Y_3, we observe several dozen thousands of employees and several hundreds of thousands of worker-months.

The firm is divided into several broad business units and workers are classified by job function. A core job function at the firm is engineering, comprising 36% of worker-months in our sample, and there are also many workers in various non-engineering functions (e.g., marketing, finance, sales). Furthermore, as in many high-tech companies, the firm 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.[5]

About 21% of observations (worker-months) are for individuals in managerial 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, the majority of the workers are hired before that date. Still, 29% of employees in our sample were hired on or after January Y_1. We next provide information on employee outcomes, manager assignment, and the employee surveys, with further details regarding the data in Appendix B.

2.1 Employee Outcomes

In knowledge-based firms such as the one we study (as well as in many non-knowledge-based firms), employee performance often has multiple dimensions, and is rarely defined according to a single productivity metric (e.g., output per hour). There are several core employee outcomes

[5] In the dataset we were provided, over 1/3 of observations are from customer service workers, making them even more numerous than engineers. Thus, if we did not exclude customer service workers, they would play an outsized role in our analysis. Customer service workers also play a qualitatively different role from most other workers at the firm. Still, our main results in Table 3 are robust to including customer service workers.

in our data, the most important one for our purposes being employee turnover:

- **Turnover.** Many firms consider turnover to be a significant problem. High-tech firms are often keenly interested in reducing turnover because employee knowledge is a key asset and turnover is a critical way that knowledge is lost.[6] 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 non-regretted, with further information on this classification in Appendix B. Thus, the data allow us to go much further than is typical in analyzing turnover.

- **Subjective performance.** The firm's subjective performance scores are on a discrete 1-5 scale, as is the case in many firms using subjective performance evaluation (e.g., Frederiksen et al., 2017), and are set biannually. The scores are set in a process involving an employee's immediate manager as well as higher-up managers. While there are some broad guidelines for the distribution of subjective performance scores across various units within the firm, there is not a fixed "curve" across managers in the number of subjective performance scores that can be provided.[7]

- **Salary increases.** While it is difficult to measure the productivity of knowledge workers, one way of proxying productivity improvements is the extent to which an employee's salary increases (or to proxy productivity using the level of salary). Because salaries are listed in local currency, we restrict analysis of salary to employees paid in US dollars following Baker et al. (1994a).

- **Promotions.** Promotions are pre-defined in the data provided by the firm, and correspond roughly to an increase in a person's salary grade. Another recent paper using promotions as a proxy for knowledge worker productivity is Brown et al. (2016).

[6] In fact, as is common in many firms, our firm has analysts who try to predict and reduce turnover.

[7] At high levels of aggregation within the firm (i.e., for top managers), there may be a curve with respect to subjective performance. To address possible bias from curving, we verified that our subjective performance results are robust to excluding high-level managers (with "high-level" as defined in Appendix Table C17).

While these outcomes are all important and capture valuable aspects of worker performance, we do not claim that we are fully measuring "productivity" in our setting.[8] Certain aspects of productivity, such as the contributions of a businessperson to a new marketing strategy or the value of an engineer's computer code, seem inherently difficult to quantify. Indeed, beyond being hard to measure, productivity in some knowledge jobs seems even hard to define. This increases the usefulness of using surveys to measure manager quality relative to using VA.

Different employee outcomes are available at different frequencies, but are coded in our data at the monthly level. Attrition and promotion events are coded in our data at the monthly level using exact dates for these events. Subjective performance reviews occur twice per year, but are also coded month-by-month. Annual salary is tracked at the monthly level, though salary increases are more likely to occur during two particular months.

2.2 Assignment of Managers to Employees

Managers usually manage employees within their function and line of business, driving the initial assignment of employees to managers and reflecting the projects and functions that require employees at any given time. Geographic needs also dictate circumstances in which employees may 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 at the firm are not being randomly assigned to different managers. Instead, managers play a significant role in selecting employees for their teams. We further discuss manager assignment at the start of Section 3, as well as in Section 4.

On average, across managers in our sample, a manager manages about 9 employees at one time. However, the average number of employees per manager is 11 when managerial

[8]In addition to these outcomes, we observe employee "engagement", a number from 0-100 about how engaged the employee is feeling obtained by the same survey used to elicit employees' perceptions of their manager, and is defined using various job satisfaction questions. We do not emphasize engagement because it is not measured at the individual level (only at the manager level), and because it is measured using the same survey as people management skills.

span is weighted by employee-months. In our sample, employees experience an average of 1.4 managers during the dataset (and 1.5 managers when managers per employee is weighted by employee tenure). These numbers reflect that, in our sample, we do not count worker-months if the manager's team at survey time is too small to have the survey administered.[9]

2.3 Employee Surveys

Every year, employees are given a detailed survey. The goal of these types of surveys is for the firm's Human Resource (HR) department and executives to gain an accurate sense of employee opinions. Because the surveys are designed to ensure the anonymity of responses, survey information about one's managers is only collected on managers who manage a minimum number of individuals.[10] Thus, workers on teams smaller than the survey threshold are not part of the analysis, but our results are robust to imputing survey scores for the managers of small teams. Appendix B gives further details.

Surveys of this type are typically administered before year-end, and consistent with this industry norm, the surveys in our data were performed in September in Y_1 and Y_2. The survey had the same format and same manager questions in Y_1 and Y_2.

For our main analysis, to match outcomes with their associated survey, 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.[11] For both the Y_1 and Y_2 surveys, the response rate was 95%.

We also have data for a third survey administered in Sept. Y_3, but we don't use it for our main analysis for three reasons. First, the survey format changed. Second, many prior questions were removed or replaced, including some of the manager questions. Third, the

[9] If we did not make the restriction that a manager's people management score be non-missing in the current and other period, there would be an average of 2.3 managers per worker (see Appendix Table C1).

[10] In the first year of the survey (Y_1), the threshold was 3 employees, whereas in the second year of the survey (Y_2), the threshold was 5 employees. Technically, the survey is "third-party confidential" instead of "anonymous," according to the firm. Anonymous means that it would be totally impossible to tie responses to employee attributes. Third-party confidential means the survey vendor, a third-party independent firm, has access to responses so it can tie them to employee attributes to generate statistical information.

[11] This is how firm analysts assigned the scores in the cleaned data provided.

survey was not administered to one large business unit, so there are many missing values.

Manager questions. Various survey questions are asked every year about what employees think about their managers. For each question, employees are asked whether they Strongly Disagree, Disagree, Neither Agree nor Disagree, Agree, or Strongly Agree. Specifically, we observe answers to the following survey items about one's immediate manager:[12]

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.

A manager's rating on an item is measured as the share of employees who marked Agree or Strongly Agree.[13] 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 in the data provided to us. If employees experience multiple managers over the survey period, they only rate their most recent manager.

A manager's overall rating (MOR) is the average of scores on the 6 items. For example, if a manager had score of 100 on the first 3 items and a score of 50 on the second 3 items, the manager's MOR is 75. The MOR is easy to compute and is used by the firm in its internal

[12] The manager questions are part of a longer survey covering many topics (e.g., engagement and satisfaction). To preserve firm confidentiality, the wording may be slightly modified from the original.

[13] This is how the data are prepared by the third-party survey provider (presumably in part to protect anonymity of responses), and is thus also the form that the firm uses in its internal reporting. Therefore, it is impossible for us to analyze other moments of the survey responses (e.g., the standard deviation of responses about a manager). However, to our understanding, it is common practice in such surveys to break up the 5-answer scale into 2 or 3 parts. For example, exhibit 7 of Garvin et al. (2013) suggests that Google grouped the 5 answers into Unfavorable (Strongly Disagree or Disagree), Neutral (Neither Agree nor Disagree), and Favorable (Agree or Strongly Agree) in its own people management survey. Thus, using the share of employees marking 4 or 5 (as we do) seems consistent with how many firms measure their managers on similar surveys.

reporting and communications. We will use MOR as our main measure of employee-survey-based manager quality, and discuss this further below in Section 2.5.

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 the current period, as well as a non-missing MOR in the other period (we define "periods" below in Section 3.1). This sample restriction is required for our IV analysis, where we instrument manager MOR in the current period using MOR in the other period. We also exclude April and May of Y_3 from our sample, as the firm's location identifiers change in these months compared to before. Thus, our sample runs from January Y_1-March Y_3, though our main results are qualitatively similar to extending through May Y_3.

