Essays in Behavioral Labor Economics

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Submitted in partial fulfillment of the
requirements for the degree
of Doctor of Philosophy
in the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY

2019

Abstract

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This dissertation consists of three essays in Behavioral Labor Economics. The first two chapters contribute to the understanding of non-standard preferences of individuals in the workplace, and the third studies how cultural values affect firm behavior.

The first chapter studies the incentive effects of top-down favoritism in employee promotions on workers and its organization-wide productivity consequences, and provides evidence on social preferences and fairness concerns among co-workers. Using data from public high schools in four Chinese cities, I first show that teachers with hometown or college ties to the school principal are twice as likely to be promoted, after controlling for characteristics on their application profiles and their value-added in teaching. I then use the results from a survey in which I asked teachers to select anonymous peers to promote from a pool of applicants applying for promotion to infer each teacher's revealed fairness views regarding promotion qualifications. Contrasting these with actual past promotions in turn allows me to measure if and when a teacher might have observed unfair promotions in her own school in the past. Exposure to unfair promotions adversely affects non-applicant teachers' output, lowering their value-added and raising the probability that high-value-added teachers quit. The value-added effect appears to be driven primarily by teachers' social preferences for peer workers and the consequent erosion of their morale when peers suffer unfair treatment, while the quitting effect comes mainly from non-favored prospective applicants' career concerns as they learn about the principal's bias and leave due to poor promotion prospects. These adverse spillover incentive effects lead to a substantial reduction in school-wide output, which is only slightly mitigated by increased productivity among favored teachers. Finally, a transparency reform that required principals to disclose to their peers the profiles of teachers that apply for promotion reduced the principals' bias and improved the overall productivity of

schools.

The second chapter documents daily targeting behavior in workers' labor supply decisions. Using a novel dataset on the daily production of a group of piece-rate manufacturing workers combined with their quasirandom daily income shocks from lunch break card game gambling, I show that the workers' afternoon labor supply responds negatively to instantaneously-paid quasi-random gambling income, although wages are paid monthly. The workers' labor supply decisions were consistent with daily mental accounting and reference-dependence where the target was set on the sum of the face - valued daily (receivable) labor and (paid) unearned income, as opposed to the neoclassical model of inter-temporal labor supply. Estimation of two structural models of daily labor supply yields a coefficient of loss aversion parameter of 1.8 to 2.0, significantly different from the neoclassical value of 1; and individual specific loss aversion structural estimates correlate positively with survey measures. Using estimated preference parameters, I back out the implied total wage elasticity of daily labor supply as well as a sizable negative reference-dependent component of it. This study overcomes the common identification issues in the daily labor supply literature by exploiting high-frequency, actively taken-up and unanticipated income shifters that are independent of other labor supply and demand confounders.

In the third chapter, we show that many employers anchor their wages at establishments outside of the home region to headquarter levels, and begin to study the consequences. Our analysis makes use of an unusual 2005-2015 establishmentyear level dataset of average wages by narrowly-defined occupation. The dataset covers 1,800 large employers that span many different sectors and each operate in a subset of 170 observed capital city locations. We show that, across the occupational skill range—including for low-skill support staff—the average wage multinationals pay domestic workers in a given occupation at foreign establishments is robustly and remarkably highly correlated with the average wage they pay workers in the same occupation in the home country. We then instrument for headquarter wage levels with changes in home country minimum wage laws and show that externally imposed wage increases at home causally raise wages abroad. The relationships we establish between headquarters' and their foreign establishments' wage levels and wage changes are both driven by employers from inequality-averse societies. Occupations are more (less) likely to be removed from, and less (more) likely to be added to the foreign establishments (headquarters) of such employers after a (minimum wage-induced) wage increase originating at the headquarter. Our results point towards the existence of "wage cultures" that influence how production is organized across space.

Contents

List of Figures

List of Tables

Acknowledgements

It is a genuine pleasure to express my deep sense of gratitude to my advisor W. Bentley MacLeod for his invaluable guidance and support throughout the years. I have hugely benefited in the development and refinement of research ideas from his perspectives and insights as a sharp theorist and a rigorous empiricist at the same time. His constant curiosity, passion and devotion as a scholar keeps motivating me to learn and improve.

