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PROMOTIONS AND THE PETER PRINCIPLE

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ABSTRACT

The best worker is not always the best candidate for manager. In these cases, do firms promote the best potential manager or the best worker in her current job? Using microdata on the performance of sales workers at 214 firms, we find evidence consistent with the "Peter Principle," which predicts that firms prioritize current job performance in promotion decisions at the expense of other observable characteristics that better predict managerial performance. We estimate that the costs of promoting workers with lower managerial potential are high, suggesting either that firms are making inefficient promotion decisions or that the benefits of promotion-based incentives are great enough to justify the costs of managerial mismatch.

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When management requires different skills than lower-level work, the best workers may not make the best managers. In these cases, do firms promote someone who excels in her current position or someone who is likely to excel as a manager? If firms promote workers based on their current performance, they may end up with worse managers. Yet if firms promote workers based on traits that predict managerial potential, they may pass over higher-performing workers, thereby weakening incentives for workers to perform well in their current roles. Such promotion policies could also lead to perceptions of favoritism or unfairness, or the impression that effort in one's job goes unrewarded.

Using detailed microdata on sales workers in US firms, we provide the first large-scale empirical evidence showing that firms prioritize current performance in promotion decisions at the expense of promoting the best potential managers. Our findings are consistent with the "Peter Principle," which, in its extreme form, states that firms promote competent workers until they become incompetent managers (Peter and Hull 1969). In particular, we show that high-performing sales workers are more likely to be promoted, but that prior sales performance negatively predicts managerial performance, even after accounting for selection into the sample of promoted workers. These results suggest either that firms make mistakes in their promotion decisions or that the incentive benefits of promoting based on sales performance justify the costs of promoting workers with lower managerial potential. We provide supportive evidence for the latter possibility by showing that firms appear to actively manage the trade-off between providing incentives and promoting the best potential managers: firms place less emphasis on sales performance in promotions where managerial roles entail greater responsibility and where sales performance is also rewarded by relatively strong pay-for-performance.

The Peter Principle applies broadly to settings in which the skills required to succeed at one level in the organizational hierarchy may differ from the skills required in the next level, e.g., science, engineering, manufacturing, academia, or entrepreneurship (Baker, Jensen and Murphy 1988). Among such settings, sales is particularly attractive from a research perspective for several reasons. First, it is an economically important occupation, accounting for 10.4 percent of the US labor force.[1] Second, sales performance is relatively easy to quantify, allowing us to observe a more reliable measure of worker performance. Finally, the sales setting allows us to explore an interesting tension. Sales is both widely cited as a

[1]In 2016, the U.S. labor force had 14.5 million workers in sales and sales-related occupations (Bureau of Labor Statistics 2016).

canonical example of where the Peter Principle is likely to apply[2] and a setting in which a simple economics model would predict that the Peter Principle may be less likely to apply: pay-for-performance is common in sales, so firms may not need to rely on promotion-based incentives to induce worker effort. Finding evidence of the Peter Principle in such a setting would suggest that pay-for-performance cannot fully resolve the tension between providing incentives and promoting the most qualified managers.

Our analysis uses new transaction-level data that are well suited for the study of firms' promotion policies.[3] These data, provided by a firm that offers sales performance management software to client firms, include standardized measures of sales transactions and organizational hierarchy for a panel of 53,035 workers, 1,531 of whom are promoted into managerial positions during our sample period. Our data cover 214 different US-based client firms in a range of industries from 2005 to 2011. Data on multiple firms allow us to study heterogeneity in how much firms prioritize current job performance as a function of firm organization or pay practices.

For sales workers, we use employment history and sales credit data to examine promotion as a function of sales performance (the dollar value of sales), sales collaboration (the number of colleagues with whom a worker shared credit on transactions), and other observable worker characteristics. For promoted managers, we evaluate their managerial performance as their "manager value added" in shaping their subordinates' sales performance, i.e., each manager's contribution to improving her subordinates' sales, controlling for subordinate and firm-year-month fixed effects, as well as other potentially confounding factors (following the methods used in, e.g., Abowd, Kramarz and Margolis 1999; Bertrand and Schoar 2003; Lazear, Shaw and Stanton 2016). While this measure of manager value added has a number of important advantages, it remains imperfect given that managers may be non-randomly matched to subordinates. Thus, we present a number

[2]Deutsch (1986) points out that "American companies have always wrestled with ways to keep the Peter Principle at bay—to prevent competent salesmen, for example, from rising to become incompetent sales managers." Baker, Jensen and Murphy (1988) state that "in many cases, the best performer at one level in the hierarchy is not the best candidate for the job one level up—the best salesman is rarely the best manager." Sevy (2016), in a Forbes article entitled "Why Great Sales People Make Terrible Sales Managers," argues that great sales workers are motivated by a desire for personal—rather than team—achievement: "success in sales is about me while success in sales management is about my team. This is where the downside of a strong achievement drive makes itself known. If I'm driven to prove my personal ability, I find it hard (nearly impossible sometimes) to step back and let others take the spotlight."

[3]We do not observe promotion offers. As such, when we refer to a "promotion policy," we refer to the combined impact of the firm's promotion offer and the worker's decision to accept the offer.

of additional tests in Section 5 to show that non-random matching is unlikely to drive our findings of evidence consistent with the Peter Principle.

We begin by documenting a strong, positive relation between past sales performance and promotion to managerial positions. Doubling sales credits increases the probability that a worker will be promoted by 14.3 percent relative to the base probability of promotion.[4] However, among promoted workers, we find that pre-promotion sales performance *negatively* predicts manager value added: doubling the new manager's pre-promotion sales corresponds to a 7.5 percent decline in the sales performance of each of the newly-promoted manager's subordinates. Equivalently, relatively poor prior sales performance among newly promoted managers is associated with significant improvements in subordinate performance. This negative correlation is consistent with the Beckerian insight about selection and discrimination: if firms' promotion policies "discriminate" against poor sales performers, then poor sales performers who are nevertheless promoted should be better managers.

Our analysis also identifies another observable worker characteristic, sales collaboration experience, which is positively related to managerial performance but not consistently correlated with promotions. Sales collaboration may be a measure of a worker's experience working in teams or with more complex or customized products that require coordination. In either case, these results suggest that firms wishing *only* to maximize managerial quality could potentially achieve better outcomes by placing more weight on collaboration experience, rather than sales, when making promotion decisions.[5]

Next, we show that these findings are robust to selection concerns. Because workers are not promoted at random, the relation between sales and managerial performance in the promoted sample may not reflect the true relation in the full sample of workers. To recover the full-sample predictive relation, we apply a model of promotions based on the Heckman (1976) selection model. We instrument for promotion, i.e., selection into the sample of promoted workers, using the average firm- and industry-level promotion rates within each time period. These average promotion rates reflect time-varyinlg vacancies and firm- and industry-level demand for managers, which are strongly positively correlated with a

[4] A doubling of a worker's relative sales performance is not an unusual occurrence given the wide dispersion in sales across workers. It is equivalent to a worker moving from the 50th to the 67th percentile in terms of sales relative to others in the same firm-year-month.

[5] We caution that our results hold in the sense that collaboration experience under the existing set of promotion policies positively predicts managerial value added. If firms began to heavily weight collaboration experience in promotion decisions, workers could potentially add fake collaborators by sharing credits. Recent examples of the gaming of various evaluation metrics include Benson (2015); Larkin (2014); Oyer (1998).

worker's probability of promotion. To satisfy the exclusion restriction, these average promotion rates must be uncorrelated with managerial performance. In general, this may not be true: high average promotion rates may reflect strong consumer demand or other time-varying firm shocks that affect the performance of all sales workers and may thus be correlated with managerial performance. In our setting, however, we measure a manager's quality as his or her value added to subordinate sales, net of worker and firm-year-month fixed effects. Because our instruments only vary across firm-year-months, they are—by construction—orthogonal to our measures of manager value added, which are extracted from a regression that controls for firm-year-month fixed effects. After accounting for selection, we continue to find a strong, negative predictive relation between pre-promotion sales performance and manager value added.

In addition to addressing potential selection biases, we also provide evidence in Section 5 that our results are not driven by other potential measurement issues such as mean reversion, non-linear relations, or the unwillingness of some top sales workers to accept promotion offers. We further test whether managers with high pre-promotion sales contribute to the firm in other ways, such as by retaining more skilled subordinates, and find no evidence that high-sales managers are better on these dimensions.

We next assess the magnitude of the costs associated with promoting high-performing sales workers at the expense of promoting the best potential managers. To do this, we compare the predicted managerial performance of actually promoted workers to the predicted managerial performance of workers who would have been promoted under a counterfactual policy in which firms weigh worker observables solely to maximize managerial quality. We find that average managerial quality, as measured by value added to subordinate sales, is 30 percent higher under the counterfactual policy.

We emphasize here that finding evidence of the Peter Principle does not imply that firms make mistakes. Alternative promotion policies that maximize managerial match quality may lead to the loss of incentive benefits associated with existing promotion policies. Indeed, a promotion policy that favors top sales workers may provide a variety of incentive benefits that justify the costs of managerial mismatch. For example, such policies may preserve tournament incentives (Lazear and Rosen 1981), especially if workers strongly value the security, stature (e.g. DellaVigna and Pope 2016; Larkin 2011), or external signaling abilities associated with promotions (DeVaro and Waldman 2012). Prioritizing objective performance measures in promotions may also improve incentives by avoiding favoritism (Fisman et al. 2017; Prendergast and Topel 1996) and maintaining pay

equity and fairness norms (e.g., Larkin, Pierce and Gino 2012). Promotion policies based on verifiable performance metrics such as sales may also discourage the manipulation of other, more fungible performance metrics such as credit sharing and collaboration experience (DeVaro and Gürtler 2015; Fisman and Wang 2017). What our findings do suggest is that the costs of not promoting the best potential managers are high: firms appear willing to forgo a 30 percent improvement in subordinate performance to achieve better incentives or to avoid costly politicking.

Our final two results explore how firms trade off the benefits of using promotion-based incentives against the costs of managerial mismatch. If firms consciously choose to provide strong incentives at the expense of selecting the best managers, we would expect to find more evidence of the Peter Principle in settings in which promotion-based incentives are relatively important and less evidence in settings where managerial quality is relatively important.

We first examine how promotion policies vary with differences in managerial responsibility. In particular, the cost of promoting bad managers is likely to be higher when managers lead large teams, and so we would expect firms with larger teams to place less weight on sales performance and more weight on collaboration experience in their promotion choices. This is indeed what we find in our data: when the quality of managers is relatively more important, firms appear more willing to promote workers who are weaker in terms of pre-promotion sales but more likely to be effective managers.

Similarly, firms should place less weight on current performance in promotions if they have chosen to incentivize worker performance in other ways. For a subset of our data, we observe both variable and fixed worker compensation, which allows us to measure how much each firm uses explicit pay-for-performance. We find that firms with stronger pay-for-performance—defined as those in which commissions and bonuses are large on average relative to fixed compensation—put less weight on sales performance when making promotion decisions. While this finding suggests that pay-for-performance can partially substitute for promotion-based incentives, it does not imply that pay-for-performance can eliminate the costs associated with the Peter Principle. Indeed, relative to other occupations, sales is associated with high pay-for-performance, and yet we continue to find evidence consistent with the Peter Principle. This again suggests that the incentive power of promotions may be quite important in practice. Pay-for-performance may not always be the most cost-effective way to motivate workers (Cullen and Perez-Truglia 2018),[6] and

[6]Cullen and Perez-Truglia (2018) use experimental evidence in the field to show that horizontal pay inequality (which could result from pay-for-performance) can demotivate workers, whereas vertical pay inequality (which could result from promotion-related incentives) can motivate workers.

workers may additionally value managerial titles associated with promotion because they confer status and can be readily advertised on resumes (DeVaro and Waldman 2012; Waldman 2003).