Table 1 provides summary statistics for our sample. The employee attrition rate is 1.37% per month (or about 15% per year). The majority of separations are voluntary ("quits"), but there are still a sizable number of involuntary separations ("fires"). There are a number of exits which are not classified in the data as voluntary or involuntary.

The average MOR is 81 out of 100. About 81% of employees are co-located with their manager, whereas the remainder are managed remotely. While the number of observations cannot be shown to preserve firm confidentiality, note that observation counts vary slightly by variable, reflecting that our dataset is created by linking multiple firm data files.

Appendix Table C1 provides summary statistics before imposing a manager's MOR to be non-missing in the current and other periods. Appendix A.1 discusses further.

2.5 Properties of MOR

Is MOR the right measure? Is there another way of combining the six manager questions that is more sensible than a simple average? We explore this using principal component analysis. As seen in Appendix Table C3, the first component explains about 70% of the variation in manager scores. Interestingly, the first component is quite close to an equally weighted average of the 6 individual items. Thus, beyond being very simple, another justification for using MOR is that it is close to the first principal component of the six questions, which explains a large share of the variance.

Persistence. 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. Appendix Figure C1 shows that similar correlations are seen on each of the six manager questions. Table 2 observes a similar pattern of some persistence while applying control variables (and using normalized survey scores). Each column takes MOR or one of the managerial quality questions from the Y_2 survey. The score is then regressed on the various manager quality questions from the Y_1 survey. For example, column 1 shows that a manager who performs 1σ higher in Y_1 scores 0.37σ higher in Y_2, conditional on controls.

The predictiveness of the scores over time is sizable, but 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 (which is reflected in the 95% response rate), measurement error often occurs when respondents are asked to answer many questions, particularly subjective ones (Bound et al., 2001). Measurement error could arise from inattention or survey fatigue, as well as factors that could influence how one rates their manager separate from true underlying manager quality (e.g., worker mood). Further, measurement error can arise from the fact that MOR scores are 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.[14] Section 3 details how our empirical strategy addresses various challenges from measurement error in the survey.

Despite the likely presence of measurement error, the results in Table 2 are consistent

[14]Other factors beyond measurement error may also limit persistence. First, a manager's responsibilities, tasks, and projects change over time. A manager might be perceived as providing excellent coaching and guidance for one type of project, but not for another. Second, if a manager scores badly multiple years in a row, he/she may be invited to attend a "bootcamp" to improve manager effectiveness. Appendix Table C4 shows a transition matrix for MOR quintiles.

with the view that managers have particular characteristics that are somewhat persistent over time. One challenge with this interpretation is that the various manager characteristics are correlated with one another.[15] To address this, we also regress each current characteristic on all the six past characteristics at once. Appendix Table C5 shows that each individual characteristic predicts the future characteristic even while controlling for the other characteristics.

As noted above in Section 2.3, we don't use the Y_3 survey for our main analysis because the format changed and it was not administered to a large business unit. Still, Appendix Table C6 shows that the result on the persistence of overall MOR (column 1 of Table 2) is qualitatively robust (but smaller) when including the Y_3 survey.

3 Empirical Strategy

While there are several parallels between the teacher VA literature and our analysis, there are also key differences. First, we are estimating the impact of a survey-measured regressor—people management skills—instead of estimating manager fixed effects. Thus, we need to address the important issue of measurement error in our survey, as opposed to sampling error in estimating large numbers of fixed effects.

Second, unlike in schools where teachers generally do not choose their students, managers at our high-tech firm play a critical role in hiring and 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). Even if we could convince the firm to randomly assign employees to managers, such an experiment 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 employee outcomes as opposed to a source of bias.[16]

[15] The correlation is relatively high, though still much less than 1. See Appendix Table C2.

[16] Beyond a good people manager selecting good workers, it might also be possible for good workers to want to select good managers, i.e., someone could have a reputation as a great people manager so that good workers

A more informative hypothetical experiment for our setting, which we try to approximate in our design based on managers switching locations or job functions in Section 4.4, would be to randomly assign managers to different parts of the firm and then observe employee outcomes, thereby reflecting the role of managers in selecting and motivating their teams. 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, which we discuss further below.

3.1 Econometric Set-up

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

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

where $j(i,t)$ represents that j is the manager of employee i at time t, though we will henceforth abbreviate $j(i,t)$ simply by j. As explained earlier, a concern is that people management skill, m, is measured with error; instead of true underlying people management, we only observe the noisy survey measure, $\widetilde{m}$. In our data, we have the two main waves of the survey, giving us two manager scores $\widetilde{m}_1$ and $\widetilde{m}_2$, with $\widetilde{m}_{j,\tau} = m_j + u_{j,\tau}$, $\tau \in \{1,2\}$. In our data, t is at the monthly level, whereas there are two values of τ.

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 $\tau(t) = 2$ if $t >$ month 9 of Y_1.[17] We refer to τ as the *period*. However, OLS models may be biased by

try to sort onto the manager's team. Indeed, having a "brand" and thus attracting strong incumbent workers could potentially be an important way good people management matters, particularly in high-skilled settings, such as the one we study. In our firm, we happen to see little evidence of observedly better workers joining the teams of better people managers, as seen in Section 4.3 below.

[17] That is, $\tau(t)$ corresponds calendar months to survey periods. Because the Y_1 survey was administered in m9 of Y_1, we analyze employee outcomes during period 1 as a function of their manager's rating during period

measurement error. An alternative approach (e.g., Ashenfelter and Krueger, 1994; Ashenfelter and Rouse, 1998) 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, compared to most empirical studies with measurement error, we make fewer assumptions, yet we still assume 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 there are caps of the management score at 0 and 100), we believe that it is approximately true in our setting, particularly because people are not selecting their own management score.[18]

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 when there is classical measurement error on the right-hand side. In the second term, *Contemporaneously Correlated*

1. This assignment of calendar dates to survey periods is also used by the firm for their internal reporting.

[18]Our analysis of measurement error draws heavily (in content and notation) from Pischke (2007). Assumption 1 seems most likely to be systematically violated when people are answering surveys about themselves and there are social pressures such as conformity bias, e.g., someone with a low amount of actual schooling or earnings might feel social pressure to report that they have more schooling or earnings than they actually have (as in Bound and Krueger (1991)).

Measurement Error, the numerator, $cov\left(\varepsilon_t, u_\tau\right)$, is the covariance between the measurement error from the survey and unobservables that affect employee outcomes. We believe that such measurement error 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, $cov(\varepsilon_t, m)$, represents the correlation of worker-level unobservables with manager quality. This could be positive or negative.

Next, consider the IV estimator where we instrument a manager's score during one period with the manager's score in the other period (as in equation (3) above). Note that different employees may evaluate the same manager during two different periods. Using $\text{plim}(\widehat{b}_{IV}) = \frac{cov(y_t, \widetilde{m}_{-\tau})}{cov(\widetilde{m}_\tau, \widetilde{m}_{-\tau})}$, we get:

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

As for OLS, the expression for the consistency of IV (equation 5) has three terms. The first term has $cov\left(u_\tau, u_{-\tau}\right)$ in place of σ_u^2. 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 error due to small numbers of subjects, one-time data imputation, 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 persistence from a persistently good long-term project or if 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 $cov\left(\varepsilon_t, u_{-\tau}\right)$ instead of $cov\left(\varepsilon_t, u_\tau\right)$ in the numerator. That is, it involves the covariance between measurement error in the *other* period and the unobserved determinants of performance in the current 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 $cov\left(\varepsilon_t, m\right)$ in the numerator, but it is divided now by $\sigma_m^2 + cov\left(u_\tau, u_{-\tau}\right)$ instead of $\sigma_m^2 + \sigma_u^2$. Thus, IV can amplify assignment bias if $cov\left(u_\tau, u_{-\tau}\right) < \sigma_u^2$.

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

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

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 $\sigma_m^2 + cov\left(u_\tau, u_{-\tau}\right)$.

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

3.2 Additional Remarks

- **What if a manager's underlying people management skill varies over time?** This could occur for several reasons, including that a manager's effort may change over time; certain managers may be better with some projects or teams than others; and, the firm may help a manager to improve their people management skills over time. Appendix A redoes the above formulas allowing manager quality to vary over time. For the IV, possible attenuation depends now on the size of the covariance of people management skills over time relative to the covariance of measurement error over time. The OLS and reduced form expressions are broadly similar to before.