I am also immensely thankful to my another advisor Jonas Hjort for his indispensable advice, encouragement and patience that no words are commensurate with. He guided me with detailed and productive discussions through the writing of my dissertation, and has been incredibly supportive through my hard times.

This dissertation has also benefited from helpful suggestions and comments from Doug Almond, Miguel Urquiola, Michael Best, Mitch Downey, Jack Willis, Wojciech Kopczuk, Mikka Rokkanen, among others, and the seminar participants at Columbia University. I also thank all the relevant parties that have helped in providing data support.

I would like to extend my gratitude to the fellow Ph.D. students and friends at Columbia University for their encouragement, feedback, discussions, continuous help and support. Last I am grateful to my family and friends, who have always been supportive in everything I have pursued.

Chapter 1

The Costs of Workplace Favoritism: Evidence from Promotions in Chinese High Schools

Xuan Li[1]

[1] I am indebted to Bentley MacLeod, Jonas Hjort and Doug Almond for their guidance and support throughout this project. I have also benefited from insightful comments and helpful suggestions from Wojciech Kopczuk, Mitch Downey, Michael Best, Miguel Urquiola, Albert Park, Francois Gerard, Miikka Rokkanen, Gregory Cox, Alex Eble, Francois Gerard, Brendan O'Flaherty, ScottWeiner, Danyan Zha, and participants of the Columbia Applied Microeconomics Colloquium for helpful discussions. Several unnamed officials in the department of education of a Chinese province provided invaluable data support. All errors are my own.

1.1 Introduction

Economists have long debated whether and when leaders in organizations should have discretion to make free choices - for example in hiring, task assignment, or promotions - as opposed to having to follow rules. The efficiency implications depend on whether the benefits of private information or the costs of favoritism dominate. Empirical evidence is mixed and context-specific.[2] However, the existing literature restricts attention to the *selection* decisions made by leaders themselves,[3] and we know almost nothing about how giving leaders discretion - and any resulting favoritism - changes the effort choices of downstream agents. Such incentive effects may be critical for the organization-wide productivity costs of granting upstream discretion.

Theoretically, the downstream incentive consequences of upstream favoritism for organizational performance are ambiguous. Favorable treatment can breed loyalty and induce reciprocity from the favored workers (Akerlof, 1982). Discrimination, however, can lower the incentives of the workers facing negative bias. (Prendergast and Topel, 1996; MacLeod, 2003). Apart from the relative importance of these two counteracting effects, the organization-wide impact also depends on how third-party co-workers, who are neither favored nor discriminated against, respond to favoritism they observe.[4] Such spillover effect can arise if third-party workers view themselves as potential victims or beneficiaries of (future) discrimination,

[2]Some studies find evidence supporting discretion (see e.g. Brogaard et al., 2014; Li, 2017; Weaver, 2018), while others find evidence supporting rules (see e.g. Bandiera et al., 2009; Giuliano et al., 2009; Fisman et al., 2017; Hjort, 2014; Durante et al., 2014a; Hoffman et al., 2017; Bandiera et al., 2017; Xu, 2018), and yet others find mixed evidence (see e.g. Beaman and Magruder, 2012; Zinovyeva and Bagues, 2015; Jia et al., 2015).

[3]For example, previous studies have looked at whether more productive workers are referred to employers by referees (Beaman and Magruder, 2012), hired by recruiters (Giuliano et al., 2009; Hoffman et al., 2017; Weaver, 2018), assigned to more tasks or more profitable positions by upstream leaders (Hjort, 2014; Xu, 2018), helped in collaborative jobs by managers (Bandiera et al., 2009), and promoted to higher positions by evaluators (Zinovyeva and Bagues, 2015; Jia et al., 2015; Fisman et al., 2017; Xu, 2018); whether higher-quality papers are published (Brogaard et al., 2014) and whether research projects of higher potentials are funded (Li, 2017); etc.