In general, our results do not allow us to reject the possibility that firms make mistakes in their promotion decisions. However, they do suggest that firms are aware of the trade-off between providing worker incentives and selecting good managers, and attempt to set their promotion policies accordingly.

This study offers the first empirical test of the Peter Principle using data on promotions across a large number of firms. Although theoretical work and reviews have hypothesized that promotions based on current job performance may yield managerial mismatch (Fairburn and Malcomson 2001; Lazear 2004; Waldman 2003), there is little empirical work that tests the Peter Principle directly. Our paper is most closely related to Grabner and Moers (2013), which shows that a bank places less weight on current job performance when a promotion would be to a job performing dissimilar tasks. However, Grabner and Moers use data from a single firm and do not attempt to estimate the overall cost of the Peter Principle.

Our analysis is motivated by research showing that managerial quality is an important determinant of firm productivity (e.g., Bloom and Reenen 2007). A large related literature on corruption in leadership dating back to Weber (1947) attributes the existence of bad leaders to selection policies that are polluted by nepotism and cronyism. However, our findings suggest that promotion policies that are more meritocratic or "fair" may still be problematic, as promoting based on merits in the current job—rather than on managerial potential—may still result in bad leaders.

Our analysis is also related to recent findings in Kaplan, Klebanov and Sorensen (2012) and Kaplan and Sorensen (2016) showing that execution ability, interpersonal skills, and other general skills are associated with performance in executive roles. These findings underscore the possibility that promoting based on lower-level job skills rather than managerial skills can be extremely costly. Our findings also relate to the literature exploring the declining popularity of internal promotions and the rising popularity of external managers or directors (e.g., Murphy and Zabojnik 2004). The decision to hire an external manager must weigh the benefits of expanding the field of candidates to improve the quality of an eventual match against the costs of reducing incentives for internal candidates. The Peter Principle may also be very relevant for entrepreneurial firms, which must decide whether to keep founders and early team members in leadership roles as the

business scales or bring in external talent whose skills may be better suited to management (Ewens and Marx 2016; Hellmann and Puri 2002).

1 Setting and data

Our data come from a firm that offers sales performance management (SPM) software over the cloud. The firm's clients input their employee records, organizational hierarchies, and sales transactions into the software, which then calculates pay for each individual worker. Transaction inputs can be entered manually or linked to order management and customer relationship management software. Pay outputs are typically linked directly to payroll software. The software also provides reporting and analysis. Sales workers and sales managers can view their sales credits, progress toward quotas, commissions, and other data. The software can also generate reports for use in auditing and compliance with Sarbanes-Oxley.

The data include 214 client firms and 53,035 sales workers, 1,531 of whom are promoted to managerial roles. The most-represented industries include manufacturing (62 firms), information technology and services (56 firms), and professional services (38 firms). In 2011, sales workers in the US earned a median monthly wage of $2,070 and about half worked in retail sales (Bureau of Labor Statistics 2011). By contrast, sales workers in our data predominantly work in business-to-business sales and earn a median monthly commission of $3,658, not including base salary and bonuses. Table 1 provides descriptive statistics for sample coverage. All firms have at least one complete fiscal year of data, and no one firm constitutes more than 8 percent of person-months.

1.1 Overview of sales positions

Sales workers are typically assigned a market consisting of a territory, a set of products, or a type of client. Within their market, they are responsible for generating leads on potential new clients, making first contact, executing the initial sale, cross-selling other products, selling upgrades, and maintaining relationships. The sales industry refers to this process as the sales cycle.

The primary measure of a salesperson's performance is the total dollar value of the sales to which he or she contributes. Our data include 156 million sales transactions tied to individual workers. Table 1 describes the distribution of sales generated. Because sales tend

to be intermittent and can vary over the year, we report rolling averages of sales credits in the previous 12 months. The quartiles for monthly worker sales are \$49,956, \$285,573, and \$1.54 million (in 2010 dollars). Reflecting the wide and skewed distribution of sales standards across markets in which workers operate, the mean of this figure is \$3.62 million.

Figure 1 also illustrates the skewness in the distribution of sales. The top left panel presents a histogram for the raw distribution of worker-level monthly sales (measured as 12 month rolling averages). The middle left panel plots the log of monthly sales, and shows that this follows a less skewed distribution. The bottom left panel, which reflects our main measure of sales performance, shows the residual distribution of monthly sales after controlling for firm-year-month fixed effects. In other words, we measure sales performance as the recent performance of a sales worker compared to others in her same company at the same period in time. Even with these fixed effects, we still observe wide variation in sales credits across workers. The interquartile range of residual log sales is 2.49, meaning that, among rank and file sales workers in the same firm in the same year-month, a worker in the 75th percentile generates approximately $e^{2.49} = 12.1$ times as much revenue as one in the 25th percentile. Although this difference is stark, it's also consistent with the so-called "80-20 rule," a well-known heuristic in the sales industry that states that the top 20 percent of the sales force is responsible for 80 percent of sales.

In addition to total sales, we also observe collaboration experience. In the potentially complex business-to-business sales settings that constitute the majority of our data, sales transactions are often credited to more than one worker. For example, a relationship manager may be a client's single point of contact. For specialized products and services, the relationship manager may consult a product specialist, and, if a sale is made, both the relationship manager and the product specialist would receive a credit. For complex products and services, a single transaction can involve salespeople across many sales functions, products, and geographies. In our data, we observe all workers credited on a transaction and define a salesperson's collaboration experience as her average number of distinct colleagues per order over the past 12 months (or for her tenure if less than a year). A salesperson's collaboration history can serve as a proxy for both experience in working in teams and experience with more complex products that require more coordination to sell.

Table 1 presents summary statistics for collaboration, and Figure 1 presents histograms of the distribution of the mean number of collaborators a sales worker has per order, over the past year (or her tenure if less than a year). Over 40 percent of workers worked alone in the past year, while the remainder vary greatly in their number of collaborators. This

difference does not merely reflect differences in work organization across firms or over time. The bottom-right panel of Figure 1 shows that even within the same firm-year-month, there is substantial variation in the extent to which workers collaborate on sales. The within firm-year-month interquartile range of sales collaborators is 1.19, signifying that the 75th percentile worker has $e^{1.19} = 3.28$ times as many collaborators as the 25th percentile worker.

This variation highlights two archetypal sales workers: those who are the only person credited on transactions and those who share sales credits on transactions. Indeed, much of the practitioner literature emphasizes different performance management practices for these groups. "Lone wolves" might be recruited for their self-confidence, resilience, and autonomy, and are stereotypically marked by their reluctance to share leads, best practices, and client relationship responsibilities with others in the organization. The most effective team players, by contrast, enable those around them by forwarding leads, crafting sales that include many others' territories and products, forwarding established clients to account managers, and developing team members so they can be effective in these capacities. These lead generation and origination activities would also generally entitle that salesperson to a portion of the sales executed by others.[7]

The correlation between our sales and collaboration measures is 0.21. While positive and statistically significant, the moderate correlation shows that there is substantial variation across these measures: workers can be effective salespeople under various models of collaboration.

Table 1 also provides summary statistics for worker compensation. Because our data provider's software is designed to track and distribute pay for sales performance, salary is an optional field and can be missing or measured with error. Based on these limited data, we believe that the median worker in our sample receives at most $89,000 in base pay per year, and more likely $50,000 to $60,000 per year in base pay, which is approximately half that of managers.[8] Given that the software outputs commission data that are often linked to payroll,

[7]We do not assume that collaboration experience is freely chosen by the worker. Indeed, some workers may be assigned to work alone or in teams. We instead focus on showing that collaboration experience, which is observable by the firm, positively predicts manager value added.

[8]The salary figures reported in Table 1 are based on data that are sometimes unclear in terms of units. Given the manner in which the software interprets these fields on online dashboards and other automated reports, we believe that the majority of salaries are reported in monthly terms. However, some salary fields may be populated with annual salaries, and some workers with missing salary data may have missing salary data precisely because they work fully on commission. We expect the latter bias to affect workers more than managers, since conversations with our data provider suggest that managers are very rarely paid entirely on commission. These two biases operate in the same direction and lead us to believe that the true median monthly salary is lower than the median $7,443 figure reported in this table.

we're more confident in these measures, although they can still be missing. The median sales worker earns $3,658 per month in commission pay, slightly less than our estimates of workers' base pay, and the 75th percentile sales worker earns the same in commission as in base pay. These numbers are generally consistent with benchmark data for the relative sizes of compensation for sales workers. However, total compensation in our data is substantively greater than BLS estimates for median pay among non-retail sales workers, which ranges from $48,200 to $71,550 per year. This is unsurprising given that our sample is largely composed of highly-compensated sales workers who engage in big-ticket business-to-business sales.

Our analysis uses monthly sales as the measure of pre-promotion sales performance, which has the advantage of being highly standardized, and after controlling for firm-year-month fixed effects, has an easy interpretation. A limitation of our sales performance measure is that we do not observe the profit margins associated with sales transactions. Nevertheless, we believe that the relative levels of sales credits among workers in the same firm and time offers a reasonable approximation of relative sales performance, and in the data, these sales mechanically determine the ultimate commissions. In theory, we could instead use worker compensation as a measure of sales performance. However, this approach would also have disadvantages. First, compensation can be difficult to interpret because firms can "pay for performance" by paying a high base rate and setting high standards for retention. Second, compensation doesn't always correspond to recent performance; for example, in a given month, workers may receive commissions for origination or renewals for sales that were made in the distant past. Third, the base pay data can be unreliable since they are not required by the software and not directly linked to payroll. Therefore, we prefer relative sales credits as our measure of sales performance.

While our data have the unique advantage of offering detailed organizational structure and worker productivity measures, we unfortunately do not observe employee demographic characteristics such as age, gender, or education. We do observe worker tenure, which may affect both worker sales and promotion prospects. The tenure variable is censored by the date the firm began using the SPM software. Therefore, we control for tenure within the SPM system and its interaction with whether tenure is potentially censored.

1.2 Overview of managerial positions

In our data, we observe the hierarchical structure linking sales managers to sales subordinates. For each person in the data, we observe the ID number of at most one direct

superior within the hierarchy, as well as the ID numbers of any direct subordinates. Therefore, we define a worker as someone with zero subordinates and a manager as someone with at least one subordinate. Some managers are higher up in the hierarchy, and have subordinates who manage other subordinates. While it would be very interesting to study promotions from managerial positions into higher level managerial positions, we observe relatively few promotions into higher levels of management. Therefore, we focus our analysis on front-line sales workers (those at the bottom of the sales hierarchy), and managers one level above these front-line sales workers.

Managers typically have titles such as "territory manager," "sales director," "regional director," "regional manager," and "regional vice president." The last panel of Table 1 summarizes the characteristics of managers in our data. On average, each manager has five subordinates. Conversations with our data provider suggest that managers typically receive greater total compensation than their subordinates and have a pay mix that favors base pay rather than commission pay. Consistent with this, managers in our data have significantly higher reported salaries than workers on average and at each quartile of the pay distribution. In absolute terms, managers also have greater commissions than workers at each quartile of the commission pay distribution, though managers' overall pay mix is more weighted toward base pay. In addition, nonpecuniary rewards are also likely to favor managers, who typically enjoy greater prestige, opportunities for career progression inside and outside the firm, benefits, job security, pay security, and better work conditions than their subordinates. Nevertheless, the top salespeople in our data earn more in commissions than the median manager. This raises the possibility that some top sales workers in our sample may prefer not to be promoted to managerial positions. We'll return to this potential selection concern in the empirical section.

