How Does Improvement in Commuting Affect Employees? Evidence from a Natural Experiment

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August 22, 2019

Abstract

We collect rich worker month-level administrative panel data from two companies for a two-year period prior to and after the opening of a nearby subway station, which significantly improved public transportation commutes for a subset of workers. Consistent with a simple principal-agent model where improvement in commute reduces the cost of effort for workers, we find a significant difference-in-differences (DID) increase (12.6% of the standard deviation) in bonus pay (which is strongly correlated to worker-level performance measures) for affected workers relative to coworkers not impacted by the subway. The bonus increase is larger for workers with more variance in performance measures (marketing personnel and non-managers), is positively correlated with commute time saved, and lower for workers with access to technology that facilitated telecommuting. We do not find that improved performance is simply a result of affected workers spending extra time at the workplace. Additionally, we find a significant relative decline in monthly exit hazard (by about 50%) for affected workers, and evidence for higher quality (conditional on wages) of new hires from affected areas. Supplementary event study analysis using a large sample of subway construction commencements suggests that shareholder value is also positively impacted by access to a nearby subway.

Keywords: labor productivity, transportation, principal-agent, worker turnover, incentives

JEL *Codes*: H54, J63, J24, J22

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1. Introduction

Across the world, hundreds of millions of workers incur significant time and expense on their daily commutes to work. Daily round-trip commutes constitute a substantial portion of worktime, ranging from 10% to 20%, and commute time has been increasing in many parts of the world.[1] Numerous studies document that commuting is a highly disliked activity, and tougher (i.e., longer, more crowded, and/or more unpredictable) commutes are associated with lower subjective well-being.[2] Given the high costs of commuting, governments continue to make huge investments in transportation infrastructure, particularly for urban transit (Redding and Turner, 2015; Lu, Han, Lu and Wang, 2016; Freemark, 2018; Koyanagi, 2017).

Because infrastructure for commute improvement is costly, how a change in commute quality impacts economic outcomes is an important question; however, significant empirical challenges make it difficult to address. In particular, the location of transportation infrastructure investment is not random, and is likely to be influenced by local economic conditions, making it hard to identify causal effects (Redding and Turner, 2015). While a number of important studies have used alternative instrumental variables approaches to study the impact of transportation infrastructure on aggregate or city-level outcomes, studies examining effects on worker-level outcomes are rare, limiting our understanding of how workers respond to costly investments to improve transit.[3]

We contribute to the literature by obtaining and exploiting rich worker month-level "insider" panel data, in a natural experiment setting that allows us to examine how workers and

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1 In the US, over 125 million workers commute (Tomer, 2017), and average one-way travel time was about 26.4 minutes in 2017. Commute time in Korea and Japan is documented to be double that for the US (OECD, 2016), and a 2015 Baidu study using mobile phone data (see e.g., Chen, 2015) estimated the average one-way commute time in China is about 28 minutes, with much higher figures for the big cities such as Beijing (52 minutes). US Census data suggest that average commute time has gone up for the US (Harrison, 2017), and studies show a rise in commute time in China (e.g., Sun et al., 2017).

2 An influential study by Kahneman et al. (2004) found commuting among the least pleasant of daily activities undertaken by respondents. A number of studies across different countries have found longer commutes are associated with lower measures of subjective well-being (e.g., Stutzer and Frey (2008) for Germany; Choi, Coughlin, and D'Ambrosio (2013) for US; Chatterjee et al. (2017) for UK; Zhu et al. (2017) for China). Studies also find that driving is relatively more stressful than other modes, with effort and predictability playing a mediating role (e.g., Wener and Evans, 2011; Legrain, Eluru and El-Geneidy, 2015).

3 Baum-Snow (2007) pioneered the use of historical planned route to construct an instrumental variable; Duranton and Turner (2011) developed a historical route instrumental variable, and Donaldson (2018) exploits similar variation. Redding and Turner (2015) provides a review of other related papers. Bloom et al. (2015) is an influential study of telecommuting that examines worker-level outcomes; however, telecommuting allowing employees to work at home also changes other aspects of the employment relationship, such as monitoring, in addition to commute time.

firms responded to an improvement in public transit.

Specifically, we examine the effects of the opening of a terminal station of Subway Line 15 under a plaza at the center of a very busy business district in Beijing, China in December, 2014. We obtained administrative data for a four-year period around the opening of the subway (from January, 2013 to December, 2016), including monthly data on wage components, performance records, attendance records, daily building entry/exit data, and worker retention, for all current and past employees in two companies located in the plaza. We supplemented the data with additional information collected through surveys and interviews of current employees.

Subway Line 15 significantly improves commute quality for affected employees in several ways. First, the availability and proximity of the new subway line reduced public transport round-trip commute time substantially, by an average of 43.5 minutes (or 9.06% of an 8 hour workday) for workers able to use the subway. Second, studies have found subways are a preferred commuting mode; one key factor highlighted in the literature (and specifically cited by workers we study) is that subway commute is much more predictable relative to other modes, which have uncertain congestion delays.[4] Finally, because the new station is a terminus, the commute on this subway is significantly less crowded (especially for return commutes), reducing an important commute-related stress factor[5] and allowing people to work during the commute[6].

Our data and setting allow us to adopt a difference-in-differences (DID) estimation strategy. Specifically, we compare the before-after change of outcomes within employees who can take advantage of the subway line (i.e., those for whom the fastest public transport route to work involves Subway Line 15, hereafter called "affected" workers) to the within-employee changes for those who do not benefit directly from Subway Line 15 (hereafter called "unaffected" or "control" workers).

We present a simple principal-agent model to guide our empirical work. We posit that a better commute reduces the cost of effort for the agent (either by improving wellbeing and,

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4 See e.g., Meyer and Dauby (2002), Wener et al. (2003), Wener and Evans (2011).
5 See Singer, Lundberg, and Frankenhauser (1978).
6 See Jain, Clayton, and Bartle (2017).

hence, productivity as in the studies reviewed by Porto (2016), or by freeing up commute time for work), as well as the agent's non-pecuniary utility (by improving subjective wellbeing as documented in the literature). Then it follows that a better commute leads to greater worker effort and hence greater bonus pay, as well as a lower probability of exit and a higher relative quality of hires from affected areas (conditional on wage). The model also generates predictions about potential heterogeneity of effects, with a lower predicted impact on bonus pay if the bonus is a smaller share of total pay, or if performance measures show lower variance.

Indeed, we find that the bonus pay of affected employees shows a significant DID increase of about 12.56% of the standard deviation (about 11.74% of the mean bonus level), following the opening of the new subway line. Our analysis of the relationship between bonus and worker-level performance scores, as well as our discussions with firm management, confirm that bonus pay is strongly linked to worker-level performance. Accordingly, we interpret the baseline results as implying that the improvement in commute led to worker performance improvements, as predicted by our model.[7]

We conduct three key tests to address potential threats to our DID identification strategy. First, we undertake an event study analysis; it confirms that there are no differences in pre-existing time trends for affected employees relative to unaffected employees and reveals a significant relative increase in bonus pay for affected workers coincident with the opening of the new subway station. Next, we use propensity score matching to construct a control group where we get better balance on observables between the treatment and control group; we find that the effect on bonus is very similar (in fact, slightly stronger) in this analysis. Finally, we conduct a permutation test (involving random assignment of the "affected by new subway" treatment across workers), which confirms the significance of our baseline results.

We undertake a number of additional robustness checks. We verify that the bonus

[7] We are careful not to interpret our results as reflecting an increase in worker productivity (i.e., greater output from same time use*)*, as we cannot rule out the workers spending some of the saved commute time, either on the commute or at home, to generate the performance improvement. As we discuss later, while empirical evidence from multiple data sources and anecdotal evidence from our interviews do not suggest a significant increase in time spent at the office for affected workers, anecdotal evidence suggests that the less crowded and bumpy commute, and phone and internet connectivity allowed some of the affected employees to undertake work during their commute. Thus, our results could be seen as evidence that the subway serves as a productivity-enhancing technology that reduces "dead time" wasted in commutes. Nevertheless, we are careful to avoid using the term "productivity" when interpreting the bonus results, and use the broader worker "performance" term throughout.

results indeed reflect improvement in underlying worker performance for affected workers, utilizing individual-level annual sales data available for marketing personnel. While our baseline analysis can be viewed as an "intent-to-treat" analysis (as we classify as "affected" any worker for whom the fastest public transport commute time was lowered by Line 15), results are robust (in fact, stronger) when using self-reported data (from a survey of remaining workers in 2017) to define (or instrument for) affected status. Results are also robust to dropping employees that moved before the opening of Subway Line 15; to including controls for housing costs (so that changes in bonus pay were not the result of additional effort induced by increased housing costs for affected workers); to excluding potential commuters on an adjacent subway line (in fact, results are stronger, consistent with reduction of crowding and improvement of commute on the nearby line); and to using alternative bonus measures. We also verify that the results are not impacted by differential pay-performance sensitivity for affected workers; not offset by negative spillovers on coworkers or subordinates; and also hold in a sample of workers who continue through the end of the sample period.

We explore the heterogeneity of our baseline bonus results to provide additional insights. First, we find more statistically significant bonus effects for non-managers relative to managers, and larger effects for marketing workers relative to other occupations (i.e., Administration and Technology). Examination of the bonus share of income (highest for marketing workers) and coefficient of variation of performance measures (highest for marketing, and lower for managers relative to non-managers) show that the observed heterogeneity effects are consistent with the predictions of our model, since additional effort yields relatively greater benefits for these groups of workers. Qualitative interviews with marketing and other employees also suggested superior ability of marketing employees to make use of the better commute (to undertake work during the commute).

Second, we use potential time saved by the subway as a continuous measure of the usage of the subway and find that the positive effect of the subway on bonus pay is indeed correlated with time saved by the subway, consistent with a specific role for commute time reduction.[8] Third, we find that access to office automation technology for paperless transactions, which also facilitated remote work for a subset of affected employees, significantly weakens the measured effects of the opening of the subway station. This finding reinforces the interpretation that the effect of the subway was through improved commutes, so that when workers have access to alternative technology to avoid commutes, the subway is less beneficial.

Fourth, we explored the heterogeneity of the impact of the Subway Line 15 with demographic characteristics. We find an interesting U-shaped relationship with age: the impact of the subway on worker bonus appears to be lower for a middle range of ages than for younger and older employees. We also find that the effects are more subdued for workers with young (less than 12 years old) children. These findings are consistent with middle-aged workers and those with young children devoting time and energy saved by the better commute towards their home rather than work.

We explore two broad channels for the observed effects, by considering worker performance as a simple product of worker intensity of effort (i.e., output per unit of time or productivity) and work time. Then one simple channel for improved worker performance could be from workers allocating some of the time saved from the reduced commute towards work. Surprisingly, however, we find no evidence for an increase in time spent at the workplace. Direct analyses of data on attendance, and late and early arrivals suggest no DID change in related behavior by affected workers. Similarly, analyses of time at work, and arrival and departure time (from minute-level data on all worker-level swipes in and out of the office building) provide no evidence for affected workers spending additional time at the office. Qualitative comments from our survey and interviews confirmed that most workers did not see the subway as affecting punctuality or time at work (as they were already strongly motivated by company norms in the pre-subway period). The lack of any increase in time at work suggests

[8] This also assuages potential concerns from unobserved heterogeneity of workers able to use the subway line because, while there could be unobserved differences potentially correlated with changes to performance in terms of workers who choose to live in different parts of the city, this is less likely to be incidentally correlated with time saved by the commute within the group of affected workers. Results are robust to using a self-reported time-saved measure from our survey (instead of the measure we construct based on estimates from the Baidu Direction API, as discussed in Section 4.2). Our baseline DID approach includes individual fixed effects, and hence controls for potential static biases from any relationship between unobserved heterogeneity and worker performance.

a prominent role for increased intensity of effort, potentially fueled by increased well-being. A rich literature finds strong evidence that shorter, less crowded and more predictable commutes are associated with higher subjective well-being (e.g. Stutzer and Frey, 2008; Chatterjee et al., 2017; Zhu et al., 2017), and a separate literature has documented strong links between workers' happiness and workers' effort and creativity (Porto 2016). Thus, the reduction and improvement of commutes for the affected workers could have impacted performance by improving subjective well-being, which translated into greater intensity of effort per unit of work time. Interviews with affected employees confirm that they value the improved "predictability of the commute", feel more energetic, and are able to devote more time to exercise, sleep, and taking care of family members.[9] However, we do not completely rule out a role for increased overall time spent on work: our interviews with marketing personnel revealed that they were able to use the improved (less crowded and work-friendly) subway commute to complete useful tasks in a number of ways including scheduling meetings, reviewing and finalizing sales contracts on laptops, and making and responding to client phone calls.[10]

Next, we investigate the effect of the new subway station on employee turnover. To the extent that the subway improved the non-pecuniary aspects of the job and potentially increased wages through an increase in worker performance, our model predicts a decline in propensity to exit for affected workers. Indeed, both regression estimates and event study graphs confirm that the job exit rate declines systematically for affected workers relative to unaffected workers.

We then investigate a final prediction of our model, that companies should be able to attract higher quality hires from locations close to the subway (conditional on wage) after the opening of the subway station. Using a sample of employees hired after the opening of the subway and data on performance scores (available for 2015 and 2016 from Company One) ,

[9] Improvement in sleep and exercise could also play a role in improving worker effort productivity (Lechner, 2015; Gibson and Shrader, 2018).

[10] Consistent with our findings, marketing interviewees suggested that others for whom work required stretches of time (e.g., to do research and development work), or face-to-face interactions with co-workers or subordinates (e.g., managers), would benefit less from the ability to work during the commute. Recent work (e.g., Jain, Clayton and Bartle, 2017) documents out how internet and phone connectivity is allowing subway (and ride share) commuters to work more effectively during their commutes, and how labor regulations in some countries allow some commuters to include travel time as part of their work day.

we find that given the same level of compensation, hires in locations affected by the subway on average have significantly higher performance scores. Although these cross-sectional regressions must be interpreted with caution, the results suggest that the subway facilitated hiring better quality workers.[11]

While our results show gains from improved commutes for workers, one immediate question that arises is whether benefits accrue to shareholders of affected firms as well. Testing firm-level outcomes requires more than the two firms we use in our worker-level analysis. We collect data related to the start dates of forty-eight subway line construction events in Beijing, and identify thirty-seven publicly listed firms impacted by the new construction. We find evidence for positive abnormal returns around the construction commencement events, consistent with an increase in shareholder value from proximity to a new subway.

In addition to presenting novel evidence on the improvement of commute on worker outcomes and shareholder value, our paper contributes to four related streams of research. First, it extends the recent literature studying the effects of workplace and external environments on worker performance. Close in spirit to our work is an influential study of telecommuting by Bloom et al. (2015) that rigorously tackles identification issues by randomizing workers into telecommuting, and finds that working from home significantly improves worker performance. While their results are consistent with ours in that elimination of commute is associated with improvement in worker performance, telecommuting changes other important aspects of the employment relationship, including the degree of monitoring and visibility of telecommuting workers, and social interactions between workers, coworkers, and managers.[12] van Ommeren and Gutierrez-i-Puigarnau (2011) find that commuting distance is positively correlated with absenteeism; we find no direct impact on punctuality or time at work.

Second, our paper contributes to the literature examining the impact of commuting on

[11] This hiring effect does not impact our worker performance and turnover analysis because the sample for estimating them excludes workers hired after the subway opens (as our use of worker fixed effects requires use of workers with observations both before and after the subway station opened).

[12] Because the key confounding effects from reduced monitoring, lower visibility, and lower in-person social interactions with co-workers and managers are likely to diminish worker performance, the Bloom et al. (2015) results are likely to be consistent with productivity gains from savings in commute time alone. Other work that has examined the effect of working from home include Dutcher and Saral (2014) who found lower effort by team partners of telecommuters, and Dockery and Bawa (2014) who find that telecommuting is viewed as a positive job attribute but raises concerns about intrusion of work into non-work domains; Allen et al. (2015) provide a comprehensive review.

individual-level outcomes. Our model (particularly the possibility of fatigue affecting worker productivity) is related to that in Rupert, Stancanelli, and Wasmer (2009), who examine job selection, wages, and commute using French time-use data. Fu and Viard (2019) find a reduction in work time from commute increase (induced by relocation of undergraduate teaching to a satellite campus) for faculty who, unlike workers in our context, have flexibility in choosing work hours. Our results on reduction of worker exits is in line with Manning (2003) who discusses the dependence of separations on commuting distance. Monte, Redding and Rossi-Hansberg (2018) present a model (and estimates) for welfare increase from reduction in commute costs (which allows workers to reside in high amenity locations and commute to work in high productivity locations).