[4]If the groups of favored and discriminated against downstream agents are equal in size, which is probable when the resources for allocation are fixed, and these two counteracting effects are of similar magnitudes, the average effect of favoritism on the victims and the beneficiaries will be zero. In this case, the overall impact depends solely on how third-party co-workers respond.

or if favoritism is perceived as a fairness violation, which can reduce workers' effort (Koszegi, 2014; Falk et al., 2018a).[5]

This paper provides empirical evidence on the impacts of leaders' favoritism on subordinates' incentives at work by examining how school principals' bias in teacher promotions resulting from social connections affects teachers' behavior and consequent school-wide performance in Chinese public high schools. I show that applicant teachers who are socially tied to the current school principal are more likely to receive promotions than their equally-qualified competitors. Principals' biased promotion decisions in favor of applicants who belong to their social groups violate teachers' fairness norms, which adversely affects non-applicant teachers' output at both the intensive and the extensive margins, lowering their teaching performance in terms of value-added and raising the probability that high-value-added teachers quit. Mandatory disclosure of promotion applicant teachers' profiles to their peer teachers makes teachers respond more harshly to unfairness, reduces principals' bias and improves the overall productivity of schools.

Given that top-down favoritism is widespread in management, understanding how it causally shapes employees' behavior has important implications for organizational performance and how to improve it. However, there are two main challenges in uncovering empirical evidence on this question. The first issue relates to the difficulties in measuring both how employees perceive favoritism and their effort. Without information on what employees think unbiasedness should look like based on their available information, it is hard to tell whether and to what extent they observe the bias of their bosses, which might in turn affect the employees' behavior.[6] It is also difficult to measure the effort expended by workers even

[5] In the *Negative Reciprocity* section of the Global Preference Survey in Falk et al. (2018a), respondents were asked about their willingness to punish someone for unfair behavior, either toward themselves or toward a third person, and a substantial share of respondents gave positive answers to both questions. Behavioral agency theory (e.g. Koszegi, 2014) predicts that workers may reduce effort in response to perceived fairness violations, especially under incomplete contracting.

[6] Previous studies have utilized surveys to understand what constructs workers' fairness views in the workplace (e.g. Kaur, 2018). However, these surveys asked respondents to self-report their attitudes towards generally described workplace situations of discrimination or unfairness, and did not simulate contingent settings in the real workplace accounting for what workers actually observe and how they process their available information.

if their output can be observed, as production may be affected by the principal's favoritism itself if there exist complementarities between inputs from the principal and from workers in the production technology.[7] The second challenge is to convincingly isolate exogenous variation in favoritism. Pairing of principals and agents is endogenous both on the labor market and within an organization;[8] even within a principal-agent pair, the principal's exhibited favoritism might correlate with many other traits of his and other characteristics of his relationship with the agent.[9] Field experiments might do a better job in terms of introducing exogeneity in favoritism and measuring differences in workers' perception of bias and effort provision,[10] but they suffer from potentially more severe external validity concerns, and it is sometimes difficult to perform organization-wide welfare analysis when artificial experimental interventions do not provide straightforward counterfactuals to benchmark realities against (see e.g. Breza, 2015).

Chinese public high schools provide an empirical setting which arguably allows me to address the endogeneity and the measurement concerns discussed above and to causally estimate the incentive effects of observing principals' favoritism on workers' effort choices. Teachers in these schools are promoted through the Chinese Professional Title Evaluation System where evaluations are conducted annually, and principals' recommendations play

[7]For example, discriminated-against workers might be provided with fewer or less desirable tasks (Bandiera et al., 2009; Hjort, 2014), interact less with managers (Glover et al., 2017), not receive prestigious titles in academia which might affect citations (Zinovyeva and Bagues, 2015; Fisman et al., 2017), etc.

[8] Existing studies deal with this problem mainly by exploiting plausibly exogenous turnover of principals (e.g. Xu, 2018) or quasi-random assignment of workers to positions or shifts (e.g. Bandiera et al., 2009; Hjort, 2014; Glover et al., 2017).