Managers also perform substantially different tasks. While sales workers are primarily engaged in direct sales activities, sales managers are responsible for building a high-performing sales team and earn commissions as a function of their team's performance. A survey of front line sales managers by the Sales Management Association (2008) reports that sales managers spend the most time on performance management, followed by company administration, sales planning, selling and market development, and staff deployment. Performance management requires leadership, coaching, and training skills that may be imperfectly related to those used in direct sales activities. Administrative duties require general management knowledge so that the sales manager can interface with other functions, such as marketing and operations. Sales planning

requires data analysis skills so that managers can read market research, set quotas, assign territories, monitor performance, and prioritize sales activities. Sales managers also oversee the development of playbooks that compile best practices and outline the company's strategy for selling their products. Successfully executing these activities reflects in the performance of their teams. For example, if the manager misreads market research, sales workers could be misallocated to unproductive products or territories, quotas could be set at unattainably demotivating thresholds, or training could encourage salespeople to emphasize the wrong product features for their market.

Measuring manager quality

Because sales managers are ultimately responsible for improving the performance of their subordinates, we measure managerial performance as the impact of the manager on the sales of his or her subordinates. In general, any measure of managerial performance that relies on subordinate performance may be biased by the non-random assignment of managers to subordinates. For example, if a manager is assigned to high-performing subordinates, the high sales numbers for these subordinates should not be attributed to the manager's skill. Indeed, Table 8, presented in later sections, shows that managers with high pre-promotion sales tend to be assigned to subordinates who had relatively high sales under their previous managers.

To address these concerns, we follow Lazear, Shaw and Stanton (2016), as well as Hoffman and Tadelis (2018) and a large literature on employer-employee, and teacher-student matched data (e.g., Abowd et al. 2001), and estimate a manager's performance as his or her *value added* from a regression of the form:

$$\text{Log}(1 + \text{Sales}_{imft}) \quad = \quad a + \delta_i + \delta_m + \delta_{f\times t} + X_{it} + e_{imft} \tag{1}$$

Here, the dependent variable is the log of worker i's sales performance under manager m in firm f in year-month t. δ_i, δ_m, and $\delta_{f\times t}$ represent worker, manager, and firm-year-month fixed effects, respectively.[9] X_{it} represents worker-time characteristics, which in our setting represents seven bins for worker tenure each interacted with an indicator for whether tenure is potentially censored. The coefficients of interest are the manager fixed effects, δ_m, which is the average, time-invariant component of a manager's quality or value added.

[9]We estimate this regression using the Stata package `felsdvreg`. Rather than estimating $\delta_{f\times t}$ directly, we demean the outcome variable by firm-year-month prior to estimation in order to reduce the computational demands of the regression specification.

By including both manager and worker fixed effects, manager value added is identified from workers whom we observe under multiple managers. A manager's fixed effect represents the average change in sales performance across all workers who switch to or from that manager. As such, a manager with a high value added is one under whom workers perform above their individual mean across all the managers under whom they have worked. Whether a manager is assigned to strong or weak subordinates should not impact our measure of value added because a manager is credited only for *changes* in the performance of her subordinates. Further, firm-year-month fixed effects net out macroeconomic, industry-specific, and other firm-time specific conditions that may impact subordinate sales performance.

Estimating managerial quality as the manager's value added as in Equation (1) has clear advantages, as described above. However, we also acknowledge that it is an imperfect measure. First, our estimates of manager value added are likely to be noisy. In Equation (1), the dependent variable is worker monthly sales, which varies widely, with some workers making zero sales in some months and large sales in others.[10] Classical measurement error in worker sales will add noise to our measures of manager value added. However, our tests of the Peter Principle will regress manager value added on each manager's pre-promotion sales experience. Noise in manager value added (the dependent variable in the regression) will reduce our statistical power but should not bias our estimated coefficients.

One may still be concerned, however, that our estimates of manager value added are systematically biased. This could happen if managers are non-randomly assigned to subordinates on the basis of *time-varying* subordinate performance. For example, if some types of managers tend to be assigned to subordinates whose sales performance is on an increasing trend, then Equation (1) may mistakenly attribute subsequent sales gains by those employees to the manager's ability. Our estimates may also be biased if workers are assigned to managers based on match-specific quality; in this case, we could not interpret a manager's estimated value added as his or her value added for the average worker. Instead, the worker's change in sales could be due to the quality of the match between the worker and the new manager or team.

However, our tests of the Peter Principle remain valid even with the existence of such biases in the estimates of manager value added, as long as the biases in value added are not

[10] In later regressions where we test how sales performance predicts promotions, we used backward rolling averages of sales to smooth this variation, but we do not use backward rolling averages in Equation (1) because we are interested in measuring changes in performance as workers move across managers; backward averages would contaminate measures of manager value added by including worker performance under previous managers.

related to the manager's pre-promotion sales performance. In particular, our main test of the Peter Principle will regress estimates of value added on the manager's pre-promotion sales performance. For example, non-random assignment of some managers to workers on upward time trends in performance will not impact our conclusions as long as this non-random assignment to time-varying worker performance is uncorrelated with a manager's pre-promotion sales.

In Section 5, we test for non-random assignment directly and show that managers are not assigned to workers with trending sales performance on the basis of the managers' pre-promotion characteristics. Further, if it were the case that we mistakenly assigned some managers higher value added on the basis of their subordinates' time-trends in performance, then we would expect there to be a correlation between a manager's estimated value added and the recent prior performance of his or her new subordinates. In Section 5, we show that this is not the case.

Summary statistics: manager quality

Table 2 presents summary statistics for our estimates of manager value added. We observe 6,515 managers in our data, of whom we are able to estimate fixed effects for 4,887. This lowered number comes from the high bar required to identify manager fixed effects: to estimate a fixed effect for a given manager, we must observe that manager supervising multiple subordinates whose own fixed effects are known through their work under other managers. Many managers in our sample do not have subordinates whose performance we observe under other managers.

Our sample is also constrained to managers within groups of workers and managers who are connected through moves. For instance, a connected group might contain a manager, her new subordinates, the previous managers of those subordinates, and the other subordinates of those managers. Fixed effects for managers within the same connected group are comparable relative to a group-specific normalization. For the average firm in our sample, 76.5 percent of workers are part of this largest connected group. To make these fixed effects more comparable across firms, we further demean them by firm specific averages. Because we estimate manager fixed effects with varying precision, we weight summary statistics and regressions involving these fixed effects by the inverse variance of our estimates.

Finally, to estimate the relation between pre-promotion characteristics and post-promotion managerial performance, we must further restrict the sample to observed

promotions. We have information on both manager value added and pre-promotion characteristics for 1,028 managers who are promoted during our sample period.

Figure 2 plots the distribution of our estimates of manager value added. By construction, manager value added has a mean of zero. The 25th percentile of this distribution is -0.71, implying that, when assigned to a 25th percentile manager, a worker's output is $e^{-0.71} = 0.49$ of what it would have been under the mean manager. Conversely, when assigned to a 75th percentile manager, a worker's output increases by a factor of $e^{0.85} = 2.34$.

Note that this interquartile range may be large because it reflects real differences in managerial performance or because of noise in the estimation of manager fixed effects, which exaggerates the variance.[11] Our analysis will regress these manager fixed effects on variables representing the managers' pre-promotion characteristics. Error in the dependent variable in these regressions should not bias our estimates of how *mean* differences in manager value added relate to pre-promotion characteristics. However, this additional noise raises the model's standard errors and upwardly biases our estimates of the *variance* of manager fixed effects.

2 What predicts promotion?

Our first empirical exercise examines how the sales and collaboration experience of front line sales workers predict promotion to management:

$$\text{Promote}_{ift} = a_1\text{Sales}_{ift} + a_2\text{Collaborators}_{ift} + W_{ift} + \delta_{f\times t} + e_{ift} \tag{2}$$

We estimate a probit model for Equation (2) on a worker-year-month level panel for worker i at firm f who has not yet been promoted as of year-month t in which at least one worker at the firm is promoted. The dependent variable, Promote_{ift}, is an indicator for whether a worker is promoted in the next month. Sales_{ift} is the log of one plus worker i's monthly sales credits, averaged over the past 12 months or over the worker's total tenure if it spans fewer than 12 months. Because sales varies significantly across firms and with market conditions, we demean worker sales by the mean in each firm-year-month. Thus, Sales_{ift} represents each worker's sales performance relative to other workers in the firm during the same time

[11] As discussed earlier, regressions presented in later sections that estimate the mean expected difference in manager value added across managers with different pre-promotion attributes remain unbiased because noise in the dependent variable should not bias regression coefficients, though they may increase standard errors.

period. Collaborators$_{ift}$ is the log of one plus worker i's average number of collaborators per order, again averaged over the past 12 months or over the total tenure if it spans fewer than 12 months. The other covariates W_{ift} include fixed effects for seven bins of worker tenure, interacted with whether tenure may be censored in the data. Some specifications also control for the industry-wide and firm-wide promotion rates in the current month.

Equation (2) estimates the determinants of firm "promotion policies," which we use as an umbrella term for the ultimate outcome in terms of which workers transition into managerial positions. We caution that firm "promotion policies" refer to more than the firm's choice of which workers to offer promotion opportunities, it also depends on the terms of the promotion offer and whether workers accept. We present a detailed discussion of non-random selection into the sample of promoted workers in Section 3.1.

Table 3 reports the regression results. We find that firms are significantly more likely to promote higher performing salespeople. This result is robust across specifications that control for a worker's pre-promotion tenure and the industry- or firm-level promotion rates in each year-month. The estimate in Column 4 implies that a doubling of a worker's relative sales performance corresponds to a 0.030 percentage point increase in a worker's probability of being promoted, or a 14.3 percent increase relative to the base rate.[12] We also note that a doubling of a worker's relative sales performance is not an unusual occurrence in our data given the wide dispersion in worker sales—it is equivalent to a worker moving from the 50th to the 67th percentile in terms of relative worker sales.

By contrast, workers with high collaboration experience do not appear, in most specifications, to be more likely to be promoted—despite, as we will later show, collaboration experience being predictive of manager value added. In Appendix Table A1, we present results using an OLS model of promotion instead of a probit model. The OLS model also accommodates firm-year-month fixed effects as additional control variables. We continue to find that sales positively predicts promotion, while collaboration experience insignificantly predicts or negatively predicts promotion probability.

The estimates presented in Columns 3 and 4 of Table 3 additionally control for industry or firm-level promotion rates in each year month. As expected, industry- and firm-level promotion rates are highly predictive of worker promotion probability. We will use this result in later analysis when we instrument for a worker's probability of promotion.

[12] A doubling of sales corresponds to a 0.699 log point increase in the independent variable. This leads to a 0.699*0.043=0.030 percentage point increase in the likelihood of promotion, relative to a base rate of 0.21 percent, making for a 0.030/0.21=14.3 percentage point increase.