Third, our paper contributes to the broad literature investigating the drivers of firm-level productivity (Syverson, 2011), and specifically complements studies finding that better traffic conditions increase firm and aggregate productivity (e.g., Tang, 2014; Gibbons et al., 2019; and Firth, 2017). Anderson (2014) uses variation from a transit worker strike to show that it negatively impacted highway delays, suggesting higher net benefit of transit systems than previous estimates. Our paper finds that access to subway stations can improve employee-level performance, which could be an important source of improvement in firm productivity.

Finally, our study adds to the broader literature on the effect of infrastructure on local economic development.[13] Our paper differs from these papers by looking at the effects of subways on individual employee-level outcomes, highlighting one potential channel – the effect on individual worker performance – for broader effects on local economic growth.

The remainder of this paper is divided as follows: Section 2 provides brief background information on the new Subway Line 15. Section 3 provides a simple theoretical model to help guide and interpret the empirical analysis. Section 4 describes the data and key variables. Section 5 discusses the empirical strategy, presents main results, robustness checks, and heterogeneity tests, for baseline bonus effects. Section 6 discusses potential channels for the

[13] For example, railways and highways have been found to affect local GDP (Aschauer, 1990; Banerjee, Duflo, and Qian, 2012; Baum-Snow et al., 2015; Bogart et al. 2015; Wang and Wu, 2015; Jaworski and Kitchens, 2016; Asturias and Ramos, 2017; Berger and Enflo, 2017) and development as measured by night lights (Alder, Roberts, and Tiwari, 2018; van der Weide, Rijkers, Blankespoor, and Abrahams, 2018).

observed bonus effects. Section 7 presents analysis of employee exit and hiring, and Section 8 presents complementary evidence on shareholder returns from subway construction. Section 9 concludes.

2. Background

The Chinese government has invested heavily in subway systems over the last decade, investing about $189.1 billion between 2010 and 2015 (Lu et al., 2016). By 2016, twenty five Chinese cities had invested in subway systems (Lu et al, 2016; China Urban Construction Statistical Yearbook, 2017), compared to just three cities in 2000. Seven of the 12 largest metro networks in the world by length are now in China, and Beijing and Shanghai have the longest systems in the world (Freemark, 2018). Chinese subway systems carried over eight million passengers a day by 2015 (Lu et al., 2016). As China's capital, Beijing in particular has ushered in a rapid and substantial development of its subway in the past two decades (see Appendix Figure 1). Before 2000 only two subway lines existed in Beijing; to prepare for the 2008 Beijing Olympic Games, the government speeded subway construction and construction has not slowed since.

Our paper focuses on Subway Line 15 in Beijing which connects Shunyi District, located in the northeastern part of Beijing, and Haidian District, located in the northwestern part of Beijing (Figure 1). Line 15 became operational in two steps. The eastern part, which connects Shunyi District and Chaoyang District located in the north-central part of Beijing, was put into operation on December 30, 2010. The line was then extended to Haidian District and the western part was put into operation on December 28, 2014.

The operation of Subway Line 15 facilitates commutes for people working or living near the stations. Our paper focuses on workers of two companies in Tongfang Science and Technology Plaza. The final stop (or Terminus) of Line 15 (Qing Hua Dong Lu Xi Kou) is a subway station located beneath the Plaza. The plaza is located at the intersection of two very busy streets (Shuangqing Road and Wangzhuang Road). Before Line 15 opened, workers at the two companies could drive or take buses, which involved severe congestion around the intersection, or take an alternative, Subway Line 13. This alternative line is inferior for a numbers of reasons (especially for the return trip home). First, the closest station for Line 13 (Wudaokou) is located about 1.1 kilometers from the Plaza. So Line 13 requires an additional 15 to 20 minutes of walk each way (relative to the walk from the new Line15 station), which is particularly unappealing on the (frequent) high pollution days in Beijing. Moreover, Wudaokou station of Subway Line 13 is extremely busy, and the security check for boarding involves delays in excess of ten minutes during rush hour. Finally, because Wudaokou station is not a terminus, it is practically impossible for riders boarding at this station to get a seat.

3. A Simple Model

We present a simple stylized model to guide the empirical work. We draw on the standard principal-agent framework (e.g., Holmstrom, 1979; Holmstrom and Milgrom, 1991), and abstract from risk aversion and uncertainty (proofs are provided in the Theory Appendix).

Output in dollars $q = f(e) \cong \eta e$, so that the Principal (employer) profit is

$$\Pi = q - w = \eta e - w$$

where η is a parameter that indicates the extent to which the (measured) output that the principal wants to maximize is impacted by the agent's effort (or performance) e, and w is the total wage in dollars. The dollar equivalent utility for each worker is given by

$$U = w - C(e) + h = w - \gamma \frac{e^2}{2} + h$$

where γ captures the difficulty in exerting effort, and h is the (exogenous) non-pecuniary utility associated with the current job. To induce worker effort, we assume the firms use a linear contract, which is known to be ubiquitous in practice (Carroll 2015), so

$w = \alpha + \beta q = \alpha + \beta\eta e$. Optimal effort choice for the worker is then given by

$$\max_e U = \max_e \left(\alpha + \beta\eta e - \gamma \frac{e^2}{2} + h \right)$$

which yields optimal effort

$$e^* = \frac{\beta\eta}{\gamma} \qquad (1)$$

so that the maximum utility derived by the worker is

$$U^* = \alpha + \beta\eta e^* - \gamma \frac{e^{*2}}{2} + h = \alpha + \frac{\beta^2\eta^2}{2\gamma} + h \ . \qquad (2)$$

The principal's optimization will take the worker's optimal effort choice as a binding incentive compatibility (IC) constraint. Next, we assume that the worker has an outside offer that provides utility $\bar{U}$ unrelated to worker effort choice or contract structure. This imposes an individual rationality constraint (IRC) that the worker will not accept a wage contract that yields lower utility than $\bar{U}$. Because the principal's profits are declining in wage paid to the worker, the IRC would be binding in optimum, so from (2) we get

$$\alpha = (\bar{U} - h) - \frac{\beta^2\eta^2}{2\gamma} .$$

The principal then maximizes

$$\max_{\beta}(\eta e^* - w) = \max_{\beta}\left(\frac{\beta\eta^2}{\gamma} - (\alpha + \beta\eta e^*)\right) = \max_{\beta}\left(-(\bar{U} - h) + \frac{\beta\eta^2}{\gamma} - \frac{\beta^2\eta^2}{2\gamma}\right)$$

which yields optimal β and α as

$$\beta^* = 1, \qquad \alpha^* = (\bar{U} - h) - \frac{\eta^2}{2\gamma} . \tag{3}$$

<u>Assumption 1</u>: Improvements in commute quality δ make it easier to exert effort, so $\frac{\partial\gamma}{\partial\delta} < 0$.

Effort could be viewed as a simple product of work time and intensity of effort, as we discuss in Section 6. Within a broader model of time choice, the cost of effort parameter γ would capture factors affecting the marginal disutility from devoting additional time to work; a reduction in commute (by freeing up dead commute time) could be seen as reducing the marginal disutility (and hence leading to effectively more effort by increasing work time). A better commute could also allow workers to use part of the commute for work (so better commute reduces the cost or difficulty of working during the commute). Finally, a better commute could translate to better wellbeing (as in Assumption 2 below) and this greater wellbeing could translate into greater work intensity or focus, per the literature linking wellbeing and productivity (reviewed in Porto (2016)).

<u>Assumption 2</u>: Improvements in commute quality δ yield non-pecuniary utility, so $\frac{\partial h}{\partial\delta} > 0$.

This is motivated by a vast literature[14] that has shown that longer, more crowded, and less predictable commutes are correlated with lower subjective wellbeing.

These two assumptions yield the following prediction:

[14] For example, Kahneman et al. (2014), Stutzer and Frey (2008), Choi, Coughlin, and D'Ambrosio (2013), Chatterjee et al. (2017), and Zhu et al. (2017).

<u>Prediction 1</u>: Improvement in commute quality increases worker effort, and hence the bonus component of wage.

The intuition here is straightforward; in this stylized model, the bonus rate (which equals unity) is independent of commute quality; however, the lower cost of effort (i.e., lower marginal disutility from additional work time or improvement in effort intensity induced by greater wellbeing) means that there is effectively more effort exerted by the worker if her commute quality is improved, which then translates to a larger total bonus.

<u>Implications for heterogeneity of bonus effects</u>**:** The impact of the improvement of commute on bonus depends on two parameters (i) η, and (ii) $\frac{\partial\gamma}{\partial\delta}$. In our setup η could be viewed as indicating (a) how much effort is translated into actual output, or alternatively (b) how strongly the *measured* output relates to worker effort. Hence we should expect larger effects on bonus for workers for whom the measured individual output is more strongly correlated with individual effort, i.e., where output is easier to measure precisely, and where the common problem of low variance in subjective performance evaluations (e.g., Lazear and Gibbs 2014, Chapter 9) is less severe, and where there is a greater impact of commute improvement on effort.[15] These intuitive implications are captured in the lemmas below.

<u>Lemma 1a</u>: Assuming outside offers, cost effort parameter (γ) and non-pecuniary utility are similar across workers, <u>the effect of commute on worker effort (and hence bonus income) is larger for categories of workers with higher shares of bonus in total income</u>.

<u>Lemma 1b:</u> Assuming the variance of individual-level draws of the cost effort parameter (γ) is similar across categories of workers, larger variance of performance measures indicates higher sensitivity of measured output to effort (η) and hence <u>the effect of commute on worker effort (and hence bonus income) is larger for those categories of workers with larger variance of performance measures</u>.

Finally the output of some type of workers (e.g., for R&D workers or managers) may be

[15] In a more general context, there could be variations in optimal bonus level as well (e.g., Holmstrom and Milgrom (1991) suggest lower powered incentives (i.e. $\beta < 1$) may be optimal if agents undertake multiple tasks and hence distortion could be a concern). Thus, more generally, when $\beta^* \neq 1,$ and measured output is a general function of effort $q = f(e)$ we would have $\frac{\partial(\text{bonus payments})}{\partial\delta} = \frac{\partial\beta^* f(e^*)}{\partial\delta} = \beta^*\left(\frac{\partial f(e^*)}{\partial e^*}\right)\left(\frac{\partial e^*}{\partial\delta}\right)$. In our model, $\beta^* = 1, \frac{\partial f(e^*)}{\partial e^*} = \eta,$ and $e^* = \frac{\beta^*\eta}{\gamma}$ so $\frac{\partial(\text{bonus payments})}{\partial\delta} = \beta^*(\eta)\frac{\partial e^*}{\partial\delta} = -\frac{\eta^2}{\gamma^2}\frac{\partial\gamma}{\partial\delta}$.

linked to team effort, so the cost of incurring effort that changes team output may be less affected by improvement in commutes at the individual level.

Lemma 1c: The impact on effort (and hence bonus income) is higher for workers for whom the improvement in commute more effectively reduces the cost of effort.

While the main focus of this paper is to examine the impact of commute improvement on worker performance, the model yields implications for base pay and total income. Intuitively, an increase in the non-pecuniary utility from the improved commute should benefit the firm in the wage bargaining process; this could lead to a decrease in base pay, so that the net impact of improved commute on aggregate compensation is ambiguous.

Corollary 1a: Improvement in commute quality decreases the non-bonus part of wage.

Corollary 1b: Improvement in commute quality has an ambiguous effect on total wage.

However, in our context, the firm may be constrained from cutting base pay for the subset of affected workers. In particular, the affected workers may see any cutback in the non-bonus pay as unfair, as it is induced by a change that is exogenous to the firm and the worker (i.e., not the fault of the worker). Further, reducing the base pay for affected workers may violate implicit work norms that link base pay to seniority levels.

The model predicts that improvement in commute improves worker retention.

Prediction 2: Improvement in commute quality reduces the probability of worker exit.

To see the intuition, consider the case where the outside wage offer has a uniform distribution $f(\bar{u})$ over $[0\ , U^{MAX}]$. Because the maximum bilateral value generated by the worker-firm match is $\frac{\eta^2}{2\gamma} + h$, the worker quits if her outside draw is in the range $\left[\frac{\eta^2}{2\gamma} + h, U^{MAX}\right]$. An improvement in commute quality shifts the bottom cutoff of this range to the right in two ways: by reducing the cost of effort (γ), and by increasing nonpecuniary utility (h), thus squeezing the range of offers that will induce the worker to exit.

Finally, the model also yields the intuitive prediction that, conditional on the same total wage, firms are able to attract better quality workers (i.e., ones with lower cost of effort), and hence higher performance, from locations with access to better commutes.

Prediction 3: Assuming similar outside utility, workers hired with access to better commutes

have lower cost of effort (γ) and hence exert greater effort, conditional on wage.

The intuition for the result is that since the firm sets total wages for each worker such that the IRC is binding, similar outside utility options imply that the workers with the better commute will be working harder (to equate utility conditional on the same wage).

4. Data and Variables

4.1. Data

Our quantitative data comes from two sources. We obtained administrative data on past and current employees from the two companies, and we also conducted a survey to collect other information from current employees of the two companies. The two companies are subsidiaries of a common parent company, which cooperated with us for this study. We also draw on two rounds of interviews with employees of both firms for qualitative evidence.

4.1.1. Administrative Data

This data set, obtained from the management of the two companies, includes detailed information of every employee who *ever worked* in these two companies between 2013 and 2016, both fixed (time invariant) characteristics, as well as time-varying monthly data.

The main time-invariant variables we use include gender, birthday, ethnicity, *hukou* type (whether household registration is agricultural or non-agricultural), education (including college, major, degree, year to get degree), joining date (which helps measure tenure at the firm), exit date (if employee left the company before the end of our sample period), and date of start of the first job (which helps measure total experience). This data set also provides information on the gender and birthdays of employees' children.

We obtain monthly information on each employee's compensation, insurance, income tax and other items withheld by company, position, occupation, and attendance. The two main components of employee compensation are base salary and bonus (discussed in more detail in Section 4.3 below).[16] In our data, we can differentiate not only managers and non-managers

[16] The data also include information on different types of insurance including unemployment insurance, endowment insurance, medical insurance, housing fund, and enterprise annuity, all of which are paid by companies. In addition to withholding income taxes, companies also withhold other fees such as union fees (and if the employee uses

(by position information), but also employees working in about twenty three different occupations, which we group into three categories — administration, technology and marketing.[17] Monthly information on each employees' attendance includes the number of days that the employee came late, left early, and took personal leave, sick leave, maternity leave, funeral leave, and marriage leave. (For some of the potentially dynamic variables, Company 1 only tracks the latest values; for consistency, we use the same basis for both companies, as discussed in Section 4.3 below.)

We clean the raw data in two ways. First, one complication in defining which workers were affected by the subway (discussed in more detail in Section 4.2 below) arises from workers moving to new addresses that change their affected status. Fortunately, we find only modest relocation; specifically, only 27 (7%) employees moved after the opening of Subway Line 15 during our sample period. We drop these employees and focus only on employees who did not relocate after Line 15 opened. Second, our empirical strategy for wage analyses (see Section 5.1 below) involves use of individual fixed effects, so that individuals who left before or who joined after the subway opened do not contribute to identification of the parameters of interest. Therefore, we include only those employees who are in the company workforce at least one month before and after the opening of the subway. Table 1 shows the numbers of employees and employee-month observations in the full sample and across the two companies, in the cleaned data set used in the baseline employee wage analyses.[18]

4.1.2. Survey Data

To complement the administrative data, in July, 2017 we conducted a survey of all employees in the two firms. We use responses from the survey for three types of information: (i) changes of home addresses since they joined the company; (ii) self-reports of use of subway and time saved by the subway; (iii) current and past (recalled) work-life balance for all

company provided housing, then rents, water fee, and electricity fee).

[17] Administration includes employees such as human resource personnel, accountants, and receptionists. Technical workers are those tasked with solving the technical problems of clients with the companies products and services. Marketing workers are those tasked with selling the companies products and services and to help provide consumers with the relevant information, such as pricing, and before- and after-sales services. The proportion of STEM (business) majors is about 28.8% (61.5%) for Administration, 75.6% (14.6%) for Technology and 52.1% (40.4%) for Marketing.