[9]To partially address this, Glover et al. (2017) surveyed the employees to provide suggestive evidence on mechanisms through which minority cashiers are less productive under biased managers in a French grocery store chain, and they find the most support for lack of workplace interactions between the minority workers and the biased managers.

[10]Generally, field experiments allow researchers to fix individual production technology and manipulate the wage rates of hired workers and their perceptions about how they are treated compared to co-workers. For example, Cohn et al. (2014) randomly assign working in groups of two performing identical individual tasks without complementarities, cut wages either of both workers or just one of the two after paying same-group workers the same, and compare study productivity responses; Breza et al. (2017) conduct an experiment in which they randomly assign workers in groups of three performing identical and non-complenentary tasks in which individual productivity is either easy or hard to observe, randomize whether coworkers within groups receive the same flat daily wage or differential wages according to their (baseline) productivity ranks, and compare their attendance and productivity at work.

a crucial part in determining who receives promotion in the form of a title elevation. A transparency reform was introduced to different cities in different years, which required principals to disclose the profiles of teachers who apply for promotion to their peers, after which teachers can presumably better observe the qualifications of promotion applicants and the favoritism exercised by principals if they promote unqualified applicants. In cooperation with a provincial department of education in China, I collected newly digitized application profiles (formatted CVs) of teachers applying for the senior-ranked title in the promotion system in 4 cities from 2001 to 2017, merging them with personnel records of teachers and school principals as well as student test scores, to construct a unique dataset containing information on the teaching performance and the promotion history of teachers in all public high schools in these cities.

To measure social connections, I construct two pre-determined proxies of connectedness between teachers and principals: whether they were born in the same city and whether they attended the same college, which have been well-documented by previous studies to play an important role in social networks and resource allocations in China (see e.g. Fisman et al., 2018; Dai et al., 2018).[11] To measure teachers' performance at work, I rely on the beginning-of-year and end-of-year class-subject average test scores of classes taught by each teacher and other class characteristics to estimate her individual-year-specific value-added (VA), namely her impact on student test scores, which is commonly used in the education literature.[12] Individual-specific average VA in a school is used to measure teachers' *quality*, while within-individual-across-year changes in VA are used to proxy their *effort*. The rationale of using value-added to measure effort comes from the observation that the production function of class test scores does not directly involve the participation of (biased) principals and is relatively homogeneous across teachers in different promotion statuses, holding class characteristics fixed.[13] I measure how teachers perceive unfairness resulting from principals'

[11]Other examples can be found in Shih et al. (2012); Cai (2014); Jia et al. (2015); Johnston (2017); Wang (2016); Fisman et al. (2017).

[12]See Hanushek and Rivkin (2010) for a review.

[13]I control for a set of observable job characteristics in the empirical analysis including workload, class

favoritism in promotions in 3 steps. First, I infer the teachers' views on appropriate qualifications for a fair promotion using the results of a survey conducted in 2018 in which I presented to the teachers the application CVs of a pool of anonymous applicants and asked them to select whom they think should receive a promotion. Then I use these inferred fairness views together with the CVs of actual applicants in the past to construct what the teachers' perceived unbiased promotion results should have looked like. In the last step, I contrast these "virtual" fair results with actual promotion decisions made by principals to measure if and when the teachers observed unfair promotions in their own schools after the transparency reform.[14]

Three main sources of variation are used to identify the existence of favoritism by principals and its impacts on teachers' behavior and school-level output. First, the turnover of principals in each school, which is decided by the local bureau of education rather than the principals themselves (unless they resign), generates shocks to the social connections between the teachers who apply for promotion and their principals. These within-school shocks enable me to examine how changes in social connectedness affect the promotion prospects of teachers in the same school, thus addressing potential sorting on time-invariant unobserved characteristics between teachers and principals to schools.

Second, holding a principal's preferences for her friends and a teacher's fairness views constant, the variation in the extent to which the principal makes unfair promotion decisions as perceived by the teacher comes from the across-year variation in the quality of the socially-connected teachers relative to the unconnected ones in the pool of applicants in a school: in a year in which the principal's socially tied applicants are better qualified or far less qualified than the untied ones, a biased principal might still make seemingly unbiased decisions in which the more qualified applicants are promoted, while in another year when the tied applicants are moderately less qualified than the untied ones, a biased principal

re-assignments and end-of-last-year class test scores, etc.