3 What predicts managerial performance?

Next, we examine the relation between pre-promotion worker characteristics and post-promotion manager value added:

$$\begin{aligned}\text{Manager Value Added}_{if} &= b_1\text{Pre-Promotion Sales}_{if} + b_2\text{Pre-Promotion Collaborators}_{if} \\ &\quad + W_{if} + u_{if} \end{aligned} \tag{3}$$

We estimate Equation (3) at the manager level because manager value added is defined as a time-invariant manager characteristic. Pre-Promotion Sales$_{if}$ is the log of one plus manager i's monthly sales credits as a worker, averaged over the 12 months prior to i's promotion or over the total tenure if it spans fewer than 12 months. Analogous to the demeaned measure of worker sales in Equation (2), Pre-Promotion Sales$_{if}$ is also demeaned by the average sales performance of all workers in the sample in the same firm-year-month to account for variation in market conditions. Thus, Pre-Promotion Sales$_{if}$ represents each manager's pre-promotion sales performance relative to other workers in the firm during the same time period.

Similarly, Pre-Promotion Collaborators$_{if}$ is the log of one plus manager i's average number of collaborators per order in the year prior to promotion or over the total tenure if it spans fewer than 12 months. In some specifications, we also control for manager i's tenure in the month prior to the promotion event.

Table 4 shows that, among promoted managers, there is a significant *negative* relation between pre-promotion sales performance and subsequent managerial performance. Column 2 shows that doubling a manager's pre-promotion sales corresponds to a 7.5 percent decline in manager value added. Since manager value added represents the change in log subordinate sales, this implies that a manager with double the pre-promotion sales leads each subordinate's sales to decline by 7.5 percent. Given that a typical manager is in charge of five subordinates, our results also imply that a doubling of a manager's pre-promotion sales predicts that total team sales under the new manager will decline by more than one third of one worker. By contrast, collaboration experience is positively correlated with manager value added. In Column 2, we find that doubling collaboration experience predicts a 15.8 percent improvement in manager value added.

While it may seem counterintuitive that good sales workers make *worse* managers because both roles are likely to require social skills, the business press offers some insights into why excellence in sales may translate negatively into managerial quality. Sevy (2016), in an Forbes

article "Why Great Sales People Make Terrible Sales Managers," argues that great sales workers are motivated by a desire for personal—rather than team—achievement: "success in sales is about me while success in sales management is about my team. This is where the downside of a strong achievement drive makes itself known. If I'm driven to prove my personal ability, I find it hard (nearly impossible sometimes) to step back and let others take the spotlight." Consistent with the idea that sales performance may be negatively correlated with team commitment, we show in later analysis that lone-wolf sales workers (that is, those that have never collaborated in group sales) tend to have low manager value added as managers.

In some cases, workers are promoted to replace their former managers. In these types of promotions, one may be concerned that our estimates of the relation between manager value added and pre-promotion characteristics may be driven by spillover effects on the team that loses the high performing salesperson. When a high performing salesperson is promoted to manage her previous teammates, the salesperson's existing pipeline and territory could roll over to the salesperson's former peers, giving a temporary boost to the newly promoted manager's estimated value added. If scenarios like this cause the net spillover effect to be positively correlated with sales performance, then this would bias us against finding evidence of the Peter Principle: better salespeople would appear to become better managers. Alternatively, promoting high performing salespeople could disrupt relationships or accounts for which peers share credit or cause resentment. If promoting high performers has net negative spillovers, then we would be biased toward finding evidence of the Peter Principle.

To examine whether changes in performance are driven by spillovers, we further restrict our sample to managers who are promoted to largely different teams. Specifically, we require that more than two-thirds of the newly promoted manager's assigned subordinates were not previously a peer, where a peer is defined as someone also working under the same manager. In Columns 3 and 4 of Table 4, we find similar results in this restricted sample.

3.1 Correcting for selection

The empirical results so far show that firms promote based on current job performance even though pre-promotion sales negatively predict managerial performance and another observable characteristic, collaboration experience, positively predicts managerial performance. This evidence is consistent with the Peter Principle and the idea that firms promote based upon current job performance at the expense of promoting the best potential managers. However, these results are estimated from the selected sample of

workers who are actually promoted. Because promotion is not random, the relationship between sales and managerial performance in the promoted sample may not reflect the true relationship among all workers. For example, suppose that firms promote to maximize expected managerial match quality, which is a positive function of sales performance plus some other unobserved worker characteristic. In this example, strong sales performance can make up for deficits in the unobserved dimension, meaning that, among promoted workers, high sales workers will tend be weaker on unobservables than low sales workers. This type of selection would negatively bias our estimates of the true relation between pre-promotion sales and managerial quality, and may lead us to mistakenly conclude that sales performance negatively predicts managerial performance in the overall sample, even if the true relationship were positive and firms were indeed promoting the best potential managers. Selection bias can also lead to a *positive* bias in the estimated relation between sales and manager value added. Top sales people with weak unobservables may be more likely to turn down promotion offers because they expect to earn more in their current positions. If so, sales and unobserved managerial ability will be positively correlated in the selected sample of workers who are offered and accept promotions. We discuss this type of selection in more detail in Section 5.

To address this measurement challenge, we apply a two-step selection model in the style of Heckman (1976), Heckman (1979), Gronau (1974), and Lewis (1974). The goal of this selection correction is to recover the predictive relationship between sales performance and latent managerial potential for the full sample of workers, so that we can assess whether firms indeed promote high performing sales workers even though sales performance negatively predicts managerial performance.

Suppose that the underlying relationship between latent managerial potential M_i and worker characteristics is given by:

$$M_i = \beta_1 \text{Sales}_i + \beta_2 \text{Collaborators}_i + X_i\beta_3 + \varepsilon_i. \tag{4}$$

However, we only observe M_i if workers are promoted. The firm's promotion policy is given by:

$$P_i = \mathbb{I}\left(\tau_1 \text{Sales}_i + \tau_2 \text{Collaborators}_i + X_i\tau_3 + Z_i\tau_4 + \mu_i > 0\right) \tag{5}$$

This promotion equation is flexible and allows promotion to depend on observable worker characteristics that impact managerial potential ($Sales_i$, $Collaborators_i$, and X_i) as well as other factors Z_i that affect the probability of promotion but not managerial potential. We

do not impose any restrictions on the relation between Equations (4) and (5) other than the standard assumption in Heckman-style selection models that their respective error terms are jointly normally distributed with mean zero and correlation ρ, and that there exist variables Z_i that affect promotion but do not relate to managerial performance (discussed shortly).

Our empirical test of the Peter Principle centers on showing that both $\tau_1 > 0$ (firms are more likely to promote high-performing salespeople) and $\beta_1 < 0$ (higher-performing salespeople are more likely to be worse managers). If this were the case in the full sample of workers, it would have to be that better sales performers were receiving some kind of positive boost in promotion probability unrelated to their managerial potential. We have already shown $\tau_1 > 0$ in the full sample of workers, and we now seek to recover β_1 after correcting for sample selection.

Before continuing, we note that $\tau_1 > 0$ and $\beta_1 < 0$ is a sufficient but not necessary criterion for the Peter Principle. Firms may be promoting the best sales workers at the expense of managerial match quality even if the underlying relation between sales and latent managerial performance is positive. In such cases, firms may simply place too much weight on sales performance in promotion decisions and thereby overemphasize current job performance at the expense of promoting the best potential managers. In other words, the Peter Principle could still apply if $\tau_1 > 0$ and $\beta_1 > 0$, but τ_1 were too large relative to the weights on other characteristics in the promotion rule. In practice, we will show that sales performance continues to negatively predict managerial performance after correcting for selection, so any positive weight on sales in the promotion decision represents a promotion policy that does not maximize managerial match quality.

In our setting, latent managerial performance defined in Equation (4) is observed only when workers are promoted according to the rule defined in Equation (5). Following the standard Heckman correction procedure, we first estimate promotion propensity in the full worker-month level panel using a probit model:

$$\begin{aligned}\Pr(P_{ift} = 1 | \text{Sales}_{ift}, \text{Collaborators}_{ift}, X_{ift}, Z_{ift}) \\ = \Phi(a_1 \text{Sales}_{ift} + a_2 \text{Collaborators}_{ift} + X_{ift} a_3 + Z_{ift} a_4) \quad (6)\end{aligned}$$

This first stage is identical to the probit regression model that was already presented in Columns 3 and 4 of Table 3. We then recover estimates for the βs in Equation (4) by estimating a regression similar to Equation (3)—presented earlier—also controlling for the

inverse Mills ratio, λ_i, of the fitted values from the first stage regression:

$$M_i = b_1 \text{Sales}_i + b_2 \text{Collaborators}_i + X_i b_3 + b_4 \lambda_i + e_i. \quad (7)$$

This selection model allows us to recover unbiased estimates for the βs in Equation (4). Here, the inverse Mills ratio is a function of sales, collaborations, and other covariates X and Z. Crucially, we assume that there exist variables Z_i that impact a worker's probability of promotion, but not her managerial potential. If this assumption holds, then λ_i is separately identified from the other variables in the second stage regression because we assume that Z_i does not directly impact managerial performance and thus would not directly enter into this regression. This is equivalent to saying that the Z_is are instruments for promotion.

For Z_i, we use of industry-level or firm-level promotion rates. As shown in Columns 3 and 4 of Table 3, these average promotion rates are strongly positively correlated with a worker's probability of promotion. However, one may be concerned that differences in industry or company conditions captured by promotion rates may also reflect strong consumer demand and other firm-level factors, which may directly impact the performance of managers. Formally, industry-level or firm-level average promotion rates must be orthogonal to latent managerial potential in the full set of workers. While it is impossible to fully test this exclusion restriction because we do not observe latent managerial performance for the full sample of workers, we measure managerial quality as the value added of managers to subordinate sales, after controlling for worker and firm-year-month fixed effects. As such, our measure of manager quality among promoted workers is—by construction—orthogonal to our instruments for selection, which do not vary within year-month for a given firm.

Before continuing, we note that this approach allows us to recover the *predictive* relationship between worker characteristics and managerial potential in the full sample of workers and is not intended to establish a causal relation between these characteristics and managerial performance. For example, it remains possible that something correlated with sales performance, rather than sales performance per se, causes lower managerial performance. The same logic applies to collaboration experience. In addition, workers may not have the option to choose their level of collaboration if their products and collaborators are assigned by the firm. In this case, collaboration experience may not reflect a worker's underlying preferences for teamwork. Nevertheless, we argue that understanding the underlying predictive relation between observable worker characteristics and managerial

performance can aid firm promotion decisions even absent causality. Firms can still take these observable characteristics into account to promote the best potential managers.

Table 5 reports the relationship between pre-promotion characteristics and manager value added, corrected for selection. We continue to find that sales performance negatively predicts manager value added. Using industry-month level promotion rates as the selection instruments in Column 1, we estimate that a doubling (0.7 log point increase) in pre-promotion sales performance predicts a 7.4 percent decline in the sales performance of subordinates. Controlling for fixed effects in a manager's pre-promotion tenure in Column 2 does not make a difference. Using firm-month promotion rates in Columns 3 and 4 also yields nearly identical estimates. Overall, these estimates are similar in magnitude to those in Table 4, which did not correct for selection issues.

In Appendix Table A2, we restrict our sample to promoted managers that are assigned to subordinates that were not also their former team members. We find similar results in this sample, suggesting that our findings are not driven by unusual time trends occurring when workers are promoted to replace their former managers.

Figure 3 plots the relationship between sales performance, promotion probability, and manager value added. In the left panel, we see that sales is strongly associated with a higher probability of promotion, after controlling for firm-year-month fixed effects and collaboration experience. At the same time, however, this residual sales performance is negatively correlated with manager value added, even after applying a selection correction. Further, this figure allows us to look for potential non-monotonicities in the relationship between pre-promotion characteristics and promotion or manager value added. An inverted relation for certain parts of the sales distribution would suggest that promoting based on sales may actually help to maximize managerial match quality, at least within those parts of the sales distribution. For example, suppose that pre-promotion sales performance negatively predicts manager value added *on average*, but positively predicts manager value added for high values of sales. If so, firms may be maximizing managerial match quality when they promote based on sales performance, at least among the sample of workers with high sales. As can been seen in Figure 3, we do not find strong evidence of inversions for sales.