[18] The definitions of "Affected and "Control" workers are discussed in Section 4.2. The sample used for exit analysis is different, and is described in Online Appendix Table 1. The Online Appendix is available at: http://webuser.bus.umich.edu/jagadees/papers/commute/Online_appendix_consol.pdf

employees in each year of 2013-2017. Using employees' work ID (also collected in the survey), we link the information in the survey data with the administrative data. However, one caveat is that the survey misses information for employees who are in our data period (2013 to 2016) but had left the company prior to July 2017; we obtained a response rate of about 85% of surviving employees in 2017.

4.2. Definition of Affected Workers and Related Variables

The most important variable in our analysis is the one identifying whether employees were "affected" by the opening of Line 15. To construct this variable, we first use Baidu Direction API to obtain all public transportation routes as well as associated travel time from employee's home address to the company.[19] We then define affected workers (denoted by a *NearSubway* dummy variable) as those for whom the fastest public transportation route from his/her home address to the office includes Line 15.[20] Overall, 38.2% of the employees in our cleaned sample are affected by the subway (see row 4 of Column 1 in Panel A of Table 1).

We also use a *TimeSaved* variable, defined as the time difference between the fastest public transportation route including and excluding Subway Line 15 (set to zero for unaffected workers), as a continuous measure of the impact of the subway on commute time for affected workers.[21]

4.3. Key Outcome Variables

A. Summary Statistics

Table 2 reports the summary statistics for the main variables in the baseline sample. A

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[19] Baidu is a Chinese company providing services similar to Google (see https://en.wikipedia.org/wiki/Baidu). Baidu Direction API provides point–to-point travel directions by different modes, and the Chinese introduction of Baidu Direction API can be found at http://lbsyun.baidu.com/index.php?title=webapi/direction-api-v2. The average time reported by Baidu Direction API is not affected by the time when the user put in the request.

[20] Employees' home address for those workers who have left the companies is based on the data available in the companies' records. Because there is some potential measurement error in this variable (as workers sometimes use their native address outside of Beijing for company records), we update this using information from our survey of existing employees based on the address they report for the start of our panel period (January 2013, or the month a new employee entered the company). As discussed earlier, the few (7%) workers who move after the opening of the subway are excluded in our baseline cleaned sample.

[21] Specifically, to measure time saved, we group all public transport routes into two categories, one including and the other excluding Line 15. We take the minimum time in each group, and then define time saved by taking Line 15 as the difference of the minimum time of these two groups. If the minimum time in the group including Line 15 is larger than that of the group without Line 15, then the time saved is zero. This measure is assigned missing for two employees for whom all of the routes provided by Baidu Direction API include Line 15. In analysis discussed in Section 5.2.E, we check robustness to using self-reported data (from workers in our survey dataset) on time saved by Line 15.

primary focus of the paper is on examining the impact of access to the subway on worker performance. Because we do not have full panel data on direct performance measures, we use available monthly individual-level panel data on bonus pay as a proxy for performance (below, we verify the link between bonus and available worker performance measures). Bonus and income variables are measured monthly and deflated to 2013 using the CPI (Beijing Statistical Yearbook, 2017). For a small number of employee-month observations, we find that the bonus amount was negative in the data; per discussions with management, we confirmed this was not due to measurement error, but due to a genuine underperformance penalty imposed by the firm. In order to retain the information in the negative bonus numbers while still undertaking analysis using log of bonus compensation, we adjust all of the bonus amounts, as well as the total income, using a constant such that the minimum value of the adjusted bonus is equal to one. Our baseline analysis reports results from variables used in levels, and we check robustness to using alternative measures (See Section 5.3.J).

Age, *experience*, *tenure*, and *number of children* vary over time (except in propensity model in Section 5.3.C where we use the initial value for the worker in the panel). *Education*, *Party Membership*, *Hukou*, and *Marriage Dummy* are static and measured on the last date when the information of these variables is available for each employee.

Consistent with Table 1, the mean of *NearSubway* dummy is 0.389, indicating that 38.9% of worker-month observations relate to affected workers. The affected workers save, on average, 21.765 minutes on a one-way commute (about 43.53 minutes on the round-trip commute). *Bonus* is a significant component of total income for workers in the companies; the average monthly bonus is 4944.79 (2013RMB), which is about 41.96% of the average total income of 11785.1 (2013 RMB). In Section 5.3.C, we explore differences in key characteristics between affected and unaffected workers, and undertake a matched DID analysis to address potential concerns.

B. Verifying the Worker-Level Bonus-Performance Link

Our model and interpretation of results hinge on a direct link between bonus and worker-level performance measures. Our conversations with managers and workers at both companies confirmed that bonuses are linked to quantitative and qualitative evaluations of worker performance (with variations across occupation groups). Nevertheless, we use available data on alternative performance measures to empirically verify the link between bonus and worker performance.

Fortunately, Company 1 tracks individual-level performance scores at a monthly frequency, with this data available for 2015 and 2016 (summary statistics are presented in Appendix Table 1a).[22] We also have annual data tracking individual-level sales for marketing personnel in both companies for the full four-year period. To establish that bonus income is indeed strongly correlated with individual-level performance as posited in our model, we regress the individual monthly log(bonus) on these performance scores, as well as one year lead of the individual sales measure.[23] Results presented in Appendix Table 2 confirm that variation in bonus is indeed strongly correlated with worker performance scores (in Panel A) and with individual sales measures (in Panel B), both in the cross-section across workers, and (importantly for the relevant variation used in our DID analysis) within-workers as well.

5. Empirical Strategy and Results for Wage Components

5.1. Empirical Strategy

We adopt a standard DID strategy to estimate the impact of the opening of the Line 15 station on different individual outcomes in wages. The baseline regression specification is

$$y_{ict} = \beta . NearSubway_i \times T_t + \alpha_i + \alpha_{ct} + \varepsilon_{ict} \ . \qquad (4)$$

Here y_{ict} is the outcome variable for employee i in company c in year-month t. $NearSubway_i$ is a dummy variable equal to one for the affected employee i, constructed as described in Section 4.2. T_t is a dummy variable equal to 1 for the period after the opening of Line 15 station and 0 otherwise. As is standard in the DID approach, we control for individual

[22] Human Resources managers at Company 1 informed us that the performance measures were set as follows: the immediate superior assigns each employee rated workload (a set of tasks) every month. If the employee finishes this set of tasks on time, her performance score would be 100 points; if she completed more (less) than her allocated tasks, the measure would adjusted proportionately. In some of our analysis, we scale this score by a factor of 0.01.

[23] We use leads to account for the fact that bonus is paid based to a significant extent on sales *initiated* in the current period, while the individual sales measure we have is sales *executed* in the current year. Thus we expect current period bonus to be more strongly related to next period executed sales –regression results confirm this is the case. More specifically (and to explain residual variation unexplained by sales), per our discussions with the companies, the bonus payments at the end of a period are linked to a combination of (i) current period measures including sales contracts initiated in the current period; (ii) actual sales executed related to contracts the marketing person may have initiated in earlier periods; and (iii) measures of current period efforts including number of client visits and new client contacts made.

fixed effects α_i to remove any individual-level time-invariant factors potentially correlated with the outcome variable, and use company-year-month fixed effects α_{ct} to allow period-specific shocks to vary across the individual companies. (We do not include $NearSubway_i$ and T_t separately because they are absorbed by α_i and α_{ct}, respectively.) ε_{ict} is the error term. To deal with potential heteroscedasticity and serial correlation within individuals, as well as across individuals within a period, we estimate standard errors by allowing for two-way clustering over individuals and year-months. The coefficient of interest is β, which captures the DID change of the dependent variable following the opening of the subway station.[24]

For studies examining the effects of new subway station at a more aggregated level, one significant concern is potential endogeneity of the location of the station. In particular, companies (that have unobserved advantages) could lobby the local government to influence the location of the subway stop, so that subsequent company-level changes may be driven by the unobserved advantages that influenced the choice of the station location. Because our focus is on employee-level outcomes, we are able to compare employees within the company, which addresses potential bias from this concern. There are other remaining concerns about identification, including about potential differences in pre-existing time trends of outcome variables across the affected and controls groups; we address these in robustness checks in Section 5.3.

5.2. Baseline Results

Table 3 presents baseline DID results (specification (4)). In all specifications, we control for individual fixed effects and company-month fixed effects. The coefficient of 636.328 on *NearSubway* × *Post* for bonus income (significant at 10% level) in Column 1 implies that, relative to employees not affected by Subway Line 15, bonus income of affected employees increases by 12.89% (636.328/4938.247) relative to the mean bonus level after the opening of the Line 15 station, which translates to 11.52% (636.328/5525.948) of the standard

[24] For individual-level analysis of exit, we include data on workers who exit before the subway opened, and use duration models (see Section 7.1).

deviation of the bonus measure. The results are similar using the log of the adjusted bonus variable in Column 4; the estimated coefficient of 0.049 (also significant at the 10% level) implies a change of 580.650 (0.049*(4944.790+6905.22) in level terms (after accounting for the linear shift of 6905.22 in the adjusted bonus variable), which translates to a 11.75% (580.650/4944.790) effect at the mean bonus level, and 12.56% of the standard deviation of the dependent variable (0.049/0.390).

Thus, in line with Prediction 1 of our model, it appears that an improvement in commute does translate into higher bonus income, suggesting that a better commute leads to better worker performance. The estimates from Panel A of Appendix Table 2, combined with the summary statistics on the performance score (in Appendix Table 1a) imply that the baseline estimate of 0.049 for log($Bonus^T$) (in Column 4, Table 3) is equivalent to an increase in the scaled performance score of about 0.104 points (based on coefficient estimate of 0.470 for the full sample with employee fixed effects in Appendix Table 2). Given the standard deviation of the scaled performance measures in the sample is 0.244 (Appendix Table 1a), this implies that affected workers saw an improvement in performance score of about 0.427 (0.104/0.244) of the standard deviation.

The opening of Subway Line 15 does not have a significant effect on total income excluding bonus, in Column 2 of Table 3. Intuitively, an increase in the non-pecuniary utility from the improved commute should benefit the firm in the wage bargaining process, leading to lower non-bonus compensation, as reflected in Corollary 1a. However, as discussed in Section 3, the firm may be constrained from cutbacks to non-bonus pay for affected workers as that could be perceived as unfair (i.e., for no fault of the worker), and because base (i.e., non-bonus) salary may be set implicitly linked to seniority and/or position so as to make it difficult to adjust specifically for the affected workers.

In Column 3, the opening of Line 15 is associated with an increase in total income of affected employees by 2.9%, significant at the 10% level; the lower aggregate effect is not surprising given the small effect on base pay, which offsets the bonus increase. In the context of our Corollary 1b, it appears that on average, the positive impact through the effort-bonus channel is not offset by the potential negative effect through utility-base pay channel.

In summary, we find that the opening of Subway Line 15 increases total income of employees affected, driven mainly by the positive effects it has on bonus.

5.3. Robustness Checks of Baseline Results

We conduct a battery of robustness checks for the baseline results; because our main variable of interest is worker performance proxied by bonus income, most of the tests focus on the impact on bonus income. For brevity, most of the results are moved to the Online Appendix.

A. Testing for Parallel Trends and Timing of Changes

A key assumption for the validity of DID estimation strategy is that the pre-existing time trends of outcome variables are parallel for affected and unaffected employees. To test whether this assumption holds, we examine an event study graph (Figure 2) that presents the differential trend for affected workers (relative to unaffected) conditional on individual and company-quarter fixed effects, before and after the subway line opening. Figure 2 confirms that there is no differential time trend for the affected group of workers prior to the opening of the subway for our main variable of interest, bonus income in Panel A (as well as for non-bonus income in Panel B). Moreover, there is a significant jump in the relative trend in bonus for the affected group, immediately following the opening of the subway, and this relative change remains persistently positive until the end of our sample period. The timing and persistence of the change is consistent with the access to the subway producing a differential improvement in performance for the affected workers (in line with Prediction 1 of our model).[25]

B. Permutation Tests

To address the potential concern that observed effects may be purely by chance (in our context from heterogeneous bonus increases for a random subset of workers), we adopt a randomization inference approach, analogous to Fisher permutation tests (Rosenbaum, 2002), by randomly assigning the dummy for being near Subway Line 15 and then estimating Equation (4), replicating 2000 times. Figure 3 shows the distribution of the estimated 2000 coefficients; the probability that a coefficient of the same or larger size to what we obtain is very low (about

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[25] We also examined the trends of the raw values of bonus and total income excluding bonus (Online Appendix Figure 1); consistent with Figure 2, we find that the trends are indeed parallel for both bonus (and total income excluding bonus) before Subway Line 15 station opened, and there is a relative increase in bonus for the affected group after the opening of the subway.

4.4%), implying that the baseline estimate is significant at the 5% level by this approach.

C. Propensity Score Matched DID

A comparison of affected and unaffected workers in Panel A (Columns 1 to 4) of Table 4 shows that there are systematic differences in characteristics. In particular, affected workers are younger, have less experience and tenure, are somewhat less likely to be male, and live on average 2 kilometers further from the office. Although individual fixed effects control for static biases from these differences, and even though our test suggests there are no differential trends induced by the level difference in some of the observed characteristics, we undertake a robustness check by using a propensity score matched sample.

To do so, we posit a model that predicts propensity to be affected by the subway based on the observables, then use the optimal matching approach in Ho et al. (2011) to construct a one-for-one matched sample. Table 4 confirms significant improvement in balance on observables in the propensity matched sample, and the estimation results reported in Table 5 are very similar to, and confirm robustness of, the baseline estimates in Table 3. We verify parallel pre-trends in the matched sample using an event study figure (Online Appendix Figure 2) and find it very similar to the analogous Figure 2 for the full sample. Similarly, raw trends for affected and control workers (Online Appendix Figure 3) confirm relative increase in bonus for affected workers coincident with the opening of the new Line 15.

D. Using Individual-level Marketing Sales

As discussed in Section 4.3.B, we do have a direct but limited measure of worker performance – individual-level annual sales for marketing personnel. We use this data to provide confirmatory evidence that the baseline results for bonus increase was related to actual worker output (as posited in our model). As discussed earlier, because the individual-level sales data we have is the annual executed sales for the year which would have been initiated several quarters earlier, and because our discussions with management revealed that bonus is linked strongly to sales initiated in the current period, the executed sales measure is a lagging indicator of past performance. Accordingly, we define the Post_t period as equal to 1 for the period post 2015 (instead of 2014), to account for lags between executed sales and contracted sales. The results, presented in Appendix Table 3, suggest there was a DID increase in sales for affected individuals (and the impact was correlated with time saved by the commute). This evidence suggests that the bonus effects are indeed reflective of superior worker performance (and not an artifact of poorly provided incentives unrelated to worker effort), at least for the subgroup of marketing workers.

E. Using Self-Reported Information from Survey Data

Our baseline definition of the *NearSubway* dummy variable for whether a worker is affected is based on the information provided by Baidu API. For personal reasons workers may choose commutes different from the fastest public transport route suggested by Baidu, so that our baseline analysis could be thought of as an "intent-to-treat" analysis where the treatment is improvement in public transport commute. As an alternative to this measure, we use self-reported data from our survey of workers in July 2017. We find the baseline results are robust (and in fact of larger magnitude) when defining *NearSubway* using self-reported data (Column 1 of Panel A of Online Appendix Table 2). An alternative use of this secondary measure is as an instrument for the Baidu-based measure, as it is reasonable for the measurement errors contained in these two variables to be unrelated. The second stage yields an even stronger effect (coefficient on 0.106) of the opening of the subway on bonus. This analysis has the limitation that we only use data on workers who survive to July 2017 and respond to our survey (which had about 85% response rate).

F. Dropping Employees Moving Before the Opening of the Subway Line

One concern is that capable employees (or those more likely to benefit) might move to subway line in anticipation of its opening, leading to bias in our estimates. Although we have shown that there are no different pre-existing trends for affected and unaffected groups (see Section 5.3.A), to address this concern, we dropped employees moving before the opening of subway line and re-estimate Equation (4), and also those who joined (in different windows) before the opening of the subway. The results are robust (Online Appendix Table 5).[26]

G. Controlling for Effects from Housing Costs

The opening of the subway station could have caused a change in housing costs for

[26] Our baseline analysis dropped the handful of workers who moved after the subway opened, and hence our baseline analysis is not impacted by movement after the subway opened. Incidentally in the data we do not find a significant movement of workers toward the subway, consistent with comments from interviewers that multiple factors in addition to own commute, including spouse's commute and children's schooling, influences home location choices.

affected workers. If it led to an increase, the negative housing cost shock could be a channel (unrelated to our model) that links subway access to increase in work effort. To address this possibility, we collated panel data on housing price and rental prices, then repeated the specification used in our baseline analysis, adding a control for log of housing price (Online Appendix Table 3) and rental prices (Online Appendix Table 4). The results are robust, ruling out a role for changes in housing costs as an alternative explanation for the observed changes in worker performance.