- **What happens if people management has persistent effects?** The key identification assumptions for IV are that $cov(\widetilde{m}_{j,-\tau}, \theta_{it}) = 0$ and that the only way that $\widetilde{m}_{j,-\tau}$ affects $y_{i,t}$ is through its influence on $\widetilde{m}_{j,\tau}$. One way this can fail is if there are persistent effects of good people management. Similar to having had a good teacher in the past, it is possible that good people management could have a persistent effect over time. We have two responses. First, existing evidence on manager effects suggests that they are not very persistent: in Lazear et al. (2015), 2/3 of boss effects disappear after 6 months, and 3/4 disappear after one year.[19] Second, some of our identification strategies rule out persistent effects. For example, when we analyze new workers joining the firm in period 2, their current manager is interacting with them for the first time at the firm, so it is impossible that the new workers benefitted from interacting with their current manager during period 1. Persistent effects can also be addressed by looking at individuals who switch managers. That we reach qualitatively similar conclusions with these identification strategies is consistent with people management having primarily a contemporaneous effect.

- **Control variables.** While the above setup ignored control variables, adding controls helps address the possibility that MOR and employee outcomes may differ systematically in the large firm we study. 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 (the firm has many offices), employee salary grade (or level), and an employee's manager's span of control. Section 4.5 shows that our results are robust to even finer controls.

- **Adding worker fixed effects.** Beyond control variables, worker fixed effects can be added to an IV specification. The key is for some workers to experience multiple managers during the period for which output is being analyzed.

[19] This evidence differs from ours in several respects. First, we analyze people management skill as opposed to overall boss effects. Second, we primarily analyze attrition whereas Lazear et al. (2015) primarily analyze output per hour. Third, we study a high-skill firm, whereas Lazear et al. (2015) study a firm where workers do a routine job. It is not clear whether such differences would lead to boss effects being more or less persistent, though we might imagine that the identity and skills of one's current manager would be particularly important for our outcome of attrition.

4 Manager Quality and Employee Attrition

Section 4.1 presents our baseline results on the relationship between MOR and employee attrition. Next, we present our three research designs: new joiners (Section 4.2); new joiners or employees switching managers (Section 4.3); and managers switching locations or job functions (Section 4.4). Exploiting different variation (and requiring different identifying assumptions) and addressing different threats to identification, all three designs provide complementary evidence supporting that people management skill substantially reduces employee attrition (Appendix Table C7 summarizes rationales for our different identification strategies). Section 4.5 addresses additional threats to identification and estimates manager VA (in line with past work on managers such as Lazear et al. (2015)).

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 -0.154 means that increasing MOR by 1σ is associated with a monthly reduction in attrition of 0.154 percentage points (hereafter "pp"), which is an 11% reduction relative to the mean of 1.37pp per month. Column 3 presents the IV estimate where MOR in one period is instrumented with a manager's MOR in the other period. Here, the 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 substantially larger in magnitude than the OLS estimate is consistent with OLS being significantly biased downward in magnitude due to attenuation bias from measurement error.[20] Still, as discussed above, IV could also be biased due to asynchronously correlated measurement error or assignment bias by the firm. We address this further below.

Our IV estimate implies that moving from a manager in the 10th percentile of MOR to

[20]Our findings suggesting significant measurement error are consistent with Bloom et al. (2017), who document significant measurement error in surveys about management practices.

one in the 90th percentile of MOR is associated with a reduction in quitting of roughly 60% (under the assumption of normality).[21] To further assess the IV magnitude, we compare it to estimates in other studies analyzing turnover, particularly those related to management. Bloom et al. (2014) show that randomly assigning call-center employees to work from home reduces turnover by 50%. Thus, having a manager in the 90th percentile of the people management distribution instead of one in the 10th percentile has an impact on turnover broadly similar to that of letting employees work from home.[22]

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 the same analyses as Panel A, but separately for quits and fires.[23] 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 "bad quits" (bad from the firm's perspective) more than it reduces "good quits."

Another way to analyze turnover is to look separately at "high" and "low" productivity workers. To classify workers as high or low productivity, we residualize workers' subjective performance scores on the controls in Table 3 and then regress the residuals on worker fixed effects. Fixed effects that are above the median are classified as high-productivity workers and those below as low-productivity workers. Appendix Table C8 analyzes turnover separately for high- and low-productivity workers. The IV coefficients are large and significant in both

[21] The 10th percentile of a standard normal is 1.28σ below the mean, corresponding to a monthly attrition rate of 1.374-1.28*(-.475)=1.98pp. The 90th percentile corresponds to a monthly attrition rate of 1.374+1.28*(-.475)=0.77pp, a reduction of slightly more than 60%.

[22] In another management-related example, Friebel et al. (2018) show that randomly sending letters from the CEO highlighting the firm's turnover problem causes grocery store managers to reduce store turnover by 1/4 for nine months. In a non-management example, Madrian (1994) analyzes the impact of having a spouse with health insurance to study "job lock." She finds that job lock reduces turnover by 25%. Thus, our IV estimate from increasing MOR from the 10th to the 90th percentile has roughly twice the impact on turnover as does one's spouse having health insurance in the US. Surveying the literature, Manning (2011) reports wage-turnover elasticities of 0.5-1.5.

[23] In the data provided, turnover is marked as voluntary, involuntary, or missing. We don't use missing turnover events in Panels B and C (missing events are included in Panel A), but one can also classify the missing data fields as voluntary turnover events.

cases. As has been found in many studies, there is strong selection on productivity in turnover (e.g., Hoffman and Burks, 2017), with high-productivity workers having a substantially lower base probability of attrition. While the IV coefficient is larger in absolute magnitude for low-productivity workers, it is very similar in percentage terms for high-productivity workers.

Beyond leading people to exit the firm, bad people management skills could also produce other types of "exits." Instead of quitting the firm, a worker may simply demand that they be moved to a new manager. Appendix Table C9 repeats our analysis using whether a worker changes manager as the dependent variable (instead of attrition). The IV estimate implies that moving from a manager in the 10th percentile of MOR to one in the 90th percentile of MOR is associated with a reduction in the probability of switching managers by 45%.

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

Repeating our IV analysis while using only 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 has several advantages relative to our baseline analysis and 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.[24]

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 sort into managers. Further-

[24]The joiners analysis also eliminates concern about events in period 1 that would lead workers to rate their managers more highly, as well as affect their quitting in period 2. For example, if an employee got to work on a very enriching project in period 1 that improved their general human capital, this could lead them to highly rate their manager in period 1, as well as to be less likely to quit in period 2. It is important to note, however, that our new joiners analysis does not help with persistent shocks, e.g., a very successful or exciting project that would make current employees rate a manager highly in period 1, as well as make new workers less likely to quit. The joiners analysis is useful, however, if people differ in their opinions about whether a project is exciting, as the joiners analysis looks at different people in period 2 compared to the raters in period 1.

more, the firm seems unlikely to have substantial information about the new worker separate from the manager who was involved in hiring the new worker.[25] 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. Overall, while we have much less statistical power here than for our full sample, the relationship between MOR and attrition in Table 4 is qualitatively similar to that in the baseline analysis. Looking at all attrition 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% (reflecting the smaller sample for the joiners analysis). 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 quits, fires, regretted quits, and non-regretted quits, 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

While the joiners analysis has several clear benefits, it also has limitations. First, it is based only on new workers, so there are potential concerns about external validity (e.g., could there be something special about the impact of people management skills on new workers?). Second, the sample size is small relative to our full sample. Third, it is difficult to do certain statistical tests regarding assignment bias. In this section, instead of just analyzing workers joining the firm in the second period, we additionally add instances of people switching managers during the second period. This analysis addresses these three limitations, and we continue to find a strong negative relation between a worker's manager's MOR and various turnover outcomes.[26]

Our analysis here is useful for multiple reasons. First, the sample is broader than

[25] Lazear et al. (2015) use new joiners as a research design for estimating manager fixed effects. Our argument that new employees have limited private information, and that the firm has limited information (separate from the manager), closely follows a similar argument in Lazear et al. (2015).