In Table 5, we also continue to find that collaboration experience positively predicts manager value added after applying a selection correction. The estimated magnitudes are economically large, but significant only when using industry-month rather than firm-month promotion-rates as the instruments for selection. Across all specifications, a doubling of

pre-promotion collaboration experience predicts a 12.3 to 16.9 percent increase in subordinate performance.

We explore the relationship between collaboration experience and manager value added in more detail in Table 6. We find that workers who never collaborate with others—so called "lone wolves"—fare particularly poorly when they are promoted into managerial roles. Anecdotally, lone wolves are known within the sales profession to be "the deeply self-confident, the rule-breaking cowboys of the sales force who do things their way or not at all" (Dixon and Adamson 2011). In our setting, whether a salesperson works alone or collaborates with others may either be a choice or be assigned. Regardless, we find that lone wolf status negatively predicts manager value added. A manager who was a lone wolf prior to promotion is associated with a 35 percent decrease in subordinate performance, relative to managers with collaboration experience. Despite this, lone wolves are not less likely to be promoted. Appendix Table A4 shows the predictive relation between lone wolf status and promotion; we find, if anything, a positive relation between the two. Similarly, Appendix Table A3 shows that this result is robust to considering workers promoted to manage different teams.

4 Could managers contribute in other ways?

Our results so far show that high-sales managers have lower value added from the perspective of increasing the average sales performance of their subordinates. It is possible, however, that managers contribute to firm value in other ways and that high pre-promotion sales managers may be better at these tasks in a way that justifies their promotion. For example, managers may play a role in reducing costly worker turnover or in recruiting new sales employees to expand the operations of the firm.

In Table 7, we show that, to the extent we are able to measure these behaviors in our data, high sales managers do not appear to be associated with better performance on these dimensions. To assess whether high-sales managers contribute by managing larger or more important divisions, we regress a manager's pre-promotion characteristics on the size of the team to which he or she is assigned. All team size and retention measures are demeaned by the average within a firm-year-month so that we can focus on differences across managers within the same firm-year-month. Column 1 shows that we find no statistically significant differences along this dimension. Next, Columns 2 and 3 show that high sales managers do not appear to be better at growing their team size or reducing turnover, respectively. Finally,

Column 4 shows that they are not better at retaining good sales workers while letting go of poor performers: subordinates departing from the teams of high sales managers appear to come from the same percentile of the performance distribution as those departing teams managed by those with poorer pre-promotion sales records. In addition to being statistically insignificant, these estimates are economically close to zero. For example, the coefficient of 0.049 in Column 1 signifies that a doubling in pre-promotion sales is associated with 0.034 more subordinates (relative to other teams in the same firm-year-month), an increase of less than one percent relative to the mean team size. In practice, our estimates are somewhat noisy and we interpret them as showing that there is no significant evidence that pre-promotion sales are associated with meaningful differences in performance along other dimensions.

5 Potential alternative explanations

The results above are consistent with the Peter Principle, which we define as promotion policies that favor higher performing workers at the expense of promoting the best potential managers. In this section, we explore whether alternative explanations or biases could explain our findings such that firms in our sample actually are promoting the best potential managers.

Lazear (2004) shows that mean reversion can generate patterns that, on the surface, look like the Peter Principle. The highest performing sales worker at any point in time may be a person who is currently selling above her individual mean; if she is promoted at this point, then her performance as a manager may fall relative to her pre-promotion performance due to mean reversion. The existence of mean reversion implies that, even if firms were promoting the best potential managers, we may see a decline in *within-person* performance after promotion. Our results cannot be explained by this type of mean reversion for two fundamental reasons. First, our empirical tests do not exploit within-person changes in performance after promotion. Instead, we show that sales negatively predicts potential managerial value added in the *cross-section* of workers. That is, regardless of whether individual workers experience mean reversion after promotion, our results suggest that firms can improve managerial performance by promoting workers with strong collaboration experience, rather than those with strong sales performance. Second, our measure of managerial performance is not based on a manager's own sales, but is instead based on the value added to the sales performance of his or her subordinates.

Thus, mean reversion in the manager's own sales performance would not affect our measure of manager value added.

Another potential concern is that newly promoted managers may be assigned to subordinates in a non-random manner. When a worker is promoted to manager, her new set of subordinates may be non-randomly drawn from the full sample of workers. In general, a simple correlation between the pre-promotion sales of newly-promoted managers and the *level* of performance of their assigned subordinates should not impact our results because we estimate manager value added from *changes* in subordinate performance under the new manager. However, we remain concerned that managers' pre-promotion performance may be correlated with time-varying aspects of a worker's sales performance. In particular, managers with high pre-promotion sales may be systematically assigned to subordinates whose sales are likely to decrease thereafter, for reasons unrelated to that manager's quality.

Table 8 explores the assignment of managers to subordinates. The first key pattern that emerges is that managers are not randomly assigned to subordinates: a doubling of a manager's pre-promotion sales is correlated with an approximately 20 percent increase in the prior sales of the subordinates to whom he or she is assigned. However, managers with higher pre-promotion sales do not appear to be assigned to subordinates with different *trends* in performance. Table 8, Columns 1, 2, and 3 examine subordinates' sales in the 7-9, 4-6, and 1-3 months prior to the arrival of the new managers. We find that the sales performance of subordinates in the 7-9 months prior to being assigned a new manager is just as predictive of that manager's pre-promotion sales performance as the subordinate's sales in the 1-3 months prior. The stability of these estimates over time suggests that managers with higher pre-promotion sales are not assigned to subordinates with increasing or decreasing trends in performance. We also find that managers with high pre-promotion collaboration experience tend to be assigned to subordinates with lower sales, although this relationship is not statistically significant, and there are no obvious time trends.

We also explore potential biases from non-random assignment by considering how subordinates' sales prior to a manager's promotion are correlated with the new manager's estimated value added. For example, high performing sales workers may have less scope for further improvement. If so, managers who are assigned to high prior sales subordinates may have low value added simply because these subordinates are already such high performers. In this case, manager value added should be negatively correlated with a subordinate's prior sales. Table 8, Columns 4 and 5 instead show a statistically

insignificant relationship between subordinate prior sales and manager value added. In Column 6, we find a borderline significant positive relationship suggesting that, if anything, high performing subordinates are associated with higher value added of future managers. Because managers with high pre-promotion sales are more likely to be assigned to these high performing subordinates, this positive relation would bias us away from finding evidence of the Peter Principle.

One may also be concerned about a different type of selection issue in which some top sales workers prefer not to be promoted. Although most workers enjoy significant pay increases after promotion, the very top sales workers in our sample earn more than the typical sales manager. It may be the case that some top sales workers do not want to be promoted and, as a consequence, we do not observe managers with very high pre-promotion sales in our sample of promoted workers. This type of selection is likely to be a bias against our findings that higher pre-promotion sales is associated with lower manager value added. Sales workers who are offered promotions should compare their expected pay as managers with their expected pay as sales workers, and then decide whether to accept the promotion. Thus, workers with strong sales should only accept promotions if they have very good prospects as managers. In other words, the selection in terms of who accepts promotion should bias toward finding that better sales workers make better managers, contrary to our finding that better sales workers become worse managers.

Even if it were the case that the very best sales workers actually made good managers but preferred to remain in their current roles, our results would still indicate that firms were not maximizing managerial performance by promoting good sales workers. If firms wish to maximize managerial match quality, they should promote the best potential managers from the set of workers who would be willing to accept such a promotion.

Finally, one may also be concerned that our results are influenced by the Great Recession, which occurs in our sample period. For example, high performing sales workers may be poor managers in recessions, but good managers otherwise, so that our results may lack external validity outside of downturns. Tables A5 and A6 provide evidence that this is not the case by showing that our results are robust to excluding the period of the Great Recession.

6 What are the performance losses from mismatch?

How much do the observed promotion policies cost firms in terms of lost subordinate sales? To analyze this, we set aside tournament incentives, worker motivation, monitoring

constraints, fairness concerns, and other potential benefits of firms' promotion rules to focus instead on the costs of managerial mismatch. Our estimates may be interpreted as the match quality that firms forgo to use promotions for other purposes.

To calculate the cost of mismatch, we examine how predicted managerial performance differs among three groups of workers: (1) actually promoted salespeople, (2) non-promoted salespeople among the promoted worker's peers, and (3) the top predicted manager among a promoted sales worker's peers. Peers are defined as other salespeople in a team managed by the same manager in the same time period. We interpret case (3) to be the performance-maximizing promotion decision under the restriction that mobility and other frictions prevent the firm from promoting among the entire organization, so that firms must promote among the peers of promoted workers. If we relax this restriction, then the estimated costs of mismatch will further increase. If firms face greater restrictions in promotions than we impose in our simulation, then we would overestimate the costs of mismatch.

We predict manager value added for each group using fitted values from the regression presented in Table 5, Column 2, modified so that pre-promotion sales are measured as an equally spaced three-part spline to allow for potential non-linearities.[13] Figure 4 shows the distributions of predicted manager value added in the three groups of workers. The mean predicted improvement in subordinate sales performance is scaled to zero for the sample of promoted workers.

The mean change in subordinate sales performance for the sample of non-promoted workers is 0.07, implying that firms' current promotion policies do slightly worse than promoting at random. This is expected, as firms' current promotion policies strongly favor sales performance, and sales performance negatively predicts manager value added (even when we allow for nonlinear relationships in the counterfactual simulation). Therefore, a random promotion policy that does not favor high sales workers outperforms the existing promotion policy in terms of promoting the best potential managers.

The mean in the sample of best predicted managers is 0.29, implying that subordinate performance could improve by up to 30 percent if firms pursued an alternative promotion policy of promoting the best predicted manager within a sales team.[14] Our estimate is not

[13]As discussed previously, the variance of manager value added may be biased upward because of measurement error in the estimation of the manager fixed effects. However, the measurement error associated with manager value added should not bias regression coefficients and estimates in this counterfactual, which uses fitted values from a regression based on Table 5, Column 2.

[14]We estimate a 30 percent gain in sales if firms switched to a promotion policy in which they promoted the best potential managers. This number would be even higher if we adjusted for the fact that promoting

meant to suggest that firms would actually achieve 30 percent gains in sales if they switched to a promotion policy in which they promoted the best potential managers. This counterfactual estimate ignores potentially large productivity declines that could result due to lost incentive and other morale effects that may occur if firms switched away from the current promotion policy of rewarding high sales with promotions. Thus, the 30 percent should be viewed as a lower bound for how large the incentive and other benefits of promoting the top sales workers would have to be to rationalize the current set of promotion policies.

7 Heterogeneity across firms

In this section, we consider whether firms vary in the extent to which they emphasize sales performance in promotion decisions. If firms are aware of the trade-off between maximizing managerial match quality and providing incentives for workers in their current roles, then we would expect firms to behave differently depending on the specific costs and benefits they face. For example, firms in which managers have greater responsibility may place more weight on picking the best managers and may therefore be more willing to promote workers who are weaker in terms of sales performance. Similarly, firms that have alternative ways of incentivizing workers to exert effort in their current job roles may prioritize managerial match quality more in their promotion decisions.