H. Checking for Differences in Pay for Performance Sensitivity

One concern with the baseline estimation could be that unobserved worker characteristics are correlated with affected status and with the pay-performance sensitivity, so that estimated effects are influenced by this factor as well. To check for this possibility, we include an interaction of the *NearSubway* dummy with the Performance Score in a regression of Log (Bonus) on the Performance Score; we find no differential pay-performance sensitivity for the affected workers in the full sample or within different occupation/ position groups (Online Appendix Table 6).

I. Potential Contamination Effects on Other Subway Lines

We control for potential contamination effects on other nearby subway lines. In particular, the opening of Subway Line 15 might divert some passengers from Line 13 to Line 15, which could have some positive effects in the form of reducing crowding for those still taking Line 13. To control for this potential contamination, we drop employees taking Line 13 from the unaffected group and re-estimate Equation (4). The results (Column 1 in Online Appendix Table 7) show estimates larger in magnitude (a 10.8% increase in bonus for all employees) and statistical significance than the baseline estimate, consistent with a positive spillover effect on commuters using the alternative line.[27]

J. Using Alternative Samples and Bonus Measurements

We conduct robustness checks using alternative samples and bonus measures. We re-estimate the results with the sample excluding affected (unaffected) employees who have

[27] We also checked robustness to excluding affected workers whose fastest route includes Line 5 and Line 15, because the transfer station between Line 5 and Line 15 opened only in December 2015; results are robust (Column (2) in the Online Appendix Table 7).

unaffected (affected) employees living within 250, 500 and 750 meters (Online Appendix Table 8) to avoid possible measurement error in defining *NearSubway* dummy. We checked robustness to winsorizing the bonus level variable (at 1% and 5%), and to using log unadjusted bonus (which drops all non-positive, i.e., ≤ 0, observations), as well as using a Box-Cox transformation of the bonus variable; for all four alternative cases, we find a significant increase in bonus after the opening of the new Line 15 (Online Appendix Table 9), with economic magnitudes similar to baseline estimates.

K. Spillover Effects on Subordinates and Co-workers for Bonus Income

If there are negative (positive) spillover effects to peers and subordinates, then the observed baseline effects for affected workers may be misleadingly large (small) as a measure of aggregate effects for the companies. The raw trends graphs (Online Appendix Figures 1 and 3) do not indicate a negative effect on unaffected workers, suggesting no strong negative spillover effects. Nevertheless to directly test for spillover effects, we define a dummy variable $NearSubway^c$ which is equal to one if any of the other employees in the same department are affected by the opening of Line 15. Then, we estimate the specification in Equation (4) by including this dummy and its interactions with the post dummy, as well as with the affected dummy. Results (in Online Appendix Table 10) suggest a large positive spillover effect (positive coefficient on the $NearSubway^c * Post$). However, we are cautious about interpreting this spillover effect, as only a small fraction (<10%) of workers do not have an affected colleague (mean of $NearSubway^c$ is 0.914), so the variation available for identification appears to be limited. Nevertheless, these results suggest that the aggregate impact of the subway on worker performance is not attenuated due to negative spillover effects (and that baseline estimates may, in fact, be biased downward).

L. Effects Conditional on Continuing till End of Sample Period

Differences in worker quality among exiters from the affected and control groups may impact the baseline results. We confirm robustness using a sample that conditions on survival to the end of our panel period, comparing effects for continuing affected employees to continuing unaffected employees (Online Appendix Table 11).

5.4. Testing for Heterogeneous Effects

In this section, we investigate the heterogeneity along different dimensions in the robust bonus income effects documented in the previous section.

5.4.1 By Different Positions and Occupations

A. Results by Position & Discussion of Possible Explanations

The administrative data includes a label indicating whether the employee is a manager or not. Roughly, an employee position is categorized as a manager if she is a (deputy) CEO, CTO, general manager, chief inspector, department manager, or team leader.

Columns 1 and 2 of Table 6 show the effects on samples split by position and by occupation (using designations at the end of 2014, just prior to opening of Line 15). The coefficient of $NearSubway \times Post$ is 0.051 and significant at the 5% level for non-managers (Column 1) while the same coefficient is 0.066 but is not significant for managers (Column 2).

Lemmas 1a, 1b and 1c suggest three potential sources for heterogeneous effects. Lemma 1a implies we should expect greater impact on bonus for employees for whom bonus is a larger share of income. In Appendix Table 1b, we do find that managers have a higher bonus share; while this is consistent with the higher point estimate in Table 6, Lemmas 1b and 1c suggest reasons why the impact is not statistically significant. In particular, our interviews with managers and employees suggested that the link of bonus to actual individual performance could be weaker for managers, because for many of them, their performance was self-evaluated and more subjective, and therefore generally anchored to higher scores, leading to lower responsiveness and variation of measured performance to actual performance. Consistent with these concerns expressed by our interviewees, we find that the coefficient of variation (i.e., the ratio of the standard deviation to the mean) is indeed significantly lower for managers (0.23) relative to non-managers (0.305). Thus, per Lemma 1b, we should expect a lower bonus effect for managers, which could be one explanation for the lower statistical significance for them in Table 6. Finally, it is plausible that, because their work involves more face-to-face meetings with others (including unaffected workers) in the office, they are less able to utilize the better commute to increase their work effort (Lemma 1c).

B. Results by Occupations & Discussion of Possible Explanations

We group about 23 reported occupations in the administrative data into three broad categories: administration, technology, and marketing (see footnote 16).

Columns 3-5 of Table 6 show the results by occupation categories. The estimated effects are largest in magnitude and statistically significant only for marketing employees (Column 5), for whom the coefficient of *NearSubway* × *Post* is larger (0.094) and significant at the 1% level. The estimate is positive and similar to baseline estimate in magnitude for administrative employees but statistically insignificant (Column 3), and smaller, negative and statistically insignificant for technology workers (Column 4).

Again, Lemmas 1a, 1b and 1c suggest potential explanations for the relatively stronger effect for marketing personnel. First, we find that the mean share of bonus in total income is largest for marketing; hence per Lemma 1a, we should expect the stronger effects we find for marketing personnel (intuitively, bonus matters more for them and hence induces greater response of effort to the commute improvement). Second, in Appendix Table 1a we find much lower coefficients of variation for Administration (0.143) and Technology (0.136), than for Marketing (0.439). Lemma 1b implies that Administration and Technology workers should see the lowest responsiveness of bonus to improvements in commute, just as we find in Table 6 (intuitively, their performance measures are not as responsive to effort, so they do not increase effort as much). Finally, Lemma 1c suggests a stronger impact on bonus for those workers for whom commute improvements better facilitates greater effort. While we do not have direct measures for how commute impacts performance for different categories of workers, our interviewees provided some clues. In particular, marketing personnel affected by Line 15 indicated that the subway improved their efficiency by allowing for better time management. They conjectured that relative to others, they are better able to utilize the short intervals of time on the subway to deal with many small but important tasks by exploiting good cell phone and internet signal coverage in the subway, like the commuters studied by Jain et al. (2017).[28] In addition, marketing personnel normally have irregular/variable working hours and also need to

[28] For example, marketing interviewees mentioned they can make and take client phone calls, send messages to clients, check and reply to emails, check inventories, check offer sheets (especially for small value deals), book tickets and reserve hotels.

deal with many emergencies, such as changing travel schedules, and answering urgent and/or unexpected questions from the clients. They can deal better with these issues on the less crowded Subway Line 15, than while driving or traveling on crowded and bumpy buses.[29] In contrast to marketing sales personnel, R&D staff usually need long stretches of time to undertake R&D development work; so utilizing commute time is less feasible for their jobs. Administration work, such as preparing or organizing meetings, and recruiting and interviewing job applicants, is normally conducted in the office. Besides, both R&D and administration work involve fewer emergencies and are much more predictable than marketing work. These anecdotes provide plausible reasons why the impact of commute quality on effort $\left(\frac{\partial \gamma}{\partial \delta} \text{ in our model}\right)$ is relatively higher for marketing workers.

5.4.2 Other Dimensions of Heterogeneity

We explore other sources of heterogeneity in Tables 7a and 7b.

(i) Heterogeneity by time saved by the subway (Table 7a, Panel A): Table 2 shows that affected employees, on average, can save about 21.765 minutes for one-way, which means about 43 minutes each day. Although all employees in the affected group are impacted by the opening of Line 15, the time they save from taking it varies. Prior work (e.g., Zhu et al., 2017, using large sample data of Chinese commuters) has found that subjective wellbeing is negatively related to the length of commute. To the extent that improvements in subjective wellbeing are triggering greater effort from the workers, we could expect the bonus increases to be correlated with time saved by Line 15. Indeed, Panel A in Table 7a shows that the more time saved by using Line 15, the larger the increase in bonus. The effects are stronger for non-managers and Marketing personnel, in line with the results in Table 6.[30]

(ii) By availability of telework-assisting paperless information technology (Panel B, Table 7a): Only if employees need to commute to the office should the opening of Subway Line

[29] These interviewees said that the reason people can work on Line 15, but not on buses is because subway travel is much more smooth/stable than shuttle buses. Shuttle buses have lots of braking and turning that makes working on phones and laptops difficult. Additionally, because Line 15 terminus is close to the company, it is generally not very crowded, so they can usually find seats on the commute home.

[30] We also check the robustness of these results to using self-reported measures of time saved (akin to the analysis in Section 5.3.E).The results are robust and, in fact, stronger, as for the *NearSubway* dummy, when using self-reported measures directly, or as an IV for the Baidu-based measure (Panel B of Online Appendix Table 2).

15 be expected to improve their performance. New software technology introduced for a subset of workers provides a unique opportunity to test this expectation. In particular, Company 1 started to use software that facilitated paperless transactions for employees working on project management (from March, 2015) and technical jobs (from November, 2015). This software allows these employees to conduct some of their work from home. Accordingly, we construct a dummy variable $ITPost_{it}$, which is equal to 1 for employees with access to the technology in month t, and 0 otherwise. The results show that the coefficient of the triple-interaction for the full sample is -0.059 and significant at the 10% level, which confirms that the ability to work from home reduces the benefits to worker from availability of better commute Subway Line 15. This benefit seems stronger for non-managers, but somewhat weaker (and noisier) for marketing personnel.[31]

(iii) <u>By demographics (Age/Children/Female) (Panels C, D and E of Table 7b)</u>: Table 7b shows heterogeneous effects by employee gender, presence of young (ages 0-12) children, and age. We find that compared with their counterparts, the effects of the new subway are not different for female employees (Panel C). There is a negative but noisy relative effect for workers with young children in the full sample in Panel D. Consistent with the finding that savings from the subway may not translate to work performance improvement for those with young (or possibly older) dependents, we find a non-linear effect for age (Panel E), which suggests that the positive effect on worker performance is the largest for younger and older workers, and lower for the middle-aged group (around age 33) workers. These pieces of evidence suggest that potential time/psychic savings from the quicker, more convenient commute may be diverted towards the home by middle-aged workers who may be parents of young children or may have older dependents, as mentioned in our interviews with firm employees.

6. Possible Channels for Observed Effects

As a simple extension, in the production function posited in our model for worker

[31] The weaker results for marketing personnel could be due to the small number of observations of affected marketing employees also impacted by telework-assisting paperless information technology.

output per period $q = \eta e$, the worker effort e could be considered as the product of an intensity of effort parameter (say μ) and the time devoted to work per period (say τ) by the worker, so $e = \mu \times \tau$ (as discussed informally under Assumption 1 in Section 3). In such an extended framework, the impact of improved commute on observed effort could be the result of increased intensity of effort (i.e., effort per unit of time) or from allocating some of the commute time savings to work.

In this section, first we use alternative sources of data to examine empirically if workers allocate time saved on commute towards extra time *at the work place*. We then summarize anecdotal evidence from our interviews with employees that shed light on the potential channels for improvement in worker performance and worker retention.

6.1 Analysis of Attendance and Late/Early Arrivals

To examine if the reduction in commute time translated into more time *at the work place*, we analyze two sources of data. First, we examine daily data collected by the companies on whether the worker arrived late, left early, or took a leave of absence from work. We aggregate the variables to the individual-month-level, and present results using a specification similar to Equation (4), in Appendix Table 4. We find no significant differential effects on affected workers in terms of late arrival, leaving early, or any of the absenteeism-related variables tracked by the companies.

While these results suggest that the subway opening did not change tardiness or extent of absenteeism, this data does not rule out that workers arrived earlier or stayed later than the cut-off time used to determine late arrivals and early departure. To check for this possibility, we also collated data on employees swiping in and out office floors and in and out of the building. Unfortunately, our close analysis of the data suggested significant measurement errors. We detected a significant proportion of odd numbered swipes per day by employees, suggesting that the system missed a number of employee entry and/or exit events, possibly because employees could enter or exit in groups, based on one individual swipe opening the entry door. Nevertheless, assuming the nature of the measurement error stays the same across affected and unaffected employees (or over time), a DID analysis should not be systematically biased. After cleaning the data, including eliminating single swipes and swipes very early in the morning or very late at night, and excluding weekends and holidays), we undertook three types of analyses.

First, for a simple descriptive look, we plotted the kernel density of time of arrival and time of leaving work, separately for the period before and after the opening of the subway (Online Appendix Figure 4). We find no systematic evidence of a shift in distribution such that workers begin arriving earlier, or leaving later after the subway opens; in fact, the estimates suggest a slight shift right (later arrivals) for time of arrivals, and a slight shift left (earlier departure) for time of departure. Second, we defined attendance time as the time between first and last swipes, and then examined a regression of log monthly average attendance time per workday using a specification similar to Equation (4). For this cleaned sub-sample of attendance time data, there is no significant effect of the subway on attendance time (Online Appendix Table 12), both overall and for non-manager and marketing sub-samples (to rule out sample composition effects, we check and confirm strong bonus effects in this sub-sample. Finally, we examined log of the time of arrival and log of the time of leaving work (defined as minutes from midnight) using a DID event study specification; consistent with the kernel density figures, we find very small (and statistically insignificant) point estimates for the effect of the subway (Online Appendix Table 13).

Taken together our results suggest there was no increase in time spent in the office by affected workers. These results are consistent with a number of anecdotes from our qualitative interviews, where employees insisted that strong monitoring and work norms enforced very high compliance with expected daily arrival and exit time cut-offs, for all workers, before and after the opening of the subway. So, they said, the availability of better commute did not significantly increase the amount of time they spent at work.

However, while we can rule out a role for extra time spent *at the work place*, our analysis does not rule out workers devoting some of the time saved on the commute to doing some work at home, or during the commute.

6.2 Qualitative Evidence from Interviews

Anecdotal evidence collected through two rounds of detailed discussions and interviews of both non-marketing (first round) and marketing personnel (second round) at the two companies yielded interesting insights that help explain some of our key findings in the context of our simple model, which we summarize below. (Other important insights from our interviews are highlighted elsewhere, e.g., see detailed discussion in Section 5.4.1 about noise in performance measurement.)

i. Confirmation that subway improves commute quality: The workers who self-identified as taking the subway confirmed that the subway improves their commute. Interestingly, a main factor they emphasized was predictability – they reported that commute times became more regular and predictable when using the subway – more than or as much as reduced travel time. Their responses are in line with prior research that finds that predictability is an important factor in commute-related stress (Evans, Wener, and Phillips, 2002; Sposato, Röderer, and Cervinka, 2012). Interviewees also highlighted that the proximity of the train station made their commute convenient too. Some noted that their commute home was especially pleasant because the proximate station was a terminus, hence the starting station for return commutes. So return commutes on Line 15 involved much less crowding and time taken for security clearance relative to the next best subway option, which required an additional walk of about 1.1 kms, and long security clearance delays. Respondents indicated time saved on commute ranged from 10 minutes to 60 minutes (consistent with our estimates, in Table 2).

ii. Confirmation that good commute provides non-pecuniary utility: Through hypothetical questions posed to the interviewees, we confirmed that a superior commute is a valued job characteristic, with workers willing to give up a job that involved navigating bad traffic. Non-marketing respondents suggested the better commute improved their life satisfaction (though not work performance (see point iii below)). This is also in line with prior research showing correlations between shorter commutes (e.g., Zhu et al., 2017) and commutes by rail (e.g., Meyer and Dauby, 2002) with greater subjective wellbeing. Our employee survey asked about current work-life balance, and the workers' recollection of their work-life balance levels in the period prior to the subway opening. Analysis of this data did not yield statistically significant effects of the subway for affected workers although estimates were positive for the full sample and non-managers (Online Appendix Table 15). We believe this is likely because of significant limitations of recall data on happiness as documented by Kahneman (2011) and Ratner, Kahn, and Kahneman (1999). In addition, research shows that satisfaction levels revert to a stable set point over time (the "hedonic treadmill" effect) except for changes from major life events for some subsets of the population (e.g., Brickman, Coates and Janoff-Bulman 1978, Fujita and Diener 2005).

iii. How workers allocate commute time saved by the subway: As discussed in Section 6.1, many of our affected respondents indicated they did not spend extra time at work, instead used saved time for household related chores/errands, taking care of parents/children, exercise, relaxation or sleep.[32] As discussed in detail in Section 5.4.1, marketing interviewees indicated several ways that they were able to actual accomplish work during their improved commute on Subway Line 15. Interestingly even unaffected workers, in answer to a question about hypothetical benefits from taking the subway, conjectured it would improve time management, physical condition, and mood for affected workers.