[14]This revealed preference approach potentially allows me to better measure teachers' past unfairness perceptions than a retrospective self-reporting approach (see e.g. Podsakoff and Organ, 1986; Podsakoff et al., 2003).

might promote the former instead of the latter, leading to perception of unfairness by a teacher when she can observe the quality of the applicants. These within-principal-teacher shocks in the level of perceived promotion unfairness allow me to study how teachers respond to biased decisions made by principals, holding the principals' time-invariant characteristics as well as the time-invariant principal-teacher matching effects constant.

Third, the sample period captures variation in the observability of principals' biased behavior to teachers introduced by the transparency reform which mandated disclosing promotion applicants' CVs to the whole teaching staff in each school. Changes in perceptions of bias might affect how teachers respond to a given level of favoritism; how much favoritism principals consequently choose to exercise in their promotion decisions, holding their preferences constant, if they care about consequences of how teachers view their unfairness; and how these two effects combine to determine school-wide output in equilibrium.

The paper presents four main empirical findings. Using event studies exploiting within-school changes in the social groups school principals belong to induced by principal turnovers, I first show that applicant teachers who are socially connected to the incumbent principal are twice as likely to be promoted as their unconnected colleagues in the same school, after controlling for characteristics on their application CVs and their individual average value-added in the school. Principals value applicants' characteristics including value-added, experience and workload when making their promotion decisions, but seem to set a lower bar for promotion for those who are socially connected to them.

The second and the main finding is on the incentive effects of principals' favoritism on teachers' work performance. Exploiting within-principal-teacher variation in the teachers' perceived unfairness in the principals' promotion decisions, an averagely-biased promotion decision made by the same principal in each year lowers the yearly value-added of a non-applicant teacher in the school by 0.35 standard deviations in the same year and increases the probability that she quits the job by 16%, compared to a promotion decision that the teacher regards as fair. These negative effects persist for at least 3 years before they fully dissipate.

Placebo tests show that perceived unfairness in future promotion results do not affect the teachers' current behavior. Unfair promotion decisions do not affect the *average* value-added or quitting probability of the applicants group; this is because the positive effects on the favorably promoted applicants are offset by the negative impacts on the unfairly denied ones. Therefore, the incentive consequences of promotion unfairness for the whole teaching staff in a school are driven almost exclusively by the adverse *spillover* effects on non-applicants.

The effect of the principal's favoritism on teachers' value-added, or effort provision, appears to be driven mainly by the teachers' social preferences for their peers and their undermined morale at work when peers suffer unfair treatment, as the effect is (1) most pronounced among non-prospective applicants and among those who have the most frequent interactions with the unfairly denied applicants,[15] (2) equally large for teachers with and without social connections to the decision-making principal, and (3) persistent even if the principal has stayed in the school for a long time, when the teachers presumably already know her well, and even if the principal has left the school and can no longer be "punished" by the teachers' reduced teaching performance. The quitting effect, on the other hand, appears to come primarily from non-favored prospective applicants' career concerns in that they learn about the principal's bias and leave due to inferred poor promotion prospects, as the effect is (1) the largest among prospective applicants who have high value-added but are not socially connected to the current principal, and (2) stronger when the principal has just entered the school, when her (biased) promotion decisions may potentially cause teachers' beliefs about her preferences to update dramatically.

Exploiting within-principal-school variation in the average level of promotion unfairness perceived by the whole teaching staff in each year, I also find that school-wide performance under the same principal is worse when the principal exhibits more favoritism in teacher promotions. In line with the adverse incentive effects on individual teachers, a school loses better-quality teachers while failing to replace them with equally able ones, produces lower

[15] Non-prospective applicants are defined as those who have already obtained the highest Professional Title and will not participate in future promotion applications.

test scores for the existing students and enrolls new students with lower prior test scores when its principal behaves unfairly in selecting applicant teachers for promotion.