Table 9 considers the first possibility by examining how promotion policies differ across firms by the supervisory responsibilities assigned to managers. We use the number of subordinates associated with each manager as a proxy for managerial responsibility and then take averages to obtain a firm-level measure. We then augment Equation (2), which examines a worker's likelihood of promotion, by interacting our measures of worker's sales and collaboration experience with the log of average team sizes each firm-year. Our estimation also controls for the direct effects of all variables.

Our results show that firms with larger subordinate teams tend to place less weight on sales performance in promotion decisions. A doubling of average team size reduces the predictive relationship between sales performance and promotion by over 25 percent. By contrast, firms with larger team sizes place relatively more weight on collaboration experience. These findings suggest that, when the costs of managerial mismatch are

lower sales workers results in less foregone sales, given that managers are no longer engaged in direct front line sales.

particularly high, firms are more willing to sacrifice possible incentive benefits of performance-based promotion tournaments in order to promote better managers.

Next, we consider how promotion policies vary with pay-for-performance. Baker, Jensen and Murphy (1988) suggest that pay-for-performance incentivizes worker effort and may offer an alternative to the tournament incentives associated with promotion policies that heavily weight current job performance. To explore this possibility, we construct a firm-level measure of pay-for-performance as the ratio of commissions and bonuses to base salary, averaged across all workers in the firm within each calendar year. Before proceeding, we note that, although we observe commission and bonus compensation for most workers in our sample, we only observe base salaries for a subsample of firms, leading to an approximate 35 percent decline in sample size for this analysis. Further, as discussed previously, our data provider's software is not designed to track or distribute base salaries, which is an optional field. Therefore, salary data can be missing or measured with error (for example, if they are not updated or the pay periods are unclear). Nevertheless, we are able to construct a reasonable, if noisy, proxy for the strength of pay-for-performance incentives across firms in our data.

As with our team size analysis, we regress whether a worker is promoted on the interaction between our measure of pay-for-performance and worker sales and collaboration experience. We find that firms with relatively stronger pay-for-performance tend to implement promotion policies that are less sensitive to worker sales performance and more sensitive to collaboration experience. This is consistent with the idea that pay-for-performance incentives can partially offset the need to provide incentives through promotion tournaments based on current job performance. However, our results do not necessarily imply that pay-for-performance can fully substitute for promotion based-incentives. Pay-for-performance may be an expensive substitute for promotion-based incentives, especially if workers also value the security, stature (e.g. DellaVigna and Pope 2016; Larkin 2011), or external signaling abilities associated with promotions (DeVaro and Waldman 2012). The sales positions we study already have strong pay-for-performance relative to many other occupations. The fact that we still observe evidence of the Peter Principle in the sales setting suggests that it may be difficult to fully substitute for promotion-based incentives.

8 Conclusion

We use detailed microdata on the performance and promotions of sales workers at a large number of firms to test the adage that "the best salesperson doesn't always make the best manager." Consistent with the Peter Principle, we find that promotion decisions place more weight on current performance than would be justified if firms only tried to promote the best potential managers. The most productive worker is not always the best candidate for manager, and yet firms are significantly more likely to promote top frontline sales workers into managerial positions. As a result, the performance of a new manager's subordinates declines relatively more after the managerial position is filled by someone who was a strong salesperson prior to promotion. This is particularly true if the new manager had little collaboration experience during her tenure as a sales worker.

We caution against interpreting these results as evidence that firms have mistaken beliefs or behave inefficiently. Rather, consistent with tournament theory, firms may heavily weight current job performance in promotion decisions to encourage workers to exert effort in their current job roles and to maintain norms of fairness. In addition, the availability of relatively clear measures of worker productivity among frontline sales workers may lead organizations to emphasize these characteristics rather than other, more subjective or fungible employee characteristics in promotion decisions.

Regardless, our analysis shows that firms do not promote to maximize managerial match quality. Instead, firms appear to actively manage the trade-off between providing incentives and promoting the best potential managers: firms place less emphasis on current job performance in promotions where managerial roles entail greater responsibility and where current performance is rewarded by relatively strong pay-for-performance. Overall, our results imply that managerial match quality, tournament incentives, and other objectives of job promotions are not perfectly aligned. The trade-off between incentives and match quality is likely to be an important consideration for any firm or institution in which the skills required to succeed at one level in the organizational hierarchy differ from the skills necessary to succeed at a higher level.

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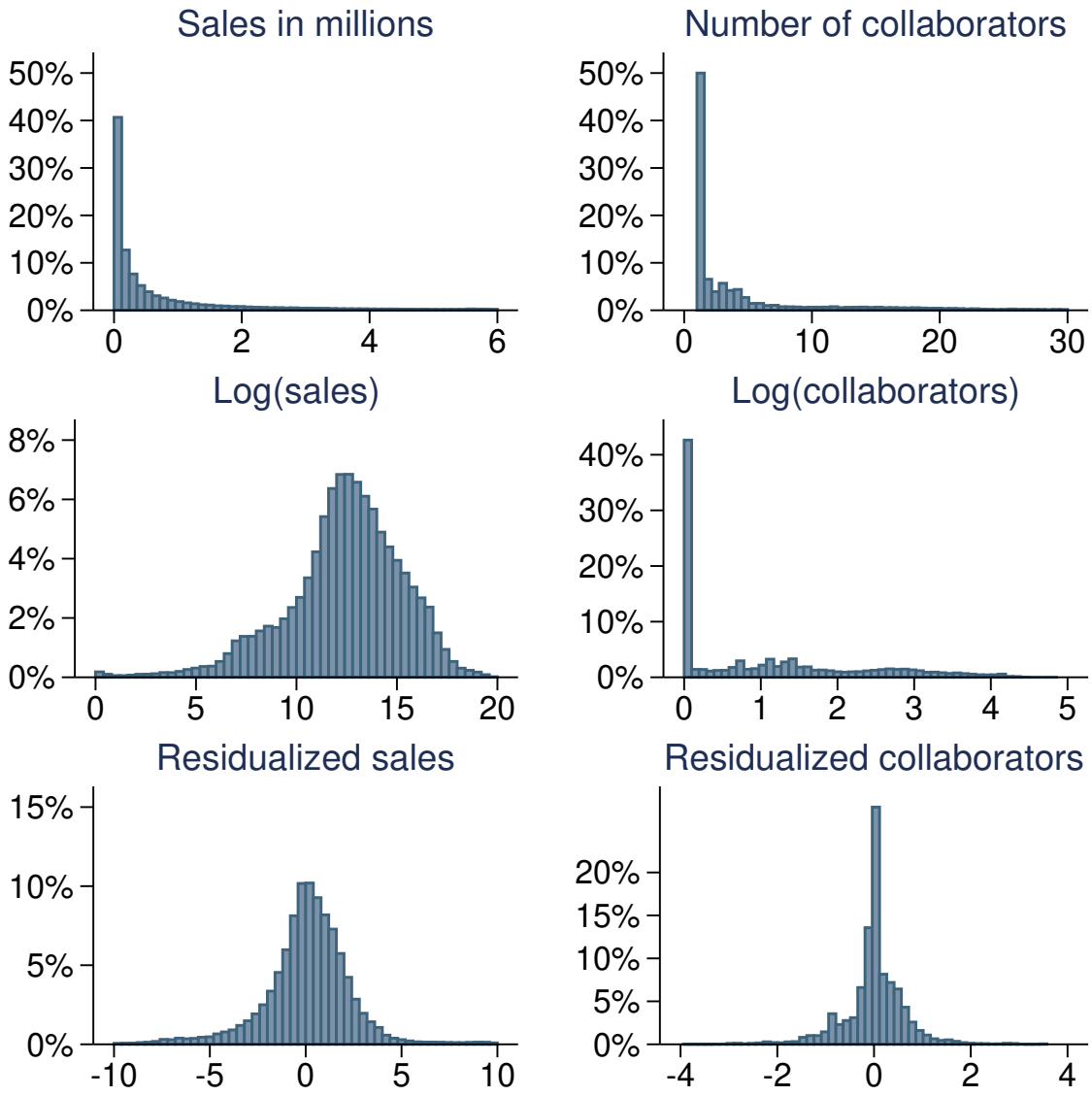
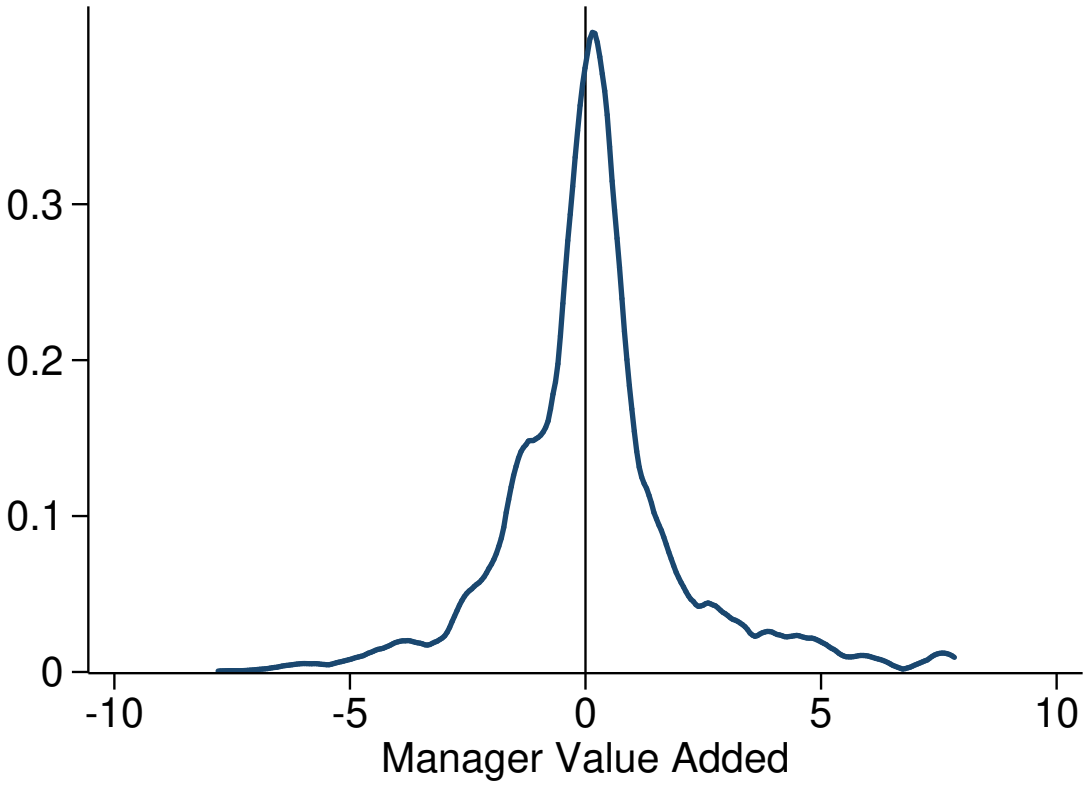
Figure 1: Distribution of Sales and Number of Collaborators