7. Impact on Employee Exit and Hiring

7.1. Impact on Employee Exit Rate

Our model (Prediction 2) implies that the opening of Subway Line 15 would lower probability of affected employees exiting the firms, because the improved commute is likely to increase both the non-pecuniary as well as wage benefits from the job.[33] We investigate this prediction in this section. We define a dummy $Exit_t$, which is equal to one in period t if the employee is not in the company in month $t+1$.

Figure 4 presents the quarterly average of the monthly ratio of employees exiting jobs from the affected and unaffected groups of workers. Before the opening of Line 15, the ratios of employees exiting jobs are similar for the groups of the (to be) affected and unaffected

[32] As recent research suggests a link between better sleep and labor productivity (Gibson and Shrader 2018), and between exercise and worker productivity (summarized in Lechner, 2015), perhaps better sleep and exercise provide potential pathways for our observed results.

[33] We refrain from framing the exits in the data as worker "quits", because an observed exit could also result from firms firing the worker. In our model, as in standard Coasian wage bargaining models, the distinction is innocuous -- the worker separates (i.e., exits) when the outside utility draw is above the maximum possible utility in the firm. While this exit could be a voluntary quit by the worker, it could also technically be a result of a firing by the firm on realizing that it cannot match the outside offer.

employees. In contrast, after the opening, the ratio of employees exiting is higher for the unaffected group relative to the affected group, suggesting that the subway opening decreases the probability of exit for affected employees, as predicted by our model. Figure 4 nets out the mean of a sharp increase of the exit rates for both affected and unaffected employees around November 2015 (evident in the raw monthly exit rate trends shown in Appendix Figure 2). Our interviewees explained that this spike was triggered by a CEO change for the parent company of the two companies under study.

To control for individual-level fixed and time varying characteristics, in Table 8 we present results from proportional hazards models, where the hazard function takes the form $h_i(t) = h_0(t)\exp(\beta_1.NearSubway_i + \beta_2.T_t + \beta_3.NearSubway_i \times T_t + \gamma \boldsymbol{X}_{it})$. $NearSubway_i$ is a dummy for affected workers, and T_t is a dummy for the period post opening, both as used in Section 5.1, and X_{it} is a vector of controls including Age and Experience (time varying), and Male dummy, Education, Number of Children, Manager dummy, Distance, Company 1 dummy, Married dummy, and New Employee dummy (defined as at the start of the worker spell within the sample period). We define the duration t as the amount of time the individual is at risk of exiting from the firm in two alternative ways. First, in Columns 1 and 2, we define duration as the time between the first and last observation of the individual within our sample period. Alternatively, in Columns 3 and 4, we adjust for prior tenure of workers starting in the beginning of the sample (using information available in the administrative data) and set duration as the tenure at time of exit. We use two alternative specifications of the proportional hazards model: in Columns 1 and 3 we use the semi-parametric Cox model, and in Columns 2 and 4 we check robustness to using a parametric (exponential) proportional hazards specification.

The coefficient of interest is β_3, which represents a DID estimate, indicating the change of hazard for the affected group relative to non-affected in the post-subway period compared to the pre-subway period. The estimates indicate that the relative hazard for the NearSubway group was not significantly different (and in fact slightly higher) in the pre-subway period (positive β_1), but is significantly lower (negative and significant β_3 coefficient) across all four specifications. The estimate from Column 2 implies a decline in hazard of exit of 49.9% (1-exp(-0.691)), while Column 4 implies a slightly larger effect of 50.98% (1-exp(-0.713)).

The survival analysis results, as well as Figure 4 and Appendix Figure 2, confirm that affected workers display a lower propensity to exit the company after the opening of Line 15, in line with the Prediction 2 of our model.[34]

7.2. Impact on Hiring

In this section, we investigate whether the companies are able to attract higher quality hires after the subway station opened. Prediction 3 of our model captures the intuitive expectation that the non-pecuniary utility from the opening of the subway should allow the companies to attract better employees from locations close to the subway, for the same salary offers as made to non-affected applicants.

The performance scores available from one company in 2015 and 2016 (used in Appendix Table 1a) allow us to assess the quality of new hires. In particular, using the sample of employees hired after the opening of the subway, we regress performance scores (in 2015 and 2016) on *NearSubway* after controlling for different forms of base salary and year-month fixed effects. As expected, we find that given the same level of compensation, employees affected by the subway, hired after the subway opening have, on average, significantly higher performance scores (Columns 1-3, in Appendix Table 5). As a comparison, we conduct the same cross-sectional regression of the performance score in the 2015-16 period, for the sample of employees newly hired before 2015; while this is not an ideal placebo check, if workers place positive probability on exiting the firm over the short run, we should expect relatively weaker effects for these hires. Indeed, we find that the coefficients of *NearSubway* are smaller and statistically insignificant (Columns 4-6, in Appendix Table 5) for workers from affected

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[34] The coefficients on controls (suppressed for brevity) were in line with expectations. In particular, we find exit hazard is consistently higher for males, lower for individuals with more children, and higher for new employees. Interestingly, exit hazard is strongly higher with distance, consistent with the baseline finding that reduction of commute reduces exit hazard, and consistent with the evidence discussed in Manning (2003). Our checks across different occupations showed similar negative effects suggesting that even groups with lower bonus effects experienced non-pecuniary gains leading to reductions in exit propensity. To address the potential effects of CEO turnover we observe in Appendix Figure 2, we checked robustness to adding controls for *CEOTurnover* and *NearSubway* × *CEOTurnover*, where *CEOTurnover* is a dummy indicator variable for last quarter of 2015 (Oct, Nov, and Dec 2015) – the coefficients on *NearSubway* ×*Post* were only slightly lower.

locations hired before the subway opening.

While these cross-sectional regressions must be interpreted with caution, the results are consistent with the prediction that the companies are able to hire higher quality workers from the affected applicant pool after the subway opened.

8. Firm-level Aggregate Effects: Impact of Subways on Shareholder Value

The above results show that opening a subway station near a company can significantly improve employee performance, reduce turnover rate, and enhance quality of hires. A natural question that follows is whether the subway increases overall firm profits (or market value).

Because data from only two affected companies does not provide statistical power to conclusively answer how aggregate firm profits were affected, we undertake supplementary data collation on a large sample of subway construction start dates and a related large sample of affected and unaffected companies. In addition to being of independent interest, understanding the impact on firm value for a large sample of firms could also be informative about the broader validity of our early results, assuming that the main driver for firm value is improved worker performance.

Specifically, to keep the study close to the scope of our baseline analysis in terms of both geography and time period, we focus on the effects of 48 subway construction start dates in a window from 2005 to 2017 in Beijing -- a period of massive investment in subway construction in Beijing (see Section 2). We collect stock price for the sample of all publicly listed A share firms during 2005 to 2017, headquartered in Beijing. We obtain precise headquarter location address information from the RESSET database. We use the Baidu Map interface to identify if each company has a subway station "nearby". To maintain close comparison to the proximity of the subway station in our baseline analysis, we define "nearby" stringently, as within 300 meters of the headquarters location, which translates to about a five minutes walking distance. That is, we define a new subway station opening as "affecting" all firms for which that station is "nearby".

We use the specific subway line construction commencement dates to conduct stock price event studies for each affected firm. We use construction commencement dates, instead of station opening dates here because the stock market is likely to adjust much before the predictable opening date, whereas the construction start date is likely to be less predictable and hence reveal information about the probability of, and time to, opening of the subway station to investors. Under the assumption that financial markets are efficient, the returns around the subway construction commencement dates should reflect the value that the nearby subway can bring to the firm (or more precisely the additional expected value from the increased expectation of completion and potentially earlier expected completion date of the subway, implied by the commencement of construction).

We use daily stock return data to estimate the abnormal returns around construction commencement. To account for firm size and value effects, we estimate the Fama-French Three-Factor Model. The risk-free rate is measured as the three-month bank deposit interest rate in China. Market index is measured as market value-weighted returns of all China A-share stocks. We regress the daily stock returns (including dividends, adjusted by the risk-free rate of returns) on the market index returns (adjusted by the risk-free return rate), the difference between small-firm returns and big-firm returns, and the difference between high and low book-to-market equity firm return during the estimation window (defined as 120 days to 30 days before the event days). We use the estimated coefficients to predict the stock returns during the event window to derive the abnormal returns. We calculate the summation of the abnormal returns during the event window, as the excess returns from nearby subway line construction commencement for the affected firm. Our event study sample includes 37 firms, identified with "nearby" subway stations built during the sample period, which also have necessary stock return data available to estimate the construction event returns. Some of these firms have multiple nearby subway stations. The 48 events examined in this analysis are associated with the construction of stations on 5 subway lines.

Because there might be some information leakage before the event dates and lag of information digestion after the event dates, we employ two symmetric event windows covering from 5 days (± 2 days including event) to 11 days (± 5 days including event). The results reported in Table 9 show that the abnormal returns over both event windows are positive and significant at the 5% and 10% levels, respectively. Based on the average market capitalization of seven billion RMB (deflated to year 2000), the magnitude of the event study abnormal returns (about 2%) suggests that initiation of construction of a new subway station near the company increases firm value by about 0.14 Billion RMB.[35] Because the market price likely already reflected a general expectation of the arrival of the new subway (based on broader plans that may have been announced earlier by the government), this estimate is likely a lower bound of the total value to shareholders from a nearby subway, reflecting only the additional value from reduced uncertainty about the construction schedule.

9. Conclusions

Using "insider" company administrative and survey data, we explore how employees responded to the availability of improved public commute, through the opening of a subway station close to their workplace. We posit that a better commute enhances worker effort (through improved wellbeing and/or from freeing up commute time) and non-pecuniary utility from the job. The subway opening is therefore expected to increase worker bonus pay, and reduce propensity to exit the firm (and enhance quality of hires from subway-accessible locations). We find that these expectations are borne out in the data, for affected workers (defined as those for whom the new Subway Line 15 provides the fastest public transport commute route from home) relative to others. We find heterogeneity in bonus effects, with effects larger for worker groups with more sensitivity of performance measurement (per coefficient of variation in the data, and also per anecdotal evidence). We find the bonus improvements are correlated with time saved, and lower for workers with a greater ability to telecommute. The baseline results are robust to a number of checks.

A careful exploration of data on attendance reveals that there was no allocation of time saved from commute towards time at the workplace. Our interviewees provide some anecdotal evidence suggesting that marketing personnel were able to use the time on the improved (less crowded) commute to accomplish some work tasks during the commute. Thus, it appears that a main pathway for the observed improvement in bonus and increased retention is through an

[35] Because the event study returns based on (-2,+2) window and (-5,+5) window are 0.017 and 0.019, we calculate the value implication from event study returns based on an estimate of 0.02.

improvement in wellbeing, which in turn prompted greater worker effort (Porto 2016).

Our study is limited in its focus on relative performance, retention, and hiring effects on affected workers, and an examination of shareholder value as captured by the stock market reaction to subway construction start dates. While our setting allows for a sharper and plausibly causal estimate of the relative effect on affected workers, our estimated positive inter-employee spillover effects and the larger effects we get from excluding a competing subway line suggest that the estimated differential effect may understate the aggregate employee performance effects of the new subway line. Our estimate of benefit to shareholders is also likely an underestimate, as it reflects only the capitalized value of a change in expectation about the timing of arrival of the subway. Other unmeasured benefits include potential reduction in air pollution from reduced road traffic congestion (Anderson 2014), and unmeasured increases in worker utility from increased leisure time and/or psychic utility from better commutes. While we do not find much relocation by workers over the short horizon (2 years prior to and after subway opening), over a longer term additional benefits from the subway would include the facilitation of relocation of employee homes to high amenity locations, which is an important welfare channel highlighted by Monte et al. (2018).

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Theory Appendix

Proof of Prediction 1: This follows directly from A1, as: $e^* = \frac{\beta^*\eta}{\gamma} = \frac{\eta}{\gamma}$, and

Bonus$^* = \beta^*\eta e^* = \frac{\eta^2}{\gamma}$, so A1 implies $\frac{\partial e^*}{\partial\delta} = -\frac{\eta}{\gamma^2}\frac{\partial\gamma}{\partial\delta} > 0$, $\frac{\partial \text{Bonus}^*}{\partial\delta} = -\frac{\eta^2}{\gamma^2}\frac{\partial\gamma}{\partial\delta} > 0$. □

Proof of Lemma 1a: $\frac{\partial e^*}{\partial\delta} = -\frac{\eta}{\gamma^2}\frac{\partial\gamma}{\partial\delta}$ and $\frac{\partial \text{Bonus}^*}{\partial\delta} = -\frac{\eta^2}{\gamma^2}\frac{\partial\gamma}{\partial\delta}$ implies responsiveness of effort and bonus income to improvements in commute is increasing in η. The share of bonus in total income $\left(\frac{\beta^*\eta e^*}{\bar{U}-h+\frac{\eta^2}{2\gamma}} = \frac{\eta^2}{\gamma}\times\frac{1}{\left(\bar{U}-h+\frac{\eta^2}{2\gamma}\right)}\right)$, so if $\bar{U}, h,$ and γ are similar across workers, then a higher bonus share indicates bigger η. □

Proof of Lemma 1b: Variance of performance measure $= Var(\eta e^*) = \eta^4 Var\left(\frac{1}{\gamma_i}\right)$. Thus if γ_i is distributed similarly across categories of workers, higher variance of performance measure will be indicative of bigger η.