[26] Manager switches occur for many reasons at the firm we study, such as new projects, manager turnover, and promotions, as well as several re-organizations ("re-orgs") that occurred for exogenous reasons.

only new joiners. 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.[27] Third, we can make "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, one might believe that matching occurring for reasons such as manager turnover or re-orgs might reflect less active involvement or deliberate matching of the firm, particularly when there are many individuals being moved at the same time. It is presumably harder for a firm to do sophisticated matching when it has to make many personnel changes in a short amount of time. Fifth, as in the joiners analysis, the workers in this design do not influence the instrument—if I switch to a new manager in T_2, I will have played no role in that new manager's scores in the T_1 survey.

The sample size here is about one quarter the size of that in our baseline analyses. When workers switch manager, 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 promotions, or from changes in job function or business unit.

Results. Table 5 reproduces Table 3 while analyzing both T_2 joiners (as in Table 4) and workers switching managers in T_2. In Panel A, the IV coefficient for overall attrition is -0.332 (corresponding to a 22% drop in attrition per 1σ in MOR). This is a bit smaller than our baseline attrition estimate and misses conventional statistical significance ($p = 0.16$). In contrast, the coefficient for quitting (panel B) is slightly larger than the one in Table 3. This drop is driven by a statistically significant drop among regretted quits. Interestingly,

[27] As discussed further in Appendix A.5, we test whether the MOR of one's future manager predicts employee outcomes prior to a manager switch. This "Rothstein test" is not possible for new joiners, as employee outcomes are unobservable prior to joining the firm. Thus, the test does not rule out that better people managers may be adept at hiring better new employees for their teams.

the MOR coefficient for non-regretted quits is positive (though not statistically significant); this is consistent with the idea that people management skills may play a particular rule in reducing "regretted attrition".

As discussed in Appendix A.4, the results here are broadly similar when only using workers switching managers.

Time path of people management impacts. Figure 3 takes the IV regressions in Table 5, but interacts MOR with the quarter since receiving a new manager. As seen in panel (a), when workers receive a new manager, they are less likely to attrite when the manager has high MOR compared to when the manager has low MOR. While negative, however, the relation between MOR and turnover in the first six months after the manager change is fairly small in magnitude. Rather, the turnover benefit builds gradually, with much of the reduction in turnover only occurring in quarters 2 and 3 after a manager change (i.e., months 7-12 since the manager change). In the other panels, the impact of MOR also tends to grow in magnitude with manager tenure, particularly for regretted quits.

The time path of results in Figure 3 seems consistent with a causal impact of people management on attrition.[28] The impact of a good manager may not be felt immediately after they become a worker's manager. Rather, it may take some time for a worker to get to know and be affected by their manager. If the results in Figure 3 were driven by assignment bias (e.g., the firm decides to match unobservedly better workers with better managers), one might imagine that turnover impacts would be seen immediately instead of growing over time. The time path also seems broadly consistent with people management playing a role in motivating workers, as opposed to simply selecting better workers.

Testing for assignment bias. A concern with the switchers analysis is that the firm may be matching unobservedly 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-

[28]Figure 3 plots the absolute magnitude of MOR impacts relative to quarter since getting a new manager. We reach the same conclusions if we instead plot percentage impacts (i.e., MOR impacts on attrition relative to attrition risk in that quarter).

attrition outcomes in the current period, following Rothstein (2010, 2017). As detailed further in Appendix A.5, we implement an IV procedure where we instrument the future manager's MOR during period 2 using the future manager's MOR during period 1. Note that we cannot use attrition for the test because workers who will experience a new manager in the future do not attrite before they experience the new manager.

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 with three other important worker characteristics, namely, the employee engagement, restricted stock units granted to an employee, and whether the firm has designated a person as a "key individual" whom they strongly want to retain. Stock grants and the key individual variable are discussed further in Section 6 when we discuss rewards for managers.

In the IV analyses in Panel B, we see little evidence that better people managers receive teams with observedly better characteristics. Of the 7 coefficients, 1 is significant at 10%, close to what might be expected from random chance. Of course, while the test suggests that better people managers are not sorted with better employees in terms of these observables, we cannot totally rule out that there would be sorting based on unobservables. Still, the test suggests that assignment bias (from the firm sorting strong people managers with unobservedly better employees) is likely limited.[29]

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 location-job function-period level (e.g., engineers at Location X in period 1), and examine the relation between average MOR and average attrition using the collapsed data. This serves two key purposes. First, it provides

[29]The test is also consistent with better people managers not selecting better existing employees for their teams. A statistical zero in the Rothstein test could conceivably also result from negative assignment bias (in terms of the firm's role) and positive selection of employees by managers given the firm's policies. Overall, we view the Rothstein test as less important for the validity of our results (compared to other settings) given that managers selecting better workers for their team should be viewed as a mechanism of people management.

further evidence (consistent with our previous tests) that assignment bias does not drive our findings. This is 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 concern 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 engineers at a location 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. Following Chetty et al. (2014), standard errors are clustered by location-job function. Appendix A.6 provides further details on implementation.

Similar to Chetty et al. (2014), our key identifying assumption is that changes in average location-function people management skills are uncorrelated with average location-function unobserved determinants of attrition, conditional on controls.[30] To control for possible changes in worker quality over the two periods (due to worker sorting or workers moving with their

[30] 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, as doing so leads to very large standard errors. Instead, we control for a rich set of location-function worker characteristics.

managers), we include controls for average location-function worker characteristics. While the key identifying assumption is difficult to test, we are not aware of 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, 6% of managers experience a change in location, 6% of managers experience a change in job function, and 11% of managers experience a change in either location or job function. In the month of a location change, the promotion rate is 2.5%, whereas in the month of a job function change, the promotion rate is 26.5%. Thus, we suspect that many (though certainly not all) of the job function changes seem to occur from promotions, whereas this does not seem to be the case for location changes. We suspect that the location changes involve a combination of business reasons (e.g., moving to a nearby location because of some business need) and personal reasons. Jin and Waldman (2017) show that within-firm changes in job function like the ones we study are also prevalent in other firms.

Results. We obtain the same broad conclusion that people management skills reduces attrition, even though we are exploiting a quite 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 possible 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 mean MOR in the other period for that location-job function.

As in our main results in Table 3, IV is larger in magnitude than OLS. Focusing first on the overall attrition results in Panel A, the IV estimates imply that a 1σ increase in a manager's MOR decreases employee attrition by 0.6-0.7pp 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 clearly overlap with that in Panel A of Table 3. Outside of Panel A, we have less power (and often lose statistical significance), but observe broadly similar results to before.

Appendix Table C11 shows that the results here are robust to (and actually a bit stronger when) restricting to location-job functions that are in the data for both periods.

4.5 Additional Threats to Identification and Robustness

Adding Richer Controls. A concern for the results is the possibility of a persistent unobservable that is not fully addressed by the above research designs (e.g., people always rate a manager well because that manager is overseeing a good project, and the manager continues working on the good project when he/she moves locations or job functions). A way to proxy for such unobservables is to add further controls.

Appendix Tables C12-C15 shows that our attrition estimates are quite similar when adding additional controls. For the 4 sets of tests (sections 4.1-4.4), we gradually add additional controls, including two-way interactions between business unit, job function, and salary grade, as well as current month dummies. For example, instead of just having dummies for being an engineer and being at a particular salary grade, we add dummies for being an engineer of a particular salary grade. To formally assess coefficient sensitivity, we perform the Oster (2017) test. Given our IV set-up, we analyze the reduced form. The results consistently imply that selection on unobservables would need to be many times larger than selection on observables (details are in Appendix A.7).

Functional Form of the Regressor. Our results use normalized MOR. To check that our results are not driven by the functional form of the regressor or by outliers, we re-ran the analysis in Panel A of Table 3 while grouping MOR in percentiles or 5 quintiles, and obtained qualitatively similar results. This is unsurprising given the fairly linear relationship in the reduced form in Figure 2. Appendix Figures C2 and C3 show binned scatter plots corresponding to the research designs in Sections 4.2-4.3.

Heterogeneity analysis. Within the large firm we study, we can also examine heterogeneity in the IV estimates by hierarchy, geography, and occupation. Appendix Tables C16-C17 show that the negative IV relation between MOR and turnover is qualitatively robust across hierarchy, geography, and occupation. Results seem particularly strong at higher levels of the hierarchy and in rich countries. For brevity, results are discussed in detail in Appendix A.8.