In the last part of the paper, leveraging the across-city variation in the timing of the transparency reform, I perform difference-in-differences and triple-difference estimations and show that when non-applicant teachers can better observe the quality of their applicant peers, they respond more adversely to unfair promotions, and consequently principals become less biased in favor of their socially connected subordinates in promotions. The overall impact of this information disclosure policy on school performance is positive, as it raised the students' test scores in the provincial College Entrance Exams in cities to which the reform was introduced.

Taken together, the results suggest that giving school principals discretion in choosing which teachers to promote is costly, as they make choices in favor of those who are socially connected to them, and this leads to negative response in the teachers' effort choices when exposed to perceived unfairness and consequent decrease in school-wide output. Revealing applicants' profiles to peer teachers can partially correct the principals' favoritism and improve efficiency.

Related Literature.

This paper contributes to the literature on social incentives in organizations (see Ashraf and Bandiera, 2018, for a great review). I show that favoritism in teacher promotions by Chinese public high school principals has adverse incentive effects on teachers' effort and quitting choices out of career concerns and social preferences, while the detrimental consequences of leaders' top-down *vertical* social preferences towards subordinates documented in previous studies result mainly from inefficient selection or its combination with incentive distortion (e.g. Bandiera et al., 2009; Hjort, 2014; Glover et al., 2017; Xu, 2018).[16] In this sense this paper bridges the organizational economics literature on rules versus discretion

[16] Xu (2018) isolates the effect of top-down social connections on downstream performance by fixing the position a colonial governor of the British Empire held during the patronage period and comparing the revenue the governor generated when he was connected to the incumbent Secretaries of State or not.

at the leadership level (e.g. Hoffman et al., 2017; Horton, 2017; Jacob et al., 2018) with the literature in labor and behavioral economics on how workers respond to perceived treatment by employers (e.g. Krueger and Mas, 2004; Gneezy and List, 2006; Kube et al., 2012, 2013; DellaVigna et al., 2016; Breza et al., 2017). It is also shown that school principals' favoritism-based discretion has negative aggregate impacts on organization-wide ouput, in line with findings of e.g. Giuliano et al. (2009); Bandiera et al. (2009); Hjort (2014); Hoffman et al. (2017); Xu (2018).

The paper also relates to the literature on fairness norms (e.g. Adams, 1963; Fehr et al., 1993; Fehr and Schmidt, 1999a; Fehr and Gächter, 2000; Falk et al., 2008; Henrich et al., 2010; Durante et al., 2014b; Falk et al., 2018a; Enke, 2018) and their workplace consequences (see e.g. Kahneman et al., 1986a; Fehr et al., 2009; Kaur, 2018). Previous studies have focused on the negative reciprocity behavior by workers upon whom unfairness is directly inflicted (e.g. Krueger and Mas, 2004; Bracha et al., 2015; Breza et al., 2017; Dube et al., 2018b; Coviello et al., 2018; Cullen and Perez-Truglia, 2018a). I show that the negative incentive effect of principals' favoritism on teachers' value-added in teaching comes primarily not from the teachers who directly suffer from the principals' biased practices themselves, but instead from the already-promoted colleagues with whom they frequently interact. This finding provides suggestive evidence of *horizontal* social preferences between co-workers (e.g. Bandiera et al., 2005; Charness and Kuhn, 2007; Cohn et al., 2014; Hjort, 2014; Breza et al., 2017), and that violations of fairness for co-workers can also trigger negative responses, providing field evidence in the workplace for the survey results of Falk et al. (2018a).[17] In addition, in their fairness notions the teachers heavily emphasize high value-added or productivity in the qualification for promotion and consequent salary increase, consistent with findings of e.g. Baron and Kreps (2013); Bracha et al. (2015); Breza et al. (2017) that workers view pay inequalities that reflect productivity differences as justifiable.

[17] A closely related finding is in Breza et al. (2017), where the authors show that paying lower wages to co-workers reduces the attendance rate of the better-paid workers within the same production unit. However, these better-paid workers are still directly affected by pay inequality and might suffer from social pressure - they are beneficiaries instead of victims.