Proof of Lemma 1c: $\frac{\partial e^*}{\partial\delta} = -\frac{\eta}{\gamma^2}\frac{\partial\gamma}{\partial\delta}$ and $\frac{\partial \text{Bonus}^*}{\partial\delta} = -\frac{\eta^2}{\gamma^2}\frac{\partial\gamma}{\partial\delta}$ so greater the magnitude of $\frac{\partial\gamma}{\partial\delta}$, the greater is the impact of commute improvement on worker effort and bonus income. □

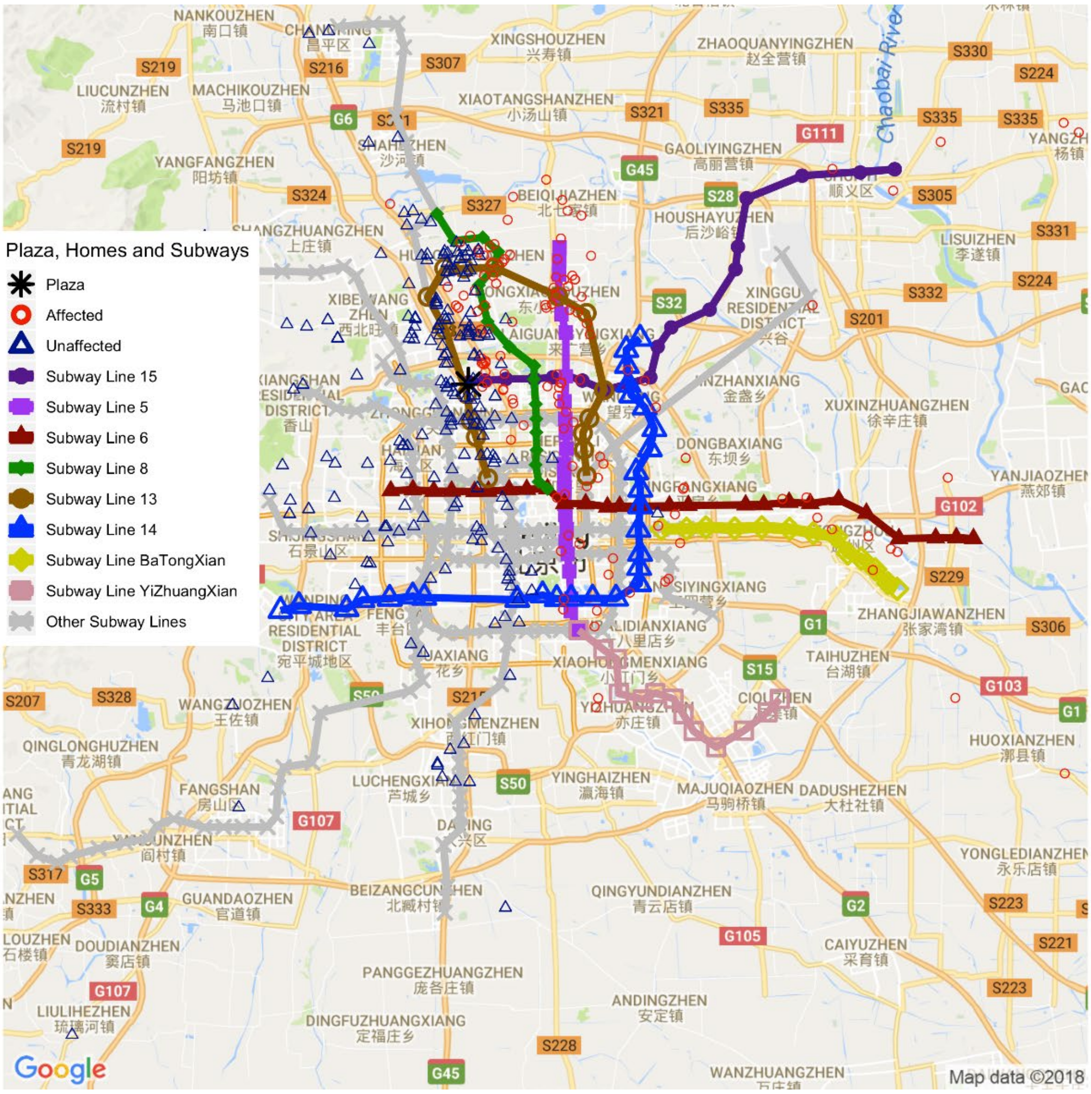
Proof of Corollary 1a: Given A1 and A2, (3) implies, $\frac{\partial\alpha^*}{\partial\delta} = -\frac{\partial h}{\partial\delta} + \frac{\eta^2}{2\gamma^2}\frac{\partial\gamma}{\partial\delta} < 0$. □

Proof of Corollary 1b: The total optimal wage $w^* = \alpha^* + \beta^*\eta e^* = (\bar{U}-h) - \frac{\eta^2}{2\gamma} + \frac{\eta^2}{\gamma} = (\bar{U}-h) + \frac{\eta^2}{2\gamma}$. Therefore $\frac{\partial w^*}{\partial\delta} = -\frac{\partial h}{\partial\delta} - \frac{\eta^2}{2\gamma^2}\frac{\partial\gamma}{\partial\delta} > 0 \leftrightarrow \frac{\partial h}{\partial\delta} < -\frac{\eta^2}{2\gamma^2}\frac{\partial\gamma}{\partial\delta}$. □

Proof of Prediction 2: $\text{P[Exit]} = \text{P}[\bar{U} > (\text{Max Firm\_Worker match Value}) + \text{h}] = \text{P}\left[\bar{U} > \frac{\eta^2}{2\gamma} + h\right] = 1 - F\left(\frac{\eta^2}{2\gamma} + h\right)$. Because cdf $F(x)$ is strictly increasing in x (given our assumption of strictly positive support), the result follows directly from A1 and/or A2, as we get: $\frac{\partial \text{P[Exit]}}{\partial\delta} = -\left(\frac{\partial \text{h}}{\partial\delta} - \frac{\eta^2}{2\gamma^2}\frac{\partial\gamma}{\partial\delta}\right)\text{F}'\left(\text{h} + \frac{\eta^2}{2\gamma}\right) < 0$. (Alternatively, the firm will retain the worker only if profits from keeping the worker $\Pi^* \geq 0$, so that $P[\text{Exit}] = P[\Pi^* < 0] = P\left[\frac{\eta^2}{2\gamma} - (\bar{U}-h) < 0\right] = \text{P}\left[\bar{U} > \frac{\eta^2}{2\gamma} + h\right] = 1 - F\left(\frac{\eta^2}{2\gamma} + h\right)$. The result follows as above.) □

Proof of Prediction 3: If worker 1 has better commute $(h_1 > h_2)$ and $\bar{U}_1 = \bar{U}_2$, $w_1 = w_2 \Rightarrow (\bar{U}_1 - h_1) + \frac{\eta^2}{2\gamma_1} = (\bar{U}_2 - h_2) + \frac{\eta^2}{2\gamma_2} \Rightarrow \frac{1}{2\gamma_1} - \frac{1}{2\gamma_2} > 0 \Rightarrow \gamma_1 < \gamma_2 \Rightarrow e_1^* = \frac{\eta}{\gamma_1} > e_2^* = \frac{\eta}{\gamma_2}$. □

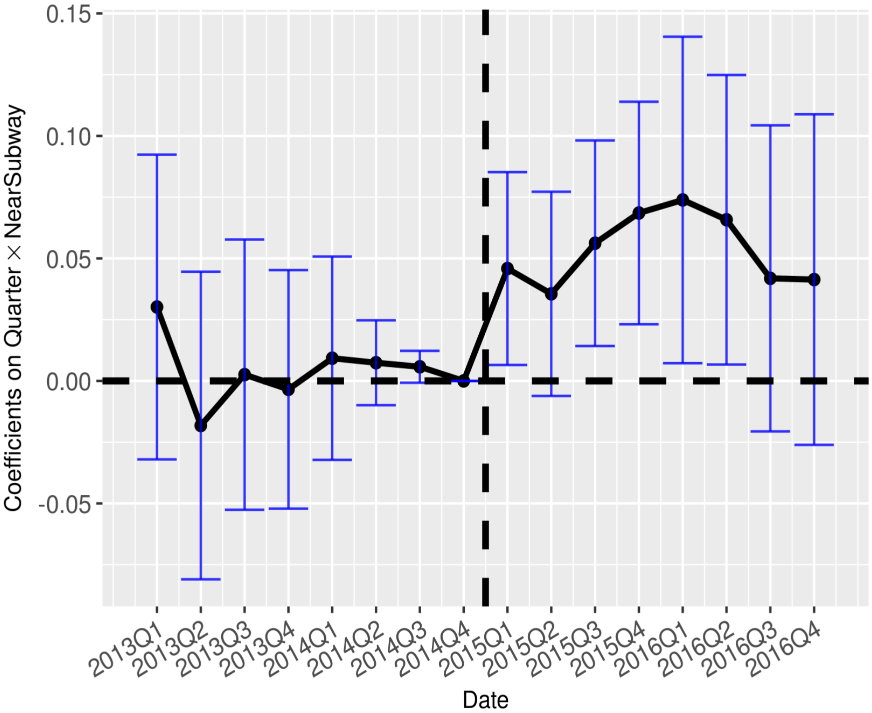
Figure 1: Subway Line 15 and Connected Lines



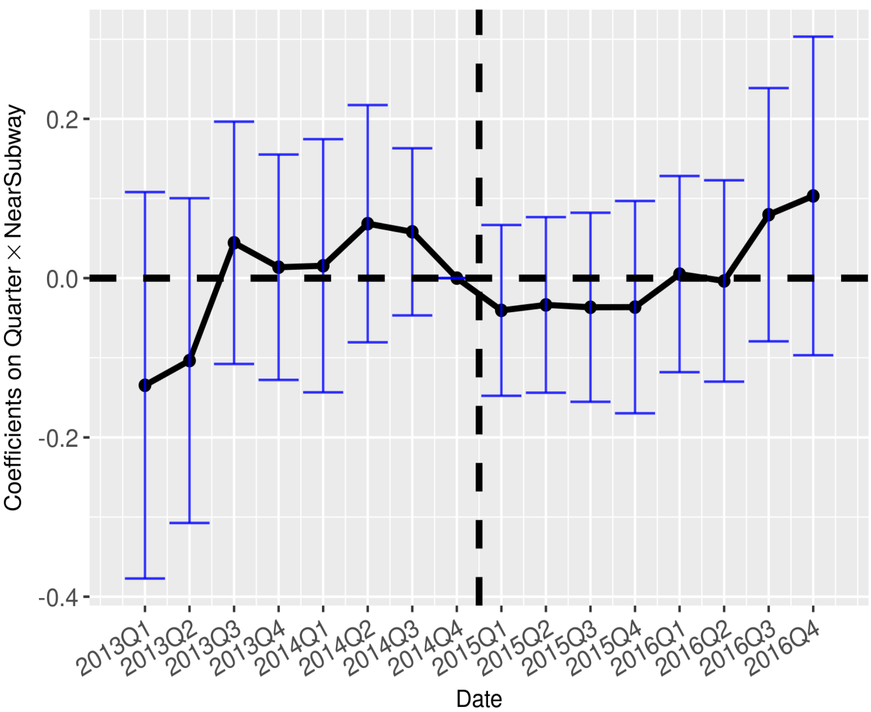
Notes: This figure highlights Subway Line 15 and the subways lines connected to Subway Line 15 as colored lines. Note that one outlier employee located near the intersection of Line 6 and Line 14 belongs to the unaffected group but all other employees nearby belong to affected group. This outlier employee happened to live close to a bus station of Special Route 9 (Te Jiu Lu), which provides shorter commute time than Subway Line 15.

Figure 2: Trends in Differentials for Income Variables

Panel A: Log (Bonus^{T})

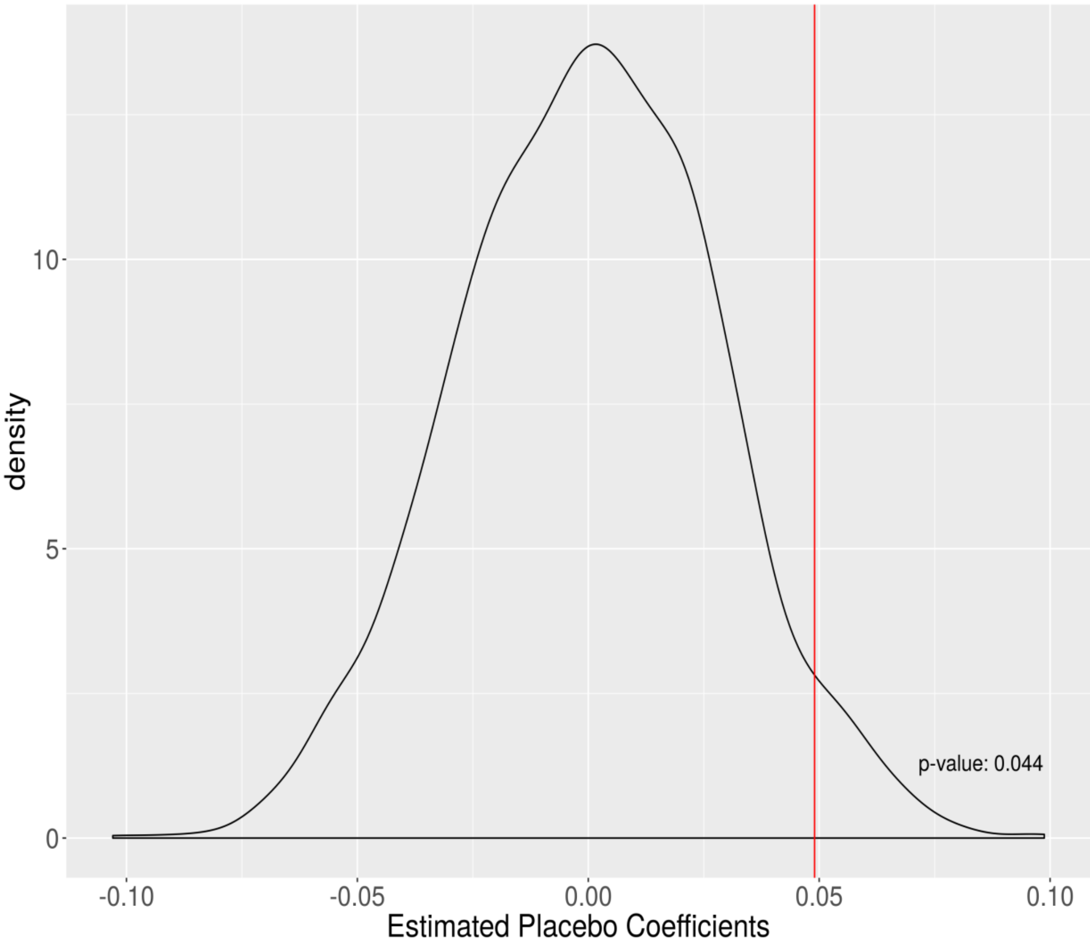


Panel B: Log (Total Income Excluding Bonus)



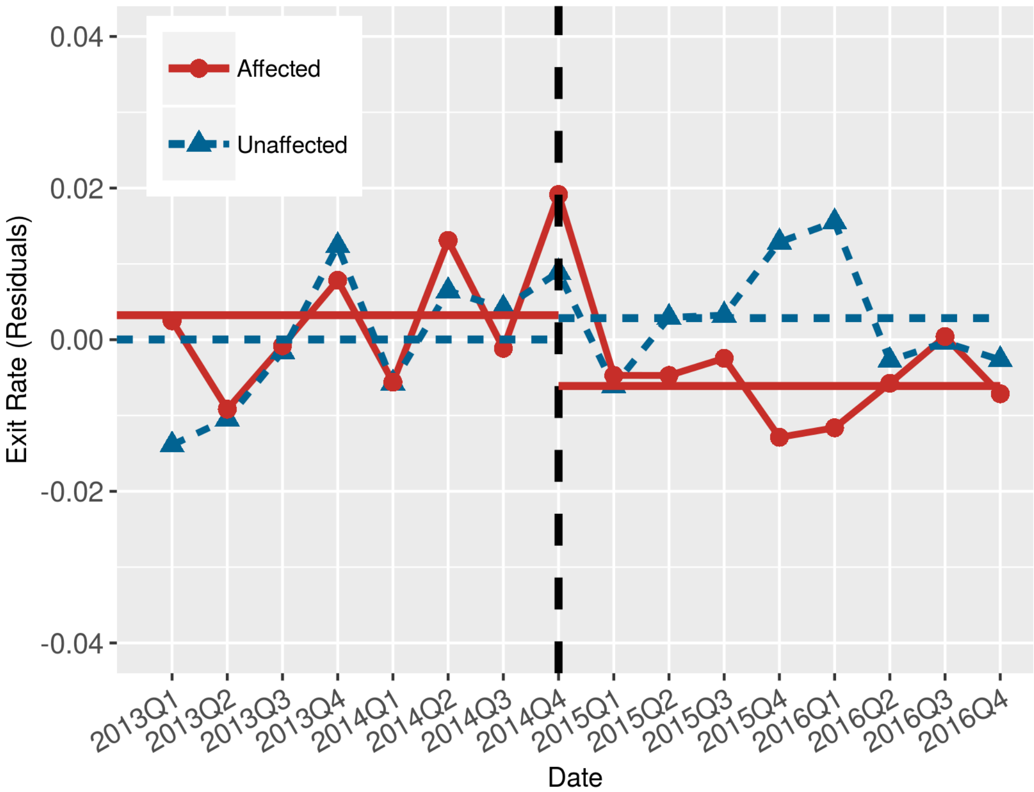
Notes: Each point is the coefficient on a quarter dummy interacted with NearSubway, which captures the difference in the Log(Bonus^{T}) in the specific year-quarter between the affected workers compared to the control group. The reference quarter is the fourth quarter of 2014 (hence normalized to zero). The error bar shows the 95% confidence interval, based on two-way (at individual and year-month level) clustered standard errors. The vertical dashed line indicates the opening of Subway Line 15.