VA Approach. We also perform a "value-added" analysis of managers, similar to Lazear et al. (2015). (We use quotation marks because we analyze attrition instead of productivity.) By doing so, we can compare the spread of manager VA with the importance of MOR for attrition. In line with our focus on attrition, we estimate:

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

where y_{it} is a dummy for whether person i attrites in month t; γ_j is a manager effect; and X_{it} are various controls. As discussed in Lazear et al. (2015), as well as in work on teacher VA, an important issue is accounting for sampling variation. That is, if manager fixed effects are estimated using a finite number of observations per manager, our estimate of the standard deviation of manager fixed effects may be biased upward. We address this using two approaches. First, we present standard deviations weighted by the number of observations per manager ("one sample" approach). Second, following Silver (2016), we split the data in two and estimate (7) for two separate samples. Assuming that the sampling error is uncorrelated across samples and uncorrelated with underlying value-add, the covariance of the estimated manager VA across the two samples is equal to the variance of underlying VA.[31] We do this either randomly splitting employee-months into two samples or splitting by period.

Appendix Table C19 shows that there is significant variation in manager effects for the outcome of employee attrition. In the split sample approach, we find that the standard deviation of manager effects for attrition is 0.67 when splitting randomly and by period. Thus, the consequence of improving manager VA in attrition by 1σ (0.67pp per month) is about 40% larger than the impact of improving underlying people management skills by 1σ in our baseline estimate in Panel A of Table 3 (0.48pp per month). Appendix A.9 discusses VA further.

[31] Let γ be the underlying VA for a manager. Let $\hat{\gamma_1} = \gamma + u_1$ and $\hat{\gamma_2} = \gamma + u_2$ be estimated VA in the two split samples, where u_1 and u_2 are errors. Note that $cov(\hat{\gamma_1}, \hat{\gamma_2}) = var(\gamma) + cov(\gamma, u_1) + cov(\gamma, u_2) + cov(u_1, u_2)$, which equals $var(\gamma)$ under the stated assumptions. This derivation also appears in Silver (2016).

5 Manager Quality and Non-attrition Outcomes

This section shows that there is no consistent evidence that MOR improves non-attrition outcomes. There is a small statistically significant relation between MOR and subjective performance, and a fairly precise no relation of MOR with either salary increases or promotion. The small positive relation for subjective performance is not robust to our research designs.

Employee subjective performance. Columns 1-2 of Table 8 show that MOR appears to have only a modest positive relation to employee performance as measured by subjective performance reviews. On the left-hand side, we use an employee's subjective performance review on a 1-5 scale, which we then normalize. Column 1 of Table 8 presents a baseline estimate without employee fixed effects. A 1σ increase in MOR is associated with a 0.04σ increase in employee subjective performance under OLS, as well as a 0.10σ increase under IV. As for the earlier attrition results, OLS is likely biased downward due to attenuation bias.

In column 2, we add employee fixed effects. It is not clear *a priori* whether the results with or without fixed effects should be preferred. The results without employee fixed effects examine the relationship between MOR and employee outcomes inclusive of managers possibly being able to select better employees. Results with employee fixed effects tell us how MOR relates to various outcomes *within an employee*, which may be useful to know if some managers happen to receive better or worse employees as a result of luck or other factors unrelated to their managerial quality. We therefore will often present results with and without employee fixed effects. In column 2, when employee fixed effects are included, the relationship between MOR and subjective performance shrinks toward 0. The IV coefficient of 0.032 seems small in economic magnitude, implying that moving from the 10th to 90th percentile of managers would only improve subjective performance by 0.08σ. Column 2 suggests that the estimate in column 1 reflects some aspect of how managers and workers are sorted together (such as better managers hiring better workers).

Employee salary increases. In Columns 3-4 of Table 8, the outcome is the increase in salary 12 months from now relative to the present. 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, a 1σ increase in MOR is associated with roughly a 0.12% increase in employee salary in OLS and a 0.06% increase in IV, both statistically insignificant. The average salary increase per year in our data is confidential, but is between 4% and 8%, so these impacts seem small compared to that. With 95% confidence, we rule out coefficients greater than 0.28-0.47 (depending on OLS or IV), i.e., we rule out that a 1σ increase in MOR predicts a salary increase more than 0.28%-0.47%. The coefficients are broadly similar when employee fixed effects are added.

Employee promotions. Columns 5-6 examine the relationship between MOR and employee promotions. In column 5 of Panel A, we see an insignificant positive relation in the OLS, but it turns negative in the IV in Panel B. In the IV in column 5, the top of the 95% confidence interval is 0.24. Given the average monthly promotion rate at the firm of between 1.5% and 2%, this means we can rule out that a 1σ increase in MOR would increase the promotion probability by about 12%-16%. Thus, while we cannot rule out small impacts, we can rule out sizable ones. Recall we find 1σ decreases attrition by 28%, so the top of the confidence interval for promotion is about half as large.

5.1 Additional Remarks on Non-attrition Outcomes Results

Research designs. Appendix Tables C20 and C21 repeat our joiners and workers changing managers research designs for the non-attrition outcomes. Compared to attrition, the results on the non-attrition outcomes are much less robust across the different research designs. Thus, while the different designs provide strong evidence that better people management reduces attrition, they fail to do so for other outcomes.

Additional outcome. Beyond the above outcomes, another outcome we can evaluate is employee engagement, which is obtained from the same survey as the manager scores. Appendix Table C22 shows some evidence that MOR may predict increased employee satisfaction, though the coefficients are small in magnitude and vary across specification. A 1σ

in people management skills predicts a 0.07σ increase in employee engagement in IV without worker fixed effects, but only a statistically insignificant 0.02σ increase in IV with fixed effects.

Differential Attrition. In Section 4, we provided evidence that higher MOR lowers employee attrition. However, such differential attrition could potentially bias estimation of the relation between MOR and non-attrition outcomes. For example, if a very good manager successfully retains all of their employees (both the stars and the mediocre ones), this might lead to the manager getting lower average achievement on an employee outcome variable than if the mediocre employees left. To address this concern, we repeated our analysis in Table 8 while restricting to employees who never attrite. Such a "balanced panel" analysis (e.g., Brown et al., 2016; Burks et al., 2015) yields qualitatively similar results to those in Table 8.

Why do we see large impacts of people management on attrition, but not on our non-attrition outcomes? There are several possible answers. First, it could be that good people management naturally matters most for attrition, which may reflect issues such as whether an employee feels respected and motivated. The people management skills of one's manager may matter less for subjective performance, salary growth, or promotions, for which technical talent and knowledge may be more important. Second (and related to the first answer), it could reflect that some outcomes are "stickier" and harder to change. It might be easier for a manager to affect whether someone feels engaged and motivated (thereby reducing attrition) and harder to affect subjective performance. Such a possibility would not diminish the importance of attrition, but would reflect that it is easier to change. Third, it could be that certain outcomes take more time and more interaction to be affected. Ultimately, it is hard for us to definitively distinguish these possibilities in our data.

6 How does the Firm Reward Good Managers?

So far, we have presented evidence that a manager's people management skills, as measured by MOR, reduce employee turnover. 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* may be complex and multi-faceted. Individuals can be rewarded through promotions, higher salary, or stock grants. The firm could also respond to managers in other ways such as changing their span of control so that better managers become responsible for managing more people.[32]

We estimate regressions similar to those in Section 3.1, except we include manager rewards on the left-hand side 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) achieved by manager j in month t. We include the same controls as for our analysis of worker outcomes.

6.1 Evidence on Manager Rewards

Manager Subjective Performance. Subjective performance is a critical measure of "reward" at our firm, as the subjective performance score is a critical determinant (though not an absolutely deterministic one) of the financial rewards that a person receives. As seen in column 1 of Table 9, a 1σ increase in MOR is associated with a 0.06σ increase in subjective performance in OLS, but a 0.40σ increase in subjective performance in IV. The IV coefficient is substantial, both statistically and in economic magnitude. As in our main results on how MOR affects employee attrition, we suspect that OLS is subject to attenuation bias from measurement error.