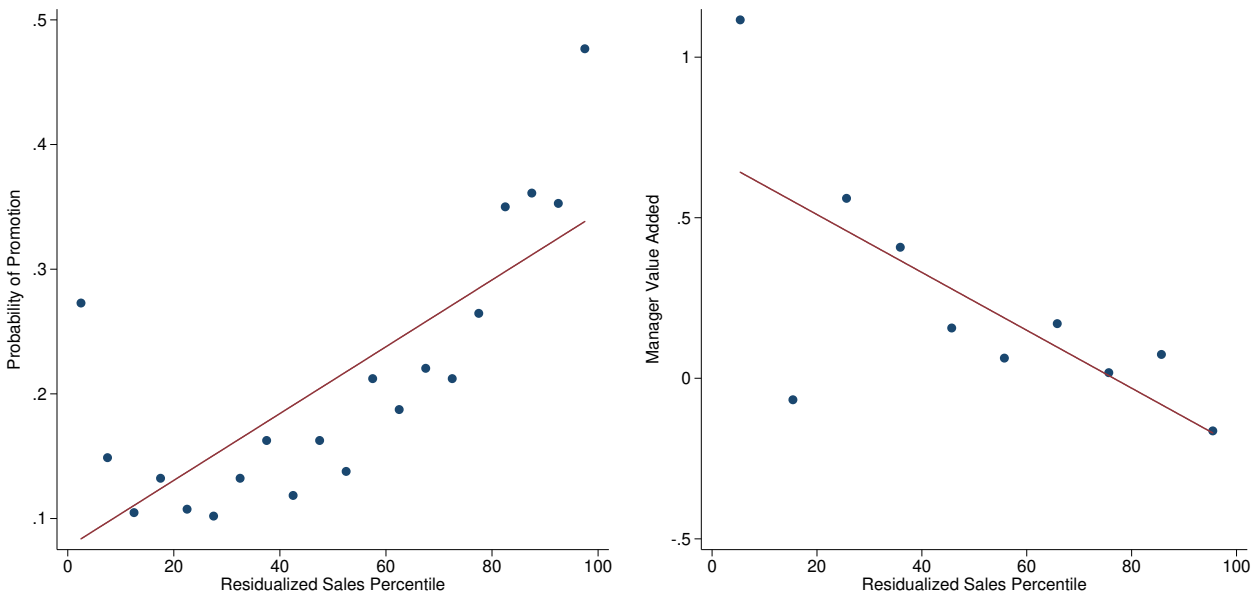
Notes: Left and right panels are 12-month moving averages for sales and number of collaborators, respectively. The top panels show the untransformed distribution. The middle panels show the log-transformed distribution. The bottom panels show the residuals after the log-transformed variables are regressed on firm-year-month fixed effects.

Figure 2: Distribution of Manager Value Added



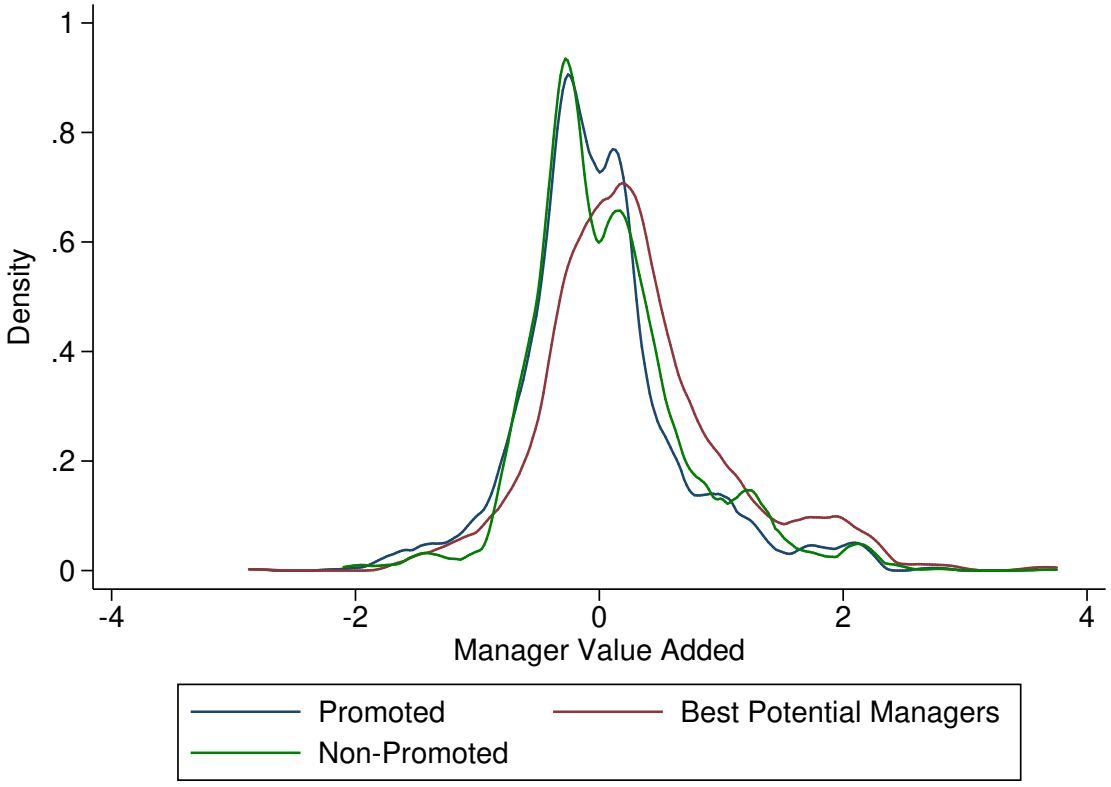
Notes: The figure shows the kernel density of estimated manager value added, which is computed as described in Section 1.2.

Figure 3: Binned Scatterplot of Promotion and Manager Value Added vs Pre-promotion Sales



Notes: The left panel shows a binned scatterplot relating worker sales and probability of promotion. Residualized sales percentile is the percentile-ranked residual from a regression of the 12-month moving average of log pre-promotion sales (measured relative to the firm-year-month mean) on log(collaborators). The right panel plots the relation between pre-promotion sales performance and manager value added. These data are at the manager-level and include only promoted managers. Residual sales percentile is the percentile-ranked residual from a regression of the manager's pre-promotion sales in the 12 months before promotion (measured relative to the firm-year-month mean) on pre-promotion log(collaborators) and the likelihood of selection into the promoted sample.

Figure 4: Actual vs. Optimal Promotion Policies



Notes: The figure shows the distribution of predicted manager value added for the samples of workers that are promoted, workers that are not promoted, and workers that would make the best potential managers. All manager value added measures are predicted fitted values of manager value added based on the regression in Table 5, Column 2, modified so that pre-promotion sales is measured as an equally spaced three-part spline to allow for potential non-linearities. To determine the best potential managers sample, we select the worker with the highest predicted manager value added within the same team and month when a worker is actually promoted. The non-promoted sample consists of other, non-promoted, workers in the same team and month when a worker is actually promoted.

TABLE 1: DESCRIPTIVE STATISTICS

Sample coverage		**Pr(Promotion)**	
# Firms	214	Overall	0.0288
# Workers	53,035	Monthly hazard	0.0021
# Workers promoted to management	1,531		
Years covered	2005-2011		

Summary Statistics	*Mean*	*25th*	*50th*	*75th*
Worker characteristics				
Monthly sales*	$3,620,399	$49,956	$285,573	$1,540,871
# Collaborators*	6.5	1	1.8	5.6
Monthly commissions*	$12,485	$1,101	$3,658	$ 9,716
Salary	$7,450	$4,938	$7,443	$9,481
Manager Characteristics				
# of subordinates	5.4	2	4	8
Monthly commissions*	$22,193	$3,112	$10,180	$25,165
Change in monthly commissions	$7,173	-$839	$2,006	$10,156
Salary	$10,563	$8,317	$10,690	$13,283

NOTES: * denotes 12 month moving average. The change in monthly commissions represents changes in pay after promotion, estimated as average monthly commissions in the 12 months after promotion minus average monthly commissions in the 12 months before promotion.

Table 2: Managers Samples

Managers sample size	
Number of managers	6,515
... with mover subordinates and estimated fixed effects	4,887
... with mover subordinates, estimated fixed effects, who are internally promoted within our sample	1,028
Share of workers who switch managers	0.273
Average size of connected group (worker-months)	13,558
Share of workers in largest connected group	0.765

Notes: Managers with mover subordinates are managers with at least one subordinate who has worked under other managers within our data sample. Internally promoted managers are managers we observe as workers prior to promotion.

TABLE 3: PROBIT MODEL FOR PROMOTIONS

	Worker is promoted			
	(1)	(2)	(3)	(4)
Log(sales)	0.0400***	0.0448***	0.0433***	0.0428***
	(0.00578)	(0.00645)	(0.00642)	(0.00610)
Log(collaborators)	0.00895	0.0235	0.0461**	-0.0103
	(0.0212)	(0.0209)	(0.0205)	(0.0207)
Industry-month promotion rate			0.315***	
			(0.0183)	
Firm-month promotion rate				0.167***
				(0.00653)
Pre-promotion controls	No	Yes	Yes	Yes
Pseudo R-squared	0.004	0.018	0.042	0.112
Observations	207,092	207,092	207,092	207,092

NOTES: This table presents the probit regression described in Equation (2). We use data at the worker-month level for workers that have not yet been promoted, for firm-months in which at least one worker at the firm is promoted. The dependent variable is an indicator for whether a worker is promoted in the next month. Log sales is the log of one plus worker i's monthly sales credits, averaged over the past 12 months or for the worker's total tenure if tenure is fewer than 12 months, and demeaned within company-year-month. Log(collaborators) is the log of one plus the average number of other collaborators worker i has per order, again averaged over the past 12 months or for the worker's total tenure if tenure is fewer than 12 months. Pre-promotion characteristics include controls for seven bins of a worker's tenure, interacted with an indicator for whether tenure may be censored. Industry-month promotion rate is the percentage of workers promoted within an industry-year-month. Firm-month promotion rate is the percentage of workers promoted within worker i's firm in the same month. Reported coefficients represent marginal effects and are multiplied by 100 to represent percentage point change in the probability of being promoted. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Predictors of Manager Value Added

	Manager value added			
	All promotions		Promoted to different team	
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.106***	-0.108***	-0.123***	-0.128***
	(0.0401)	(0.0395)	(0.0418)	(0.0409)
Pre-promotion log(collaborators)	0.244*	0.226*	0.219	0.193
	(0.132)	(0.131)	(0.140)	(0.141)
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.04	0.066	0.066	0.077
Observations	1,028	1,028	909	909

Notes: This table presents the regression described in Equation (3). We use data at the manager level. The sample is restricted to promoted managers for whom we can observe pre-promotion characteristics and for whom we can estimate manager value added fixed effects using movements of subordinates across managers. The dependent variable is manager value added, estimated as the change in subordinate performance associated with each manager (see Equation (1)). Log sales is the log of one plus manager i's monthly sales credits as a worker, averaged over the 12 months prior to i's promotion (or for i's total pre-promotion tenure, if fewer than 12 months), and demeaned within company-year-month. Log(collaborators) is the log of one plus manager i's number of distinct other collaborators per order, averaged over the 12 months prior to promotion (or for i's total pre-promotion tenure, if fewer than 12 months). Even-numbered columns include controls for the manager's tenure in the month prior to promotion, as described in Table 3. Columns 3 and 4 further restrict the sample to managers who are assigned to subordinates, at least two-thirds of whom were not their previous teammates. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Heckman Selection Model for Predictors of Manager Value Added

	Manager value added			
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.106***	-0.110***	-0.109***	-0.114***
	(0.0400)	(0.0394)	(0.0407)	(0.0394)
Pre-promotion log(collaborators)	0.242*	0.228*	0.233*	0.176
	(0.133)	(0.131)	(0.130)	(0.130)
Instrument	Industry × month	Industry × month	Firm × month	Firm × month
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.04	0.066	0.042	0.073
Observations	1,028	1,028	1,028	1,028