Figure 3: Permutation Test for Log($Bonus^T$)



Notes: We randomly assign NearSubway without replacement, then estimate the regression shown in Column (4) in Table 3. We repeat the process 2,000 times. The figure shows the distribution of the coefficients of NearSubway × Post from these 2,000 regressions. The vertical line represents the coefficient of NearSubway × Post in Column (4) in Table 3, which is 0.049. The value shown in the figure is a one-sided p-value.

Figure 4: Trends of Employee Exit Rates



Notes: The sample is from February, 2013 to November, 2016. The exit rate is calculated as the number of employees exiting divided by the average number of employees in each month for affected and unaffected groups, respectively. Employee exit in month t if month t is the last month for this employee to be in the company. The average number of employees in month t is equal to the mean of the number of employees at month t-1 and month t+1. We average the exit rate to the quarterly level, then regress the exit rate on a dummy denoting 2015Q4 to control for the CEO turnover in that quarter. The quarterly means of the residuals (by affected and unaffected groups) from this regression are plotted in the figure. The vertical dashed line indicates the opening of Subway Line 15. Horizontal lines indicate the means of the exit rate for affected and control over all quarters before (after) the opening of the Subway 15 if it is to the left (right) of the vertical dashed line.

Table 1: Sample Distribution (for Compensation Analysis)

Variable	(1)	(2)	(3)	(4)
	Full Sample		Non-managers Only	
	Employee Number	Observations	Employee Number	Observations
		Panel A: Both Companies		
Total	377	14807	289	11166
Affected	144	5763	112	4392
Unaffected	233	9044	177	6774
Share of Affected	0.382	0.389	0.388	0.393
		Panel B: Company 1		
Total	265	10398	211	8031
Affected	105	4242	84	3242
Unaffected	160	6156	127	4789
Share of Affected	0.396	0.408	0.398	0.404
		Panel C: Company 2		
Total	112	4409	78	3135
Affected	39	1521	28	1150
Unaffected	73	2888	50	1985
Share of Affected	0.348	0.345	0.359	0.367

Notes: This table presents sample characteristics of our main sample. We eliminate those employees who moved after the subway station opened, and we keep only those employees who are in the company workforce at least one month before and after the opening of the subway. Columns (1) and (2) cover the full sample, and Columns (3) and (4) exclude managers. Employees belong to the `affected' group if and only if the fastest public transport route contains Subway Line 15.

Table 2: Summary Statistics

Variable	(1) Obs.	(2) Mean	(3) Std. Dev.	(4) Median
Age (year)	14,807	34.656	7.518	34.000
Male Dummy	14,807	0.528	0.499	1.000
Experience (week)	14,807	626.521	423.536	578.143
Tenure (week)	14,807	360.295	280.544	298.714
Number of children	14,807	0.385	0.528	0.000
Education (year)	14,807	16.229	1.560	16.000
Party Membership Dummy	14,807	0.161	0.367	0.000
Hukou (Non-Agricultural = 1)	14,807	0.895	0.306	1.000
Married Dummy	14,783	0.741	0.438	1.000
Manager Dummy	14653	0.238	0.426	0.000
NearSubway Dummy	14,685	0.389	0.488	0.000
TimeSaved (one-way, in minutes, affected workers only)	5,689	21.765	24.884	12.667
Distance (kilometer)	14,807	10.498	8.732	8.651
Bonus (RMB)	14,807	4944.790	5525.948	3305.469
Total Income Net of Bonus (RMB)	14,807	6840.306	4808.899	5500.000
Total Income (RMB)	14,807	11785.100	7863.572	9488.189
Log ($Bonus^T$)	14,807	9.296	0.390	9.231
Log(Total Income Net of Bonus)	14,807	8.552	0.958	8.613
Log (Total $Income^T$)	14,807	9.767	0.352	9.705

Notes: This table reports the summary statistics of the main variables. Education, Party Membership, Hukou, and Marriage Dummy are as on last observed date for the employee. NearSubway, TimeSaved, TimeSaved Percent, and Distance, are defined at the start of the worker panel period (address is updated with information from the July, 2017 employee survey, where available). The Manager dummy is defined based on employee status in November, 2014 (month prior to opening of Line 15). The remaining variables vary across the months. Bonus and income variables are deflated by CPI using 2013 as the base year. To retain information on negative and zero bonus income in logs, the $Bonus^T$ (and for consistency Total $Income^T$) are a simple linear transformation of bonus and total income by adding a constant (6905.22) such that the minimum value of the adjusted bonus is equal to one.

Table 3: Impact of the Opening of Subway Line 15 on Employee Compensation

	(1)	(2)	(3)	(4)	(5)	(6)
	Bonus	Total Income Net of Bonus	Total Income	Log ($Bonus^T$)	Log(Total Income Net of Bonus)	Log (Total $Income^T$)
NearSubway × Post	**636.328***	**-69.159**	**567.169**	**0.049***	**-0.005**	**0.029***
	(376.378)	**(396.401)**	**(387.618)**	**(0.027)**	**(0.056)**	**(0.016)**
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Company-Year-Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.674	0.777	0.825	0.722	0.562	0.848
Obs	14,807	14,807	14,807	14,807	14,807	14,807
N (affected group)	144	144	144	144	144	144
N (unaffected group)	233	233	233	233	233	233
Mean of dep. var.	4944.790	6840.306	11785.100	9.296	8.552	9.767
Std. dev. of dep. var.	5525.948	4808.899	7863.572	0.390	0.958	0.352
Change on level variable	636.328	-69.159	567.169	580.650	-34.202	542.019
Effect as % of mean bonus level	12.87%	-1.01%	4.81%	11.74%	-0.50%	4.60%
Effect as % of SD of Dep Var	**11.52%**	**-1.44%**	**7.21%**	**12.56%**	**-0.52%**	**8.23%**

Notes: See Section 4.3 for definitions of the dependent variables. NearSubway is a dummy variable equal to one if the fastest public transport route from the worker's home address to office contains Subway Line 15, otherwise zero. Two-way (at individual and year-month level) clustered standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Table 4: Propensity Model and Improvement of Balance in Matched Sample

	(1)	(2)	(4)	(5)	(6)		(7)	(8)	(9)	(10)	(11)
	Mean Affected	Mean Unaffected	z stat	p-value	Propensity Model: NearSubway Coeff	(Std Err)	Mean Affected	Mean Unaffected	z stat	p-value	% balance improved
	PANEL A: Unmatched sample						PANEL B: Propensity matched sample				
Age	30.645	33.008	-3.00	0.003	0.069	(0.061)	30.660	30.547	0.14	0.889	95.384
Male	0.493	0.584	-1.83	0.067	-0.371	(0.232)	0.489	0.518	-0.48	0.634	70.932
Experience	410.570	550.445	-3.24	0.001	-0.002**	(0.001)	412.058	408.878	0.07	0.942	97.816
Tenure	210.914	263.693	-1.82	0.069	0.000	(0.001)	215.216	214.620	0.02	0.984	98.853
Education	16.069	16.309	-1.33	0.184	-0.160*	(0.088)	16.071	16.078	-0.04	0.966	96.717
Number of Children	0.410	0.455	-0.86	0.390	0.206	(0.262)	0.411	0.397	0.22	0.827	71.357
Manager Dummy	0.218	0.309	-1.86	0.063	0.008	(0.315)	0.220	0.220	0.00	1.000	100.000
Distance	11.904	10.155	1.85	0.064	0.020	(0.013)	11.987	11.633	0.31	0.757	80.155
Company 1 Dummy	0.729	0.687	0.98	0.329	0.061	(0.260)	0.730	0.745	-0.27	0.787	70.380
Married Dummy	0.650	0.721	-1.36	0.174	-0.072	(0.324)	0.660	0.631	0.50	0.619	57.348
New Employee	0.333	0.296	0.71	0.478	-0.144	(0.297)	0.326	0.333	-0.13	0.899	79.700
Constant					0.954	(1.555)					
								Overall test	Chi-square	p-value	
Log Likelihood					-235.621			Unmatched	20.10	0.043	
Obs					371			Matched	0.71	1.000	

Notes: This table shows the estimates from propensity model (in Column 1), and the change in balance (difference in means between affected and control group) after the nearest neighbor propensity score matching (in Columns 2-6). The dependent variable in Column 1 is the NearSubway dummy, which is equal to one if the fastest public transport route from the worker's home address to office contains Subway Line 15, otherwise zero. We pick the initial value when each employee enters the sample, so this regression uses a cross-sectional worker-level sample. New Employee is a dummy equal to one for workers entering the firm after January, 2013. Standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively. The overall test undertakes a joint test of balance of all independent variables in both unmatched and matched samples.

Table 5: Impact Using Propensity-Score Matched Sample

	(1)	(2)
	Bonus	Log (Bonus^T)
NearSubway × Post	**693.642***	**0.050***
	(409.905)	**(0.030)**
Individual fixed effects	Yes	Yes
Company-Year-Month fixed effects	Yes	Yes
Adj. R-Squared	0.689	0.723
Obs	11,086	11,086
N (affected group)	141	141
N (control group)	141	141
Mean of dep. var.	4,668.134	9.278
Std. dev. of dep. var.	5,244.803	0.377
Mean of NearSubway	0.510	0.510
Std. dev. of NearSubway	0.500	0.500
Change on level variable	693.642	578.668
Effect as % of SD of Dep Var	**13.22%**	**13.26%**

Notes: The sample consists of affected workers and their nearest propensity-score matched neighbor, per procedure in Ho et al. (2011).Two-way (at individual and year-month level) clustered standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Table 6: Heterogeneity of Bonus Impact by Position/Occupation

	(1)	(2)	(3)	(4)	(5)	(6)
	By position:		By occupation:			Intersection:
	Non-Managers	Managers	Administration	Technology	Marketing	Non-Managers & Marketing
NearSubway × Post	0.051**	0.066	0.056	-0.035	0.094***	0.088***
	(0.025)	(0.081)	(0.069)	(0.040)	(0.033)	(0.032)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Company-Year-Month fixed	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.683	0.724	0.779	0.764	0.721	0.695
Obs	11,166	3,487	3,289	4,663	6,701	5,367
N (affected group)	112	29	31	54	56	44
N (unaffected group)	177	51	47	72	109	88
Mean of dep. var.	9.228	9.519	9.344	9.303	9.271	9.232
Std. Dev. of dep. var.	0.329	0.478	0.449	0.370	0.369	0.324
Mean of NearSubway	0.393	0.376	0.401	0.437	0.350	0.343
Std. dev. of NearSubway	0.489	0.484	0.490	0.496	0.477	0.475
Mean Bonus in Sample	3894.903	8363.817	5885.383	4891.390	4549.999	3927.295
Implied Change in Bonus Level	550.806	1007.756	716.274	-412.881	1076.791	953.261
Effect as % of SD of Dep Var	**15.50%**	**13.81%**	**12.47%**	**-9.46%**	**25.47%**	**27.16%**

Notes: See Section 5.4.1 for a discussion of positions and occupations. The dependent variable is Log($Bonus^T$). Two-way (at individual and year-month level) clustered standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Table 7a: Heterogeneity of Bonus Impact by Time Saved and Access to Paperless IT

	(1)	(2)	(3)
	Dependent variable: Log($Bonus^T$)		
	Full Sample	Non-Managers	Marketing
		Panel A: TimeSaved	
Log(TimeSaved + 1) × Post	0.007*	0.008**	0.014***
	(0.004)	(0.004)	(0.005)
Adj. R-Squared	0.721	0.683	0.722
Obs	14,733	11,140	6,701
N (affected group)	142	111	56
N (unaffected group)	233	177	109
Mean of dep. var.	9.296	9.229	9.271
Std. dev. of dep. var.	0.389	0.328	0.369
Mean of Log(TimeSaved + 1)	2.516	2.577	2.324
Std. dev. of Log(TimeSaved + 1)	3.281	3.307	3.243
		Panel B: Paperless IT	
NearSubway × Post	0.062**	0.069***	0.101***
	(0.028)	(0.025)	(0.036)
NearSubway × ITPost	-0.059*	-0.065**	-0.054
	(0.035)	(0.033)	(0.041)
P-value of the joint F-test	0.932	0.916	0.163
Adj. R-Squared	0.727	0.688	0.722
Obs	14,807	11,166	6,701
N (affected group)	144	112	56
N (unaffected group)	233	117	109
Mean of dep. var.	9.296	9.228	9.271
Std. dev. of dep. var.	0.390	0.329	0.369
Mean of NearSubway	0.389	0.393	0.350
Std. dev. of NearSubway	0.488	0.489	0.477
Mean of ITPost	0.104	0.121	0.066
Std. dev. of ITPost	0.305	0.326	0.249
Individual fixed effects	Yes	Yes	Yes
Company-Year-Month fixed effects	Yes	Yes	Yes

Notes: ITPost is a dummy variable with one for workers with access in that period to paperless IT software that facilitated telework. ITPost by itself was included in panel B regressions, and had positive coefficients in the full sample and for non-managers, suggesting paperless IT helped improve performance of workers. Two-way (at individual and year-month level) clustered standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Table 7b: Heterogeneity of Bonus Impact by Demographics

	(1)	(2)	(3)
	Dependent variable: Log($Bonus^T$)		
	Full Sample	Non-Managers	Marketing
	Panel C: Female		
NearSubway × Post	0.065*	0.071*	0.140***
	(0.039)	(0.038)	(0.042)
NearSubway × Post × Female	-0.029	-0.019	-0.068
	(0.053)	(0.051)	(0.063)
P-value of the joint F-test	0.311	0.704	0.114
Adj. R-Squared	0.722	0.685	0.722
	Panel D: Having 0-12 years old children		
NearSubway × Post	0.074**	0.060**	0.088*
	(0.031)	(0.030)	(0.047)
NearSubway × Post × Having 0-12 years old children	-0.052	-0.031	0.012
	(0.055)	(0.052)	(0.067)
P-value of the joint F-test	0.698	0.965	0.218
Adj. R-Squared	0.724	0.686	0.722
	Panel E: Age		
NearSubway × Post	0.827*	-0.191	0.584
	(0.487)	(0.404)	(0.799)
NearSubway × Post × Age	-0.051*	0.011	-0.030
	(0.028)	(0.022)	(0.046)
NearSubway × Post × Age^2	0.001**	-0.000	0.000
	(0.000)	(0.000)	(0.001)
Inflection point (Age)	32.717	49.099	34.348
Adj. R-Squared	0.729	0.690	0.722
Individual fixed effects	Yes	Yes	Yes
Company-Year-Month fixed effects	Yes	Yes	Yes
Obs	14,807	11,166	6,701
N (affected group)	144	112	56
N (unaffected group)	233	117	109
Mean of dep. var.	9.296	9.228	9.271
Std. Dev. of dep. var.	0.390	0.329	0.369
Mean of NearSubway	0.389	0.393	0.350
Std. dev. of NearSubway	0.488	0.489	0.477
Mean of Female	0.472	0.498	0.506
Std. dev. of Female	0.499	0.500	0.500
Mean of Having 0-12 years old children	0.406	0.366	0.480
Std. dev. of Having 0-12 years old children	0.499	0.482	0.500
Mean of Age	34.632	33.097	36.063
Std. dev. of Age	7.510	7.186	7.365

Notes: Two-way (at individual and year-month level) clustered standard errors are reported in parentheses.*, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively

Table 8: Effects of the Opening of Subway Line 15 on Employee Exit

	(1)	(2)	(3)	(4)
	At risk from entry or start of panel		At risk from start of tenure	
	Cox	Exponential	Cox	Exponential
Post	0.185	0.310**	0.903***	0.972***
	(0.180)	(0.136)	(0.179)	(0.175)
NearSubway	0.115	0.113	0.0745	0.0830
	(0.158)	(0.154)	(0.162)	(0.162)
NearSubway × Post	**-0.667*** **	**-0.691*** **	**-0.695*** **	**-0.713*** **
	(0.226)	**(0.226)**	**(0.243)**	**(0.249)**
Constant		-4.587***		-5.121***
		(0.801)		(0.848)
Controls	Yes	Yes	Yes	Yes
Observations	17,718	17,718	18,196	18,196
Log Likelihood	-1901	-702.7	-1751	-692.8
Wald Test	216.8	227	350.9	468.7

Notes: Each column represents a proportional hazards model. Columns (1) and (3) use a Cox Proportional Hazards specification, while columns (2) and (4) present an exponential model. In the first two columns the duration to exit is defined from the start of the panel period (for workers entering the panel at the start) and from month of entry for new employees. In columns (3) and (4), the duration to exit is from start of tenure till the exit date. For workers entering the panel at the start, we utilize administrative data on tenure; for new employees we can directly measure the tenure as months in the panel. All specifications include time-varying controls (Age, Experience) and static (defined at start of panel) controls (Education, Number of Children, Male Dummy, Manager Dummy, Distance, Company 1 Dummy, Married Dummy, and New Employee Dummy). Robust standard errors (clustered at the individual level) are reported in parentheses. *,**, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