Promotions. Table 9 additionally shows that better people managers are substantially more likely to get promoted, with a 1σ increase in MOR predicting a 0.7pp increase in promotion probability each month. Given the average monthly promotion rate of 1.5%-2%, this coefficient is economically substantial. The result is consistent with the firm wishing to pro-

[32] Another outcome to examine is whether a manager attrites. Our IV analysis is not ideally suited for analyzing manager attrition because it requires observing a manager score for a manager in both periods. This caveat aside, if one performs the analysis in Table 9 using manager attrition as an outcome, one finds a statistically significant negative relation between a manager's MOR and the manager's turnover, i.e., managers with better people management skills are less likely to attrite.

mote good people managers to higher levels of the organization where they may have greater impact, i.e., a selection story. It is also consistent with an incentive story, where the firm encourages good people management skills by promoting those who exhibit them (Lazear and Rosen, 1981).[33]

Compensation. There no significant relationship between MOR and log salary. However, there is a statistically significant positive relationship between MOR and log salary growth. A 1σ increase in MOR is associated with a 1.4% larger increase in salary over a 12-month period. As reported earlier, the average annual increase in salary is confidential, but is between 4-8%, so this seems economically meaningful. If we re-do the result using 1-month increase in log salary, there is also a significantly positive IV relationship.

Another means of compensation, particularly in high-skill 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 the level of salary or stock grants. Overall, it seems that there is no consistent relationship between MOR and compensation, though there is also some evidence for a positive relationship.

Span of Control. In models of optimal span of control such as Lucas (1978) and Garicano (2000), firms optimally assign better managers to manage larger teams. Empirically, we examine whether managers who achieve higher MOR become more likely to manage larger teams. Column 7 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 data show that better

[33] While this result is intuitive, it is far from obvious. If a manager is doing well, there is no guarantee that they would also succeed in a new role after a promotion. Such considerations seem to be given less weight by the firm than the selection and/or incentive rationales of promoting people with high MOR.

people managers are more likely to be designated "key individuals" (with a 1σ increase in MOR predicts a 2.1pp increase in the probability of being designed a key individual), though the relation is not statistically significant.

6.2 Threats to Identification and Discussion

Adding richer controls. A concern for our rewards results in whether they could reflect some unobserved variable. For example, if there was a persistent unobservable that affected the way employees ranked their manager as well as how a manager was rewarded (e.g., a good project), this could be a violation of the exclusion restriction.

Similar to as in Section 4.5, Appendix Table C23 presents results on the statistically significant reward variables (subjective performance, promotions, and salary increases) as further controls are added. The key IV coefficient is fairly stable across specifications, with the limited selection on observables suggesting that selection on unobservables is also likely small (Oster, 2017). Appendix A.7 discusses further.

Pay for past performance. An issue in considering manager rewards is whether pay for past performance could constitute a violation of the exclusion restriction. To address this, we redo our compensation results while restricting attention to compensation in the first period (so that the instrumental variable is a manager's people management score in the second period). As seen in Appendix Table C25, the results are broadly qualitatively similar.[34]

Including manager VA as a regressor. One question is whether the results are robust to controlling for a manager's overall skill at reducing attrition. That is, conditional on a manager's ability to reduce attrition, are managers with greater underlying people management skills still rewarded? To address this question, we repeat Table 9 while including a manager's attrition VA fixed effect on the right-hand side. We normalize the fixed effects and

[34]For subjective performance, the IV coefficient is 0.20, which is statistically significant at 5%, and a bit smaller than in Table 9, though the 95% confidence intervals overlap. For promotions, the IV coefficient of 0.94 is a bit higher than in Table 9, but is no longer statistically significant, reflecting a large standard error. For compensation, the coefficient on log salary growth remains positive but loses statistical significance, whereas the coefficient on log change in stock grants increases (but is not statistically significant).

multiple by -1 to create a manager fixed effect in terms of retention. To account for sampling error in manager VA, we use a split sample IV approach (e.g., Frederiksen et al., 2017); as in Section 4.5, we estimate manager fixed effects separately after randomly splitting the data in two. We use one fixed effect as the endogenous regressor and one as the instrument.

Appendix Table C26 shows that the relation of MOR to rewards is mostly similar when controlling for manager VA. Appendix A.10 provides further discussion.

Discussion. Broadly speaking, Table 9 shows that better people managers do 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 appears to reward good people management. In fact, if one redoes the analysis by occupation, there is a stronger relation between MOR and a manager's subjective performance score for engineers (IV coef=0.99, se=0.32), relative to the overall population.

One important caveat regarding the reward results is that we only observe a relatively short panel. In the longer-run, it could be that better people managers are rewarded to a greater (or lesser) extent than observed here.

7 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 may be difficult in knowledge-based firms and other firm contexts where objectively measuring productivity is challenging. We pursue an alternative approach using employee surveys. Employee surveys also enable us to address a different and complementary question to VA work. VA papers answer: how much do managers matter? We answer: how much does one particular set of skills, namely, *people management skills*, or interpersonal skills for dealing with one's subordinates, matter? Employee surveys about managers are used by many firms, but we have little hard evidence on the importance of people management skills.

In our baseline IV results, we find a strong positive relationship between people management skills and employee retention, a critical outcome in high-skill firms. A causal interpretation is strengthened using several complementary research designs. However, people management skills do not seem to consistently improve non-attrition employee outcomes. Managers with better people management skills receive higher performance ratings and are more likely to be promoted, consistent with the firm attaching significant value to managers' impacts on employee turnover.

Although 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 the external validity of our results (which is frequently a challenge in personnel economics), and suggests that our conclusions may hold in other contexts. While statistical power is somewhat limited in heterogeneity analyses, there is some intriguing evidence that people management skills seem to be particularly important at high levels of the organization and in rich countries. Because the surveys we study are collected by many firms, future work should examine how the importance of people management skills vary by firm and context.

One direction related to our paper is the growing literature on social skills, i.e., skills that are primarily interpersonal in nature. Scholars have shown that social skills play a critical role in determining wages and occupations, and that social skills command a rising return in the labor market (e.g., Borghans et al., 2014; Deming, 2017).[35] Our work provides novel evidence on the importance of social skills in management, an area where they have received much less economic attention than others, but where new evidence is starting to emerge.[36]

Our results help open the black box of managerial production, focusing on the specific importance of people management skills. Still, while our results indicate that people man-

[35] Social skills are generally thought of as one component of "soft skills;" see Heckman and Kautz (2012) for a general discussion on soft skills.

[36] Kuhn and Weinberger (2005) show that men who occupied high school leadership positions earn more years later. Lazear (2012) shows that successful business leaders tend to be generalists instead of specialists. Schoar (2016) shows that a randomized intervention in Cambodian garment factories aimed at improving supervisors' communication skills and treatment of workers improved productivity.

agement skills matter to a significant degree, and matter primarily for certain outcomes, the precise manner by which they matter remains an open question. Another important question for future work is, is it possible to predict what types of individuals are likely to exhibit good people management skills at a later time? How much are people management skills innate, and how much can they be taught? Future work can address these questions.

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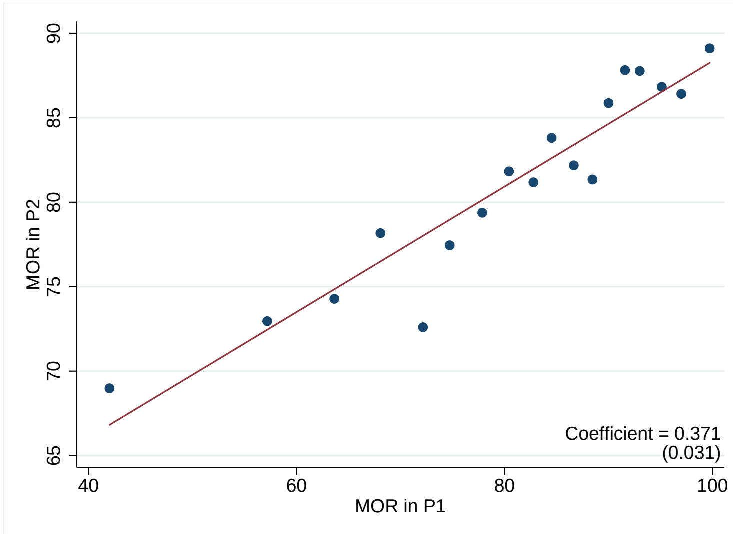
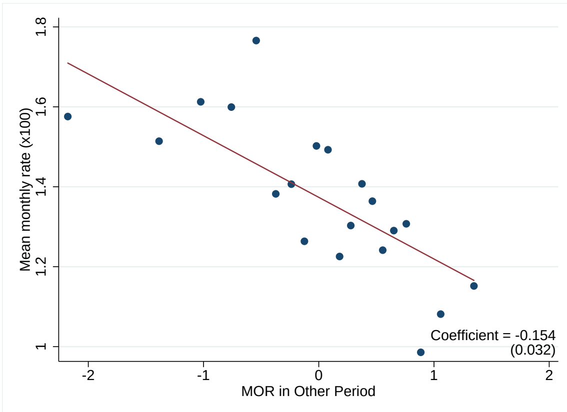
Figure 1: Correlation of Manager Overall Rating (MOR) across Two Surveys