Notes: This table re-estimates Table 4 using the Heckman selection model, using industry-month or firm-month promotion rates as the instruments for selection into the sample of promoted managers. All other variables are defined as in Table 4. In Columns 1 and 2, the promotion instrument is the industry-month promotion rate; for Columns 3 and 4, it is the firm-month promotion rate. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Heckman Selection Model for Predictors of Manager Value Added, Lone Wolf

	Manager value added			
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.110***	-0.114***	-0.113***	-0.116***
	(0.0415)	(0.0403)	(0.0422)	(0.0402)
Pre-promotion lone wolf	-0.379**	-0.357**	-0.353**	-0.218
	(0.176)	(0.172)	(0.167)	(0.166)
Instrument	Industry × month	Industry × month	Firm × month	Firm × month
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.039	0.065	0.04	0.071
Observations	1,028	1,028	1,028	1,028

Notes: This table re-estimates Table 5 but with collaboration experience defined as an indicator variable for whether a manager collaborated with at least one other person on a sale in the year prior to promotion (or pre-promotion tenure if less than one year). All other variables are as defined for Table 4. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

TABLE 7: RETENTION

	Number of subordinates	Fraction joining	Fraction leaving	Percentile of leavers
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	0.0494	0.000639	0.000241	0.000573
	(0.0585)	(0.000423)	(0.000405)	(0.00265)
Pre-promotion log(collaborators)	0.00587	-0.000784	0.00167	0.0160
	(0.401)	(0.00228)	(0.00180)	(0.0142)
Pre-promotion controls	Yes	Yes	Yes	Yes
R-squared	0.165	0.013	0.009	0.05
Observations	14,397	14,397	14,397	1,627

NOTES: This table tests whether manager pre-promotion characteristics predict the employee retention after the manager's promotion. The data are at the promoted-manager-month level. Pre-promotion sales and collaborators are as defined in Table 4. Number of subordinates is the size of the manager's team; fraction joining is the fraction of new team members joining each month; fraction leaving is the fraction of team members exiting each month; and percentile of leavers is the sales percentile of departing workers, measured relative to all workers in each firm-month. All these variables are measured relative to the firm-year-month mean in the full sample. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are clustered by manager. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Assignment of Managers to Subordinates

	Log(subordinate sales)					
Months before promotion window:	[-9, -7]	[-6, -4]	[-3, -1]	[-9, -7]	[-6, -4]	[-3, -1]
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-promotion log(sales)	0.292***	0.316***	0.289***			
	(0.0702)	(0.0696)	(0.0716)			
Pre-promotion log(collaborators)	-0.173	-0.339	-0.157			
	(0.282)	(0.249)	(0.253)			
Manager value added				0.00986	-0.00689	0.0462
				(0.0809)	(0.113)	(0.115)
R-squared	0.149	0.178	0.155	< 0.001	< 0.001	0.001
Observations	569	569	569	569	569	569

Notes: This table explores how new managers are assigned to subordinates. The sample is at the manager-level, and includes all promoted managers for whom there exist data on manager value added, and are assigned to subordinates with observable performance in the nine months before the promotion window of the manager, with the window defined as [-1,+1] months around the promotion date. The dependent variable is the team-wide average of subordinate monthly log sales in the 7-9 month, 4-6 month, and 1-3 month period prior to the promotion window. All other variables are as defined in Table 4. Observations in Columns 4-6 are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Heterogeneity in Promotion Policies by Team Size

	Worker is promoted			
	(1)	(2)	(3)	(4)
Log(sales)	0.524***	0.507***	0.425***	0.666***
	(0.0677)	(0.0676)	(0.0644)	(0.0946)
Log(sales) x log(avg teamsize)	-0.206***	-0.198***	-0.164***	-0.252***
	(0.0308)	(0.0307)	(0.0291)	(0.0420)
Log(collaborators)	-0.387**	-0.251*	-0.348**	-0.267
	(0.152)	(0.145)	(0.146)	(0.350)
Log(collaborators) x log(avg teamsize)	0.178**	0.128*	0.161**	0.0528
	(0.0757)	(0.0719)	(0.0702)	(0.163)
Log(avg teamsize)	-0.897***	-0.796***	-0.412***	
	(0.120)	(0.115)	(0.119)	
Industry-month promotion rate		0.363***		
		(0.0185)		
Firm-month promotion rate			0.182***	
			(0.00595)	
Firm-month FE	No	No	No	Yes
Pseudo R-squared	0.033	0.054	0.12	0.043
Observations	186,532	186,532	186,532	186,532

Notes: This table explores how promotion policies vary with the average size of teams across firms. Log average team size is the log of the average number of subordinates assigned to each manager within each firm-year. Observations are at the worker-month level. All other variables and sample restrictions are as described in Table 3. Columns 1-3 are estimated using a probit model, while Column 4 is estimated using OLS to accommodate firm-year-month fixed effects. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Heterogeneity in Promotion Policies by Pay-for-Performance

	Worker is promoted			
	(1)	(2)	(3)	(4)
Log(sales)	0.200***	0.182***	0.181***	0.342***
	(0.0470)	(0.0471)	(0.0429)	(0.0782)
Log(sales) x ratio variable pay	-0.373***	-0.329***	-0.316***	-0.597***
	(0.110)	(0.110)	(0.103)	(0.180)
Log(collaborators)	-0.387***	-0.238**	-0.0902	-0.733***
	(0.111)	(0.107)	(0.109)	(0.255)
Log(collaborators) x ratio variable pay	0.910***	0.575**	0.135	1.33**
	(0.276)	(0.263)	(0.282)	(0.639)
Ratio variable pay	-1.42***	-0.994**	-0.677*	
	(0.403)	(0.399)	(0.405)	
Industry-month promotion rate		0.543***		
		(0.0364)		
Firm-month promotion rate			0.175***	
			(0.00783)	
Firm-month FE	No	No	No	Yes
Pseudo R-squared	0.018	0.037	0.092	0.031
Observations	136,083	136,083	136,083	136,083

Notes: This table explores how promotion policies vary with the strength of pay-for-performance across firms. Ratio variable pay represents the ratio of commissions plus bonus to salary, averaged across all workers within each firm-year. Observations are at the worker-month level and exclude firms for which we do not have compensation data. All other variables and sample restrictions are as described in Table 3. Columns 1-3 are estimated using a probit model, while Column 4 is estimated using OLS to accommodate firm-year-month fixed effects. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix for Online Publication

Appendix Table A1: Linear Probability Model for Promotions

	Worker is promoted				
	(1)	(2)	(3)	(4)	(5)
Log(sales)	0.0371***	0.0377***	0.0338***	0.0417***	0.0465***
	(0.00500)	(0.00505)	(0.00502)	(0.00496)	(0.00547)
Log(collaborators)	0.00518	0.0206	0.0790***	-0.0530**	-0.157***
	(0.0219)	(0.0223)	(0.0225)	(0.0217)	(0.0412)
Industry-month promotion rate			1.640***		
			(0.140)		
Firm-month promotion rate				0.986***	
				(0.0455)	
Pre-promotion controls	No	Yes	Yes	Yes	Yes
Firm-month FE	No	No	No	No	Yes
R-squared	< 0.001	0.002	0.011	0.043	0.044
Observations	207,092	207,092	207,092	207,092	207,092

Notes: This table presents the regression described in Equation (2) using OLS instead of probit. All variables are as describe in Table 3. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A2: Heckman Selection Model for Predictors of Manager Value Added, Different Teams

	Manager value added			
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.122***	-0.129***	-0.126***	-0.134***
	(0.0416)	(0.0407)	(0.0420)	(0.0407)
Pre-promotion log(collaborators)	0.217	0.194	0.205	0.135
	(0.141)	(0.141)	(0.138)	(0.141)
Instrument	Industry × month	Industry × month	Firm × month	Firm × month
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.066	0.077	0.073	0.086
Observations	909	909	909	909

Notes: This table re-estimates Table 5 using only the sample of managers who are assigned to subordinates, at least two-thirds of whom were not their previous teammates. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A3: Heckman Selection Model for Predictors of Manager Value Added, Lone Wolf, Different Teams

	Manager value added			
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.127***	-0.132***	-0.130***	-0.135***
	(0.0431)	(0.0416)	(0.0436)	(0.0414)
Pre-promotion lone wolf	-0.345*	-0.316*	-0.311*	-0.153
	(0.186)	(0.183)	(0.176)	(0.181)
Instrument	Industry × month	Industry × month	Firm × month	Firm × month
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.065	0.076	0.071	0.085
Observations	909	909	909	909

Notes: This table re-estimates Table 6 using only the sample of managers who are assigned to subordinates, at least two-thirds of whom were not their previous teammates. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A4: Probit Model for Promotions, Lone Wolf

	Worker is promoted			
	(1)	(2)	(3)	(4)
Log(sales)	0.0428***	0.0463***	0.0429***	0.0465***
	(0.00663)	(0.00623)	(0.00664)	(0.00625)
Lonewolf	-0.0681	0.118***	-0.0195	0.184***
	(0.0417)	(0.0385)	(0.0519)	(0.0508)
Log(collaborators)			0.0396	0.0534**
			(0.0255)	(0.0260)
Industry-month promotion rate	0.315***		0.316***	
	(0.0184)		(0.0184)	
Firm-month promotion rate		0.168***		0.169***
		(0.00656)		(0.00657)
Pre-promotion controls	Yes	Yes	Yes	Yes
Pseudo R-squared	0.042	0.113	0.042	0.113
Observations	207,092	207,092	207,092	207,092

Notes: This table re-estimates Table 3, but with collaboration experience defined as an indicator variable for whether a manager collaborated with at least one other person on a sale in the year prior to promotion (or pre-promotion tenure if less than one year). All other variables are as defined for Table 3. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A5: Probit Model for Promotions, Excluding the Recession

	Worker is promoted			
	(1)	(2)	(3)	(4)
Log(sales)	0.0208***	0.0233***	0.0229***	0.0201***
	(0.00298)	(0.00305)	(0.00302)	(0.00262)
Log(collaborators)	0.00577	0.0132*	0.0158**	-0.00262
	(0.00673)	(0.00685)	(0.00677)	(0.00674)
Industry-month promotion rate			0.0779***	
			(0.00414)	
Firm-month promotion rate				0.0681***
				(0.00320)
Pre-promotion controls	No	Yes	Yes	Yes
Pseudo R-squared	0.006	0.017	0.034	0.141
Observations	523,132	523,132	523,132	523,132

Notes: This table re-estimates Table 3, but excluding year-months corresponding to the Great Recession (NBER dates: December 2007 to June 2009). All variables are as defined for Table 3. Standard errors are clustered by worker. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A6: Heckman Selection Model for Predictors of Manager Value Added, Excluding the Recession

	Manager value added			
	(1)	(2)	(3)	(4)
Pre-promotion log(sales)	-0.127***	-0.132***	-0.130***	-0.135***
	(0.0431)	(0.0416)	(0.0436)	(0.0414)
Pre-promotion lone wolf	-0.345*	-0.316*	-0.311*	-0.153
	(0.186)	(0.183)	(0.176)	(0.181)
Instrument	Industry × month	Industry × month	Firm × month	Firm × month
Pre-promotion controls	No	Yes	No	Yes
R-squared	0.065	0.076	0.071	0.085
Observations	909	909	909	909

Notes: This table re-estimates Table 5, but excluding managers who were promoted in year-months corresponding to the Great Recession (NBER dates: December 2007 to June 2009). All variables are as defined for Table 5. Observations are weighted by the inverse variance of the manager value added measures. Standard errors are adjusted for heteroskedasticity. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.