Finally, the paper speaks to the consequences of social connections in developing economies. When markets function imperfectly, social connections can either enhance or lower economic efficiency.[18] In the context of China, hometown ties and college ties are well-established in earlier work to play significant roles in China in the growth of private enterprises (Dai et al., 2018) and in promotions in the Chinese bureaucracy (e.g. Shih et al., 2012; Jia et al., 2015; Wang, 2016; Fisman et al., 2017) and academia (Fisman et al., 2018). This paper complements previous findings by documenting the negative productivity consequences of these social ties in the Chinese public school system, which is an important setting in that schooling is crucial for economic growth (e.g. Barro, 1991) and school quality is critical to the return to education (e.g. Card and Krueger, 1992). Evidence on public high schools could shed light upon the whole Chinese public sector, including state-owned enterprises,[19] to which the same employee promotion system applies. Moreover, the paper points towards *internal* information disclosure within organizations as a potentially effective instrument to help address the costs of favoritism based on social ties in personnel policies, supplementing previous studies on the role of *external* transparency to the public in combating corruption and improving accountability of government officials in the developing world (see e.g. Besley and Burgess, 2001, 2002; Brunetti and Weder, 2003; Ferraz and Finan, 2008; Reinikka and Svensson, 2004, 2005, 2011).

The remainder of the paper is organized as follows. Section 1.2 introduces the empirical setting, the data, and the construction of the social ties and value-added measures. Section 1.3 shows evidence of favoritism by principals in teacher promotions based on social connections. Section 1.4 explains how I use the survey to infer the teachers' revealed fairness views and their perceived unfairness in past promotion results. Section 1.5 presents the

[18]Efficiency-enhancing roles of social networks include supporting business activity, facilitating job search, and providing social insurance and liquidity, while the negative efficiency implications inlcude impeding labor mobility, distorting resource allocation, and lubricating rent-seeking and corruption. See Munshi (2014) for a recent review on the empirical evidence of social networks and their implications in developing countries.

[19]The state-owned enterprises (SOEs) hold more than 30% of total assets of the secondary and tertiary sectors in China, see `http://blogs.worldbank.org/eastasiapacific/state-owned-enterprises-in-china-how-big-are-they`.

adverse incentive effects of unfair promotions on teachers' behavior as well as their impacts on school-wide output. Section 1.6 evaluates the transparency reform in terms of reducing favoritism and improving productivity. Section 1.7 concludes.

1.2 Empirical Context and Data

1.2.1 Chinese Professional Title Evaluation System

Public high schools belong to the system of Public Institutions in China, where teachers are evaluated and promoted through the Professional Title Evaluation System that covers all professionals working in state-run enterprises and public institutions. Launched in 1986, this evaluation system is used to assign different professional ranks (or titles) to workers, which are a crucial determinant of the government-funded salaries they receive. Employees are typically elevated step by step in their ranks during their professional career.

For high school teachers, there are mainly 3 ranks from low to high: junior, middle and senior.[20] The junior rank is automatically assigned to teachers that are formally employed by the public high school system; the middle rank and the senior rankcan be applied for after fulfilling mandatory workload and seniority requirements.[21] Therefore, there are two levels of rank elevation: from junior to middle, and from middle to senior. Evaluations are conducted annually.

The fraction of senior-ranked teachers in each school is decided by the local bureau of education (20% to 30%, depending on schools and years), so the number of middle-to-senior promotion slots for each school in a given year is pre-determined, while there are no strict

[20] A higher rank called "high senior" was introduced in 2013 to high school teachers, but only less than 1% with "exceptional contributions" have received it. I do not consider this rank in this paper, and merge it to the senior rank.