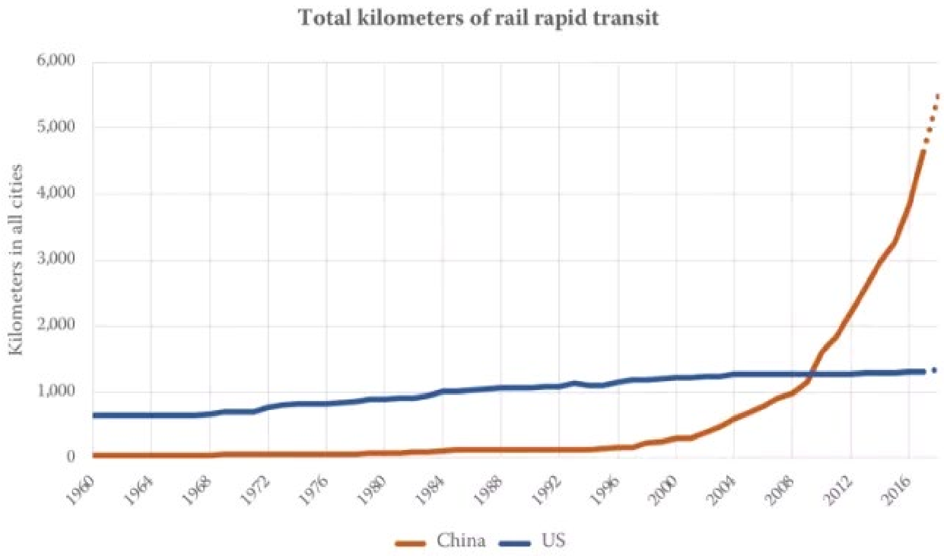
Table 9: Announcement Returns of Nearby Subway Station Construction Commencements

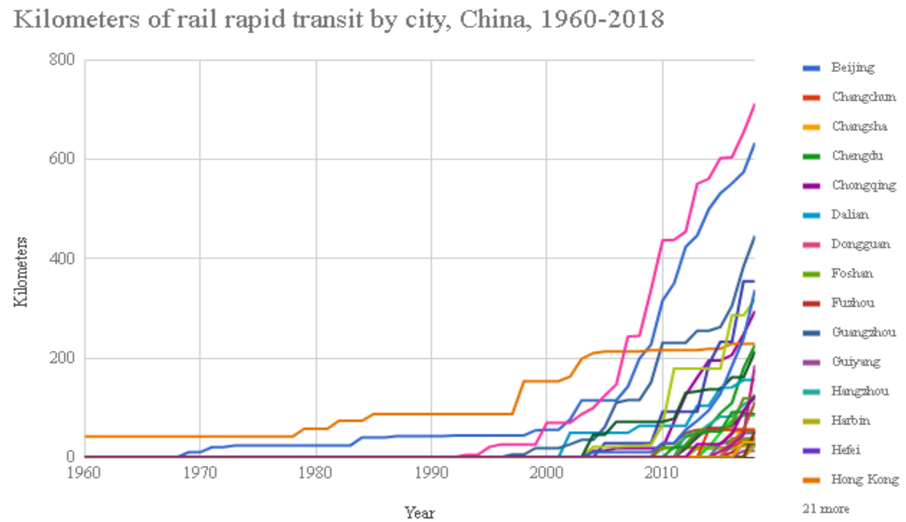
	(1)	(2)
	CAR(-2,2)	CAR(-5,5)
Mean	0.017**	0.019*
P-Value	(0.040)	(0.061)
Number of Firms	37	37
Number of Announcement Events	48	48
Number of Subway Lines	5	5

Notes: CAR(-2,2) and CAR(-5,5) are accumulated abnormal returns around the announcements of nearby subway station construction commencements of firms over 5 day and 11 day event windows, respectively. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Appendix Tables & Figures

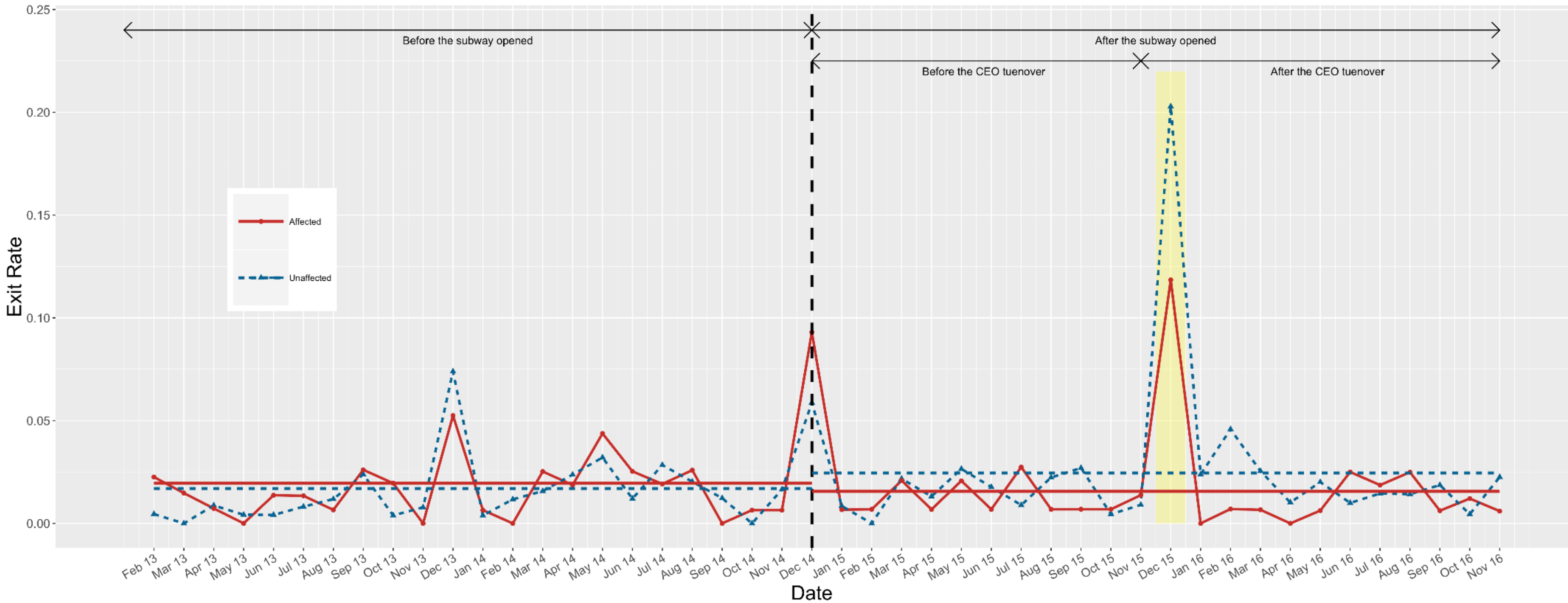
Appendix Figure 1: Increase in Subway Length in China





Notes: These figures are taken from Freemark (2018).

Appendix Figure 2: Raw Trends in Monthly Exit Rates



Notes: This figure shows the dynamics of the exit rate. Data are from Feb 1, 2013 to Nov 30, 2016. We eliminate those employees who moved after the subway opened. The exit rate is calculated as the number of exiting employees divided by the average number of employees at month t for affected and control groups, where the average number of employees is equal to the mean of the number of employees at month t-1 and month t+1. The vertical dashed line indicates the opening of the Subway Line 15. Horizontal lines indicate the means of the exit rate for affected and control over all the months before (after) the opening of the Subway 15 if it is to the left (right) of the vertical dashed line. The yellow shaded rectangle indicates the months around when the CEO turnover happened, which triggered a spike in worker exit.

Appendix Table 1a: Summary Statistics on Performance Scores

Sample			Performance Score			
Position	Occupation	Obs	Mean	Std. Dev.	Median	Coefficient of Variation (SD/Mean)
Full Sample		5,084	0.828	0.244	0.928	0.294
Non-Managers	-	3,928	0.819	0.250	0.913	0.305
Managers	-	1,098	0.871	0.200	0.953	0.230
-	Administration	865	0.924	0.132	0.953	0.143
-	Technology	2,125	0.853	0.116	0.870	0.136
-	Marketing	2,036	0.768	0.337	0.934	0.439
Non-Managers	Administration	363	0.952	0.089	0.967	0.093
Non-Managers	Technology	1,830	0.848	0.119	0.858	0.140
Non-Managers	Marketing	1,735	0.761	0.342	0.928	0.449
Managers	Administration	502	0.904	0.153	0.953	0.169
Managers	Technology	295	0.882	0.092	0.915	0.104
Managers	Marketing	301	0.804	0.305	0.948	0.379

Notes: This table reports summary statistics for individual monthly performance scores (scaled by a factor of 0.01) for workers in Company 1 for the period Jan. 2015 to Dec. 2016.

Appendix Table 1b: Summary Statistics on Bonus as Proportion of Total Income

Sample			Bonus/Total Income		
Position	Occupation	Obs	Mean	Std. Dev.	Median
Full Sample		14,807	0.369	0.272	0.384
Non-Managers	-	11,166	0.357	0.264	0.377
Managers	-	3,487	0.408	0.290	0.400
-	Administration	3,289	0.365	0.273	0.389
-	Technology	4,663	0.308	0.212	0.375
-	Marketing	6,701	0.413	0.298	0.393
Non-Managers	Administration	1,791	0.318	0.273	0.330
Non-Managers	Technology	4,008	0.301	0.214	0.375
Non-Managers	Marketing	5,367	0.411	0.284	0.393
Managers	Administration	1,498	0.422	0.261	0.400
Managers	Technology	655	0.348	0.195	0.383
Managers	Marketing	1,334	0.422	0.349	0.398

Notes: This table reports the summary statistics on individual-month level share of bonus in total income for both companies over the 2013-2016 panel period.

Appendix Table 2: Correlation between Bonus and Performance Score/Individual Sales

	(1)	(2)	(3)	(4)	(5)	(6)
	Log ($Bonus^T$)					
	Full Sample		Non-Managers		Marketing	
	Panel A: Monthly Performance Score in Company 1					
Performance Score	0.417***	0.470***	0.282***	0.431***	0.275***	0.383***
	(0.053)	(0.067)	(0.054)	(0.070)	(0.055)	(0.065)
Individual fixed effects	No	Yes	No	Yes	No	Yes
Year-Month fixed effects	No	Yes	No	Yes	No	Yes
Adj. R-Squared	0.103	0.790	0.067	0.750	0.109	0.711
Obs	5,632	5,632	4,324	4,324	2,246	2,246
	Panel B: Annual Individual Sales in Both Companies					
Log($Sales_{t+1}$)	0.054***	0.038***	0.065*	0.033***	0.078***	0.045***
	(0.018)	(0.012)	(0.037)	(0.012)	(0.029)	(0.014)
Individual fixed effects	No	Yes	No	Yes	No	Yes
Company-Year fixed effects	No	Yes	No	Yes	No	Yes
Adj. R-Squared	0.064	0.730	0.073	0.700	0.110	0.748
Obs	142	142	103	103	127	127

Note: Monthly individual-level Performance Score (scaled by a factor of 0.01) is from Jan, 2015 to Dec, 2016 for all workers in Company 1, Annual individual-level Sales is available for marketing personnel in both companies from 2013 to 2016; to account for the gap between initiated and executed sales, the sales measure used is a one-period lead of sales (i.e., $\text{Log}(\text{Bonus}_t^T)$ is regressed on $\text{Log}(\text{Sales}_{t+1})$. While individual sales is tracked only for marketing personnel, some marketing personnel move to other groups, so we have some observations for non-marketing personnel (defined as at Nov, 2014); hence the full sample is somewhat larger than the marketing sample. $Bonus^T$ is at employee-month level in Panel A and is summed up to employee-year level in Panel B. Two-way cluster standard errors are reported in parentheses, and they are clustered at individual and year-month level in Panel A and at individual and year level in Panel B. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Appendix Table 3: Effect of Subway on Individual-Level Marketing Sales

	(1)	(2)	(3)	(4)
		Log(Sales)		
NearSubway × Post’	0.797*	-	0.790*	-
	(0.451)	-	(0.442)	-
Log(TimeSaved + 1) × Post’	-	0.109*	-	0.108*
	-	(0.061)	-	(0.060)
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No	No
Company-Year fixed effects	No	No	Yes	Yes
Adj. R-Squared	0.654	0.654	0.657	0.657
Obs	202	202	202	202
N (affected group)	23	23	23	23
N (unaffected group)	56	56	56	56
Mean of dep. var.	13.320	13.320	13.320	13.320
Std. Dev. of dep. var.	2.058	2.058	2.058	2.058
Mean of Log(TimeSaved + 1)	-	2.170	-	2.170
Std. dev. of Log(TimeSaved + 1)	-	3.263	-	3.263
Mean of NearSubway	0.314	-	0.314	-
Std. dev. of NearSubway	0.465	-	0.465	-

Notes: The individual sales data is at employee-year level for both companies. To account for lags in reporting of sales used in bonus determination, we define Post’ as month>Dec 2015. We eliminate those employees who moved after the subway opened, and we keep only those employees who are in the company workforce at least one month before and after the opening of the subway. Two-way (at individual and year level) clustered standard errors are reported in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Appendix Table 4: Effect of the Subway on Employees' Attendance and Time at Work

Dependent variable	(1) Log (Late for Work)	(2) Log (Leave Early)	(3) Log(Sick Leave)	(4) Log (Personal Leave)	(5) Log (Maternity Leave)	(6) Log(Funeral Leave)	(7) Log(Marriage Leave)
	Panel A: NearSubway as the variable of interest						
NearSubway × Post	-0.001	0.003	0.000	0.005	0.015	0.000	-0.001
	(0.018)	(0.003)	(0.009)	(0.017)	(0.010)	(0.000)	(0.001)
Adj. R-Squared	0.275	0.047	0.071	0.103	0.102	0.005	0.007
N (affected group)	144	144	144	144	144	144	144
N (control group)	233	233	233	233	233	233	233
Obs	14,742	14,742	14,742	14,741	14,742	14,742	14,742
	Panel B: TimeSaved as the variable of interest						
Log(TimeSaved + 1) × Post	0.000	0.000	0.000	0.001	0.002	0.000	0.000
	(0.003)	(0.000)	(0.001)	(0.002)	(0.002)	(0.000)	(0.000)
Adj. R-Squared	0.275	0.047	0.071	0.103	0.102	0.005	0.007
N (affected group)	142	142	142	142	142	142	142
N (unaffected group)	233	233	233	233	233	233	233
Obs	14,668	14,668	14,668	14,667	14,668	14,668	14,668
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Company-Year-Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each dependent variable is in terms of the number of days of the month, e.g., Log(Late for Work) is defined as Log(Number of days in the month that the employee was marked as late for work in the administrative data +1). Two-way (at individual and year-month level) clustered standard errors are reported in parentheses.*, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.

Appendix Table 5: Impact of Subway Opening on Hiring Quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Performance Score					
	Employees hired after 2015			Employees hired before 2015		
NearSubway	11.029**	13.554***	14.363***	4.015	3.209	3.861
	(5.171)	(3.557)	(5.145)	(2.550)	(2.484)	(2.538)
Log(Total Income Net of Bonus)	0.430	-3.501		3.698*	-1.870	
	(1.693)	(2.153)		(1.998)	(2.470)	
Log ($Bonus^T$)		113.283***			26.529***	
		(16.435)			(5.618)	
Log (Total $Income^T$)			31.945***			12.734***
			(7.205)			(3.524)
Year-Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.175	0.618	0.23	0.028	0.115	0.056
Obs	1,095	1,095	1,095	5,084	5,084	5,084
N (affected group)	61	61	61	105	105	105
N (control group)	81	81	81	160	160	160
Mean of dep. var.	66.280	66.280	66.280	82.798	82.798	82.798
Std. Dev. of dep. var.	40.205	40.205	40.205	24.375	24.375	24.375

Notes: We obtained performance scores from Company One in 2015 and 2016. Two-way (at individual and year-month level) clustered standard errors are reported in parentheses.*, **, and *** denote statistical significance at 10%, 5%, and 1%, respectively.