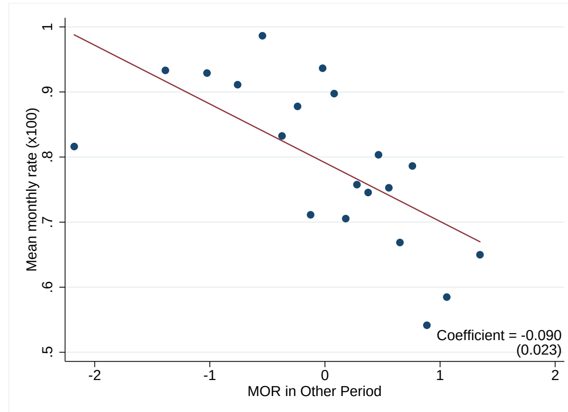


Notes: This figure presents a binned scatter plot of MOR in period 2 on MOR in period 1 (using "binscatter" in Stata), with no control variables. An observation is a manager. In the lower-right of the figure, we list the regression coefficient (with a robust standard error in parentheses) for a manager-level regression of MOR in period 2 on MOR in period 1. See Appendix Figure C1 for a similar figure, but made separately for each of the six manager survey questions.

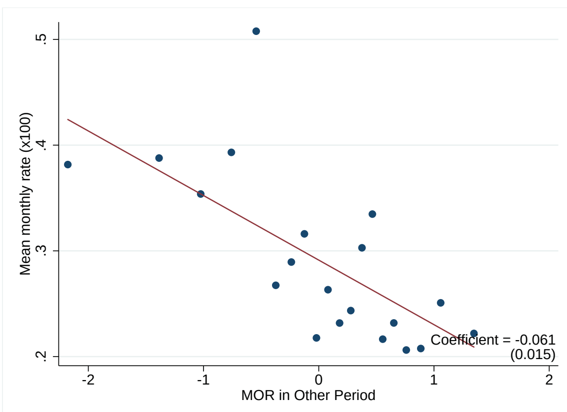
Figure 2: Reduced Form Binned Scatter Plots: Regressing Attrition Variables on Current Manager MOR in Other Period



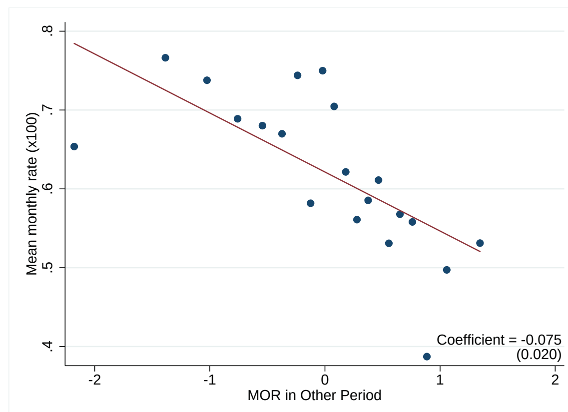
(a) Attrition



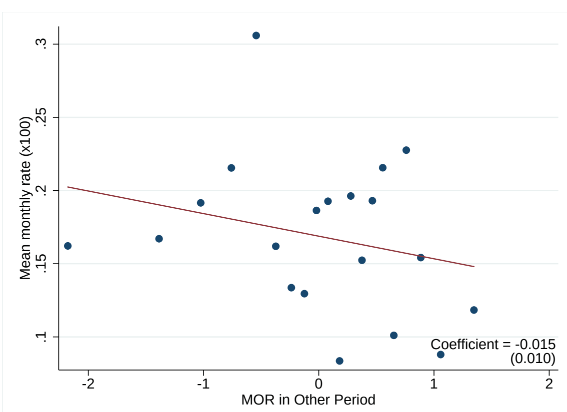
(b) Quits



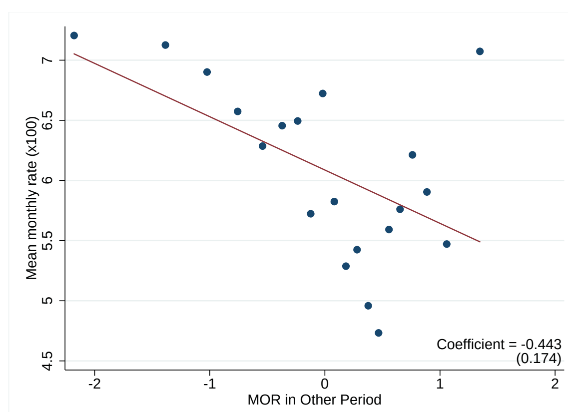
(c) Fires



(d) Regretted Quits



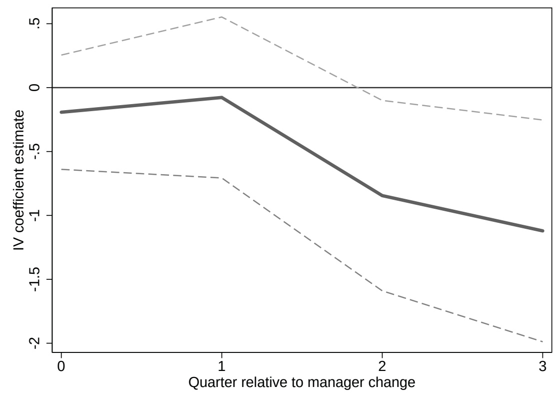
(e) Non-regretted Quits



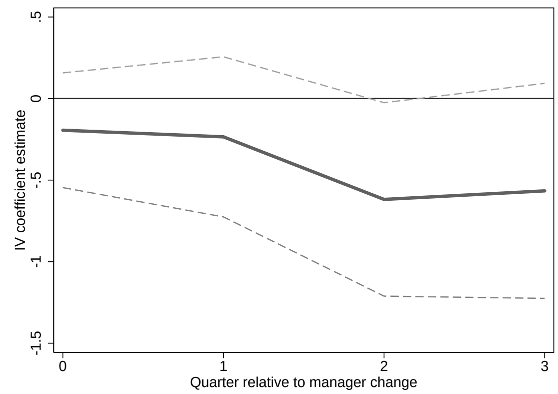
(f) Worker changes manager

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

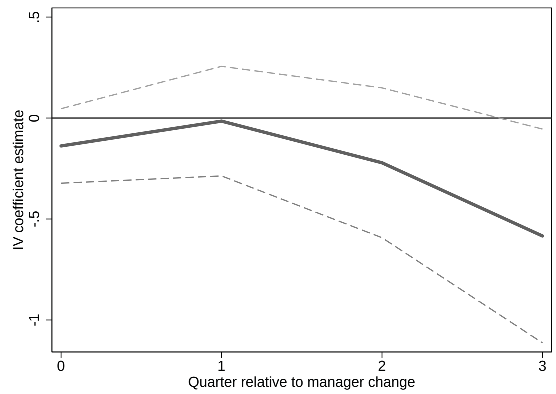
Figure 3: Impacts of MOR on Attrition Outcomes by Quarter Since Getting New Manager



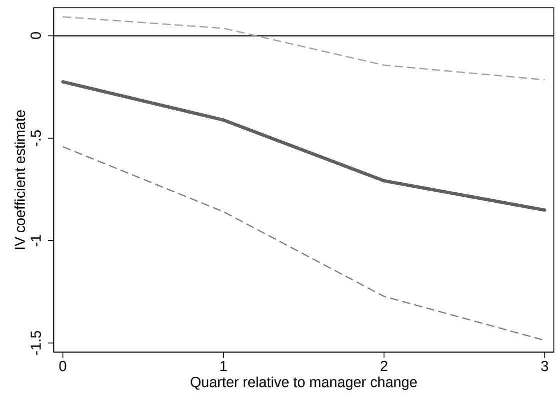
(a) Attrition



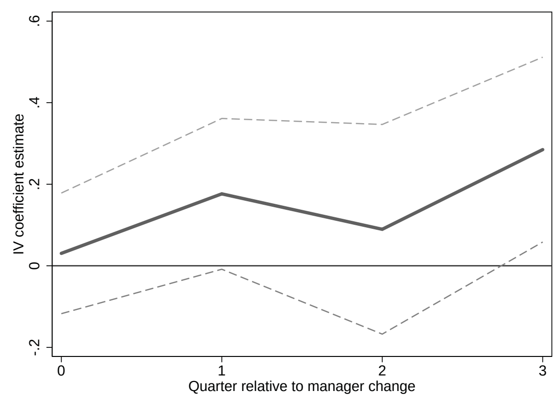
(b) Quits



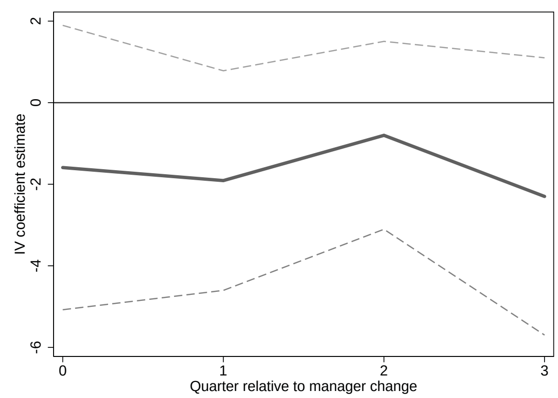
(c) Fires



(d) Regretted Quits



(e) Non-regretted Quits



(f) Worker changes manager

Notes: Dotted line shows 90% confidence interval 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 difference is that instead of using MOR, we use MOR interacted with quarters 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.

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
Co-located with manager	0.81
Same function as 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

Panel B: Several outcomes and regressors of interest

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	3.33	0.82	1	5
Log salary	Confidential			
Promotion probability (monthly)	Confidential			
Manager overall rating (MOR)	80.86	15.27	15	100
Manager gives clear expectations	84.23	13.5	0	100
Manager provides coaching	76	17	0	100
Manager supports career dev	77.7	16.31	0	100
Manager involves people	84.27	14.36	0	100
Manager instills poz attitude	82.95	15.57	0	100
Manager is someone I trust	82.49	15.03	13	100

Notes: This table presents important summary statistics regarding our sample. The data are at the employee-month level. The number of observations is withheld to protect firm confidentiality. 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 further detail on sample construction, see Appendix B. Appendix Table C1 repeats this table using the data before imposing the restriction that MOR be non-missing for the current manager in both surveys.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Variables:	Overall MOR	Clear expectations	Coaching	Career dev	Involves people	Positive attitude	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	0.233	0.183	0.205	0.224	0.179	0.251	0.205

Notes: Robust standard errors in parentheses. An observation is a manager. Each column regresses a 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 controls for business unit, dummies for year of hire (observations before 2001 are lumped in one year), salary grade dummies, 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 2.3. * 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 whether an employee attrites in a given month. In the other panels, the dependent variable is whether an employee experiences a particular type of attrition event in a given month. 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. * 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). * 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). * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Rothstein Test: 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	Employee engagement (normalized)	Log stock grant holdings x100	Key individual (x100)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
MOR of future manager	0.037*	-0.776**	0.294*	0.170	0.018*	2.326	0.785
measured in 2nd period	(0.023)	(0.346)	(0.164)	(0.139)	(0.010)	(1.553)	(0.643)
Panel B: IV							
MOR of future manager	0.121	-0.574	0.795	0.176	0.063*	8.495	-0.450
measured in 2nd period	(0.088)	(1.198)	(0.781)	(0.479)	(0.037)	(8.381)	(1.636)
Panel C: Red. Form							
MOR of future manager	0.034	-0.178	0.243	0.049	0.018*	1.571	-0.126
measured in 1st period	(0.025)	(0.373)	(0.240)	(0.135)	(0.010)	(1.512)	(0.459)

Notes: Standard errors clustered by 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 employees who experience a 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. 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	-0.305***		-0.572**	
	(0.100)		(0.229)	
MOR of current manager in 2nd period		-0.216**		-0.680***
		(0.092)		(0.204)
Mean dep. var.	1.663	1.663	1.663	1.663
F-stat on excl instrument			32.46	38.48
Panel B: Quits				
MOR of current manager in 1st period	-0.108*		-0.173	
	(0.061)		(0.136)	
MOR of current manager in 2nd period		-0.065		-0.241*
		(0.057)		(0.125)
Mean dep. var.	0.913	0.913	0.913	0.913
F-stat on excl instrument			32.46	38.48
Panel C: Fires				
MOR of current manager in 1st period	-0.115**		-0.129	
	(0.052)		(0.100)	
MOR of current manager in 2nd period		-0.049		-0.257**
		(0.040)		(0.104)
Mean dep. var.	0.223	0.223	0.223	0.223
F-stat on excl instrument			32.46	38.48
Panel D: Regretted Quits				
MOR of current manager in 1st period	-0.043		-0.192	
	(0.055)		(0.125)	
MOR of current manager in 2nd period		-0.073		-0.096
		(0.050)		(0.110)
Mean dep. var.	0.727	0.727	0.727	0.727
F-stat on excl instrument			32.46	38.48
Panel E: Non-regretted Quits				
MOR of current manager in 1st period	-0.067**		0.016	
	(0.031)		(0.071)	
MOR of current manager in 2nd period		0.006		-0.150**
		(0.029)		(0.068)
Mean dep. var.	0.186	0.186	0.186	0.186
F-stat on excl instrument			32.46	38.48

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. Instead of controlling for current year, we control for period. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: MOR and Non-Attrition Outcomes

Dep. Var.	Subjective performance (normalized)		Log Salary Growth (x100)		Promotion (x100)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
MOR (normalized)	0.037***	-0.009	0.123	0.067	0.072	-0.048
	(0.007)	(0.008)	(0.079)	(0.102)	(0.048)	(0.116)
Panel B: IV						
MOR (normalized)	0.095***	0.032***	0.064	0.157	-0.020	0.014
	(0.023)	(0.012)	(0.205)	(0.170)	(0.135)	(0.225)
Panel C: Red. Form						
MOR (normalized)	0.031***	-0.015**	0.022	-0.086	-0.006	-0.007
	(0.007)	(0.007)	(0.071)	(0.115)	(0.044)	(0.107)
Employee FE	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered by manager in parentheses, with the exception of the even columns of Panel B, where standard errors are calculated via block bootstrap (50 replications), with subsampling over managers. We do only 50 replications because the IV with fixed effects is computationally demanding. The controls are the same as in Table 3. "Subjective performance" is an employee's subjective performance on a 1-5 scale. In columns 3-4, "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. In columns 5-6, the outcome is whether an employee receives a promotion, with coefficients multiplied by 100 for readability. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: What are Managers Rewarded For? Employees Survey Scores (MOR)

Dep var:	Subjective performance (normalized)	Promoted (x100)	Log salary (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)	(8)
Panel A: OLS								
MOR in current period	0.0641***	0.101	-0.479	0.135	-2.685*	0.277	0.0637	-0.895
	(0.0225)	(0.0899)	(0.403)	(0.162)	(1.543)	(3.190)	(0.0937)	(0.863)
Panel B: IV								
MOR in current period	0.419***	0.673**	-2.381	1.405**	-0.670	5.874	0.260	2.078
	(0.0870)	(0.311)	(1.505)	(0.627)	(5.252)	(9.943)	(0.303)	(2.696)
Panel C: Red. Form								
MOR in other period	0.138***	0.221**	-0.743*	0.462**	-0.216	1.882	0.0843	0.682
	(0.0222)	(0.0950)	(0.448)	(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. "Subjective performance" is a manager's subjective performance on a 1-5 scale. "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 sotck 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. * significant at 10%; ** significant at 5%; *** significant at 1%