[21] A junior-ranked teacher can apply for the middle rank after holding the junior rank for at least 4 years if he has a college degree and 2 years if he has a graduate degree. A middle-ranked teacher is eligible to apply for the senior rank after being assigned the middle rank for at least 5 years and having worked in the current school for at least 4 years.

quotas for the middle-ranked.[22] Consequently, there is within-school competition among the middle-ranked teachers applying for the senior rank each year, while the junior-ranked ones do not need to compete with each other for the middle rank. I focus on the elevation from the middle to the senior rank, to which the term "promotion" refers throughout the paper unless indicated otherwise. Promotion brings an increase in monthly salaries by 12%-15% (depending on cities and years), and as salary growth is exponential over time and pensions are proportional to the salaries at retirement, an (early) promotion has a persistent influence on teachers' life-long earnings, making the promotion results economically important to them.[23]

In the four cities I study, application starts in September, while the final promotion decisions are made available to teachers in each school in November. The process goes as follows: first the middle-ranked applicants submit their application profiles (a detailed CV and a personal statement) to the principal of the school they work in; after gathering all applications in the school, the principal writes a recommendation opinion for each applicant, which is very crucial in determining the results; then the recommendation letters, together with the application profiles, are submitted to a third-party committee in the city bureau of education, which is the final decision maker; after reviewing the application materials from all schools in the city, the committee delivers the promotion decisions back to each school, which are then made public to all the teachers.

Transparency Reform.

In the 4 cities I study, it is required that the name list of applicants be publicized to all the teachers in each school during the whole sample period. In 2005, city A became a pilot city of the nation-wide Open Public Information program, the then newly appointed Secretary of city A's Municipal Communist Party Committee launched an anti-corruption

[22] There are cases where one or two available slots are left vacant in a year and filled in the next years, which are not very common.

[23] The salaries of these public high school teachers are determined by their seniority and professional titles, which are independent of the variation in their performance. The salaries are funded jointly by the municipal and the provincial finance.

campaign requiring information disclosure in all personnel appointments and promotions in the city's public institutions, including the public high schools. This transparency reform requires that in addition to the name list, the promotion applicants' CVs should also be posted publicly to the whole teaching staff in each school. After piloting, the Regulations of the People's Republic of China on Open Government Information was adopted by the State Council in May 2007 and effective in May 2008[24] However, the regulations do not cover public institutions other than local governments, and information disclosure in public schools is not compulsory nationally even after the regulations became effective.

Although not mandatory, The transparency policy has since been adopted by the public school system of all the four cities by 2013 (see Table 1.1). Adoption was decided by the then incumbent leaders of the local bureau of education in each city. Conversations with local officials suggest that the policy was adopted by subsequent cities as it was believed that it could effectively combat corruption and improve school performance in previously adopting cities.[25]

1.2.2 High School Operation

Principals in the public high schools studied in this paper are appointed by the local bureau of education which commonly rotates principals across different schools in each city, and the average length of a principal's term in a school is around 6 years. The high schools are managed by the principal, the highest leader, together with a Communist Party secretary, and a group of middle-level leaders including the deputy principals, the deans and the vice deans for academic affairs, moral education, general services, and the teachers' union, etc. These middle-level leaders are usually selected from the senior-ranked teachers in the school, and to spare time for administrative responsibilities, their teaching workload is usually reduced

[24] See for example https://www.cecc.gov/resources/legal-provisions/regulations-of-the-peoples-republic-of-china-on-open-government.

[25] In Section 1.6, I evaluate this policy using econometrics tools and show that this was indeed true.

to half of that of the regular teachers.[26]

A cohort stays three years (or grades) in Chinese high schools. Students are enrolled in high schools based on their scores in the High School Entrance Exams (HEE), which are organized at the city level. The composition of students in a class does not change across grades in principle, except at the end of the grade 1 when the students choose one of the the two categories of subjects, liberal arts or sciences, to major in and prepare for the corresponding College Entrance Exams (CEE) upon graduation.[27] The College Entrance Exams are organized each year at the provincial level. At the end of grade 1 and grade 2, each city also organizes uniform end-of-year exams, so students' scores in these tests are comparable within the same cohort in a city.

It is common practice that a teacher follows the same classes in all grades for which the subject she teaches is required, and then returns to grade 1 with another school cohort. Most teachers teach several classes within one grade at a time. There is a class head teacher to manage student affairs for each class, which position is usually held by one of its teachers for all the three years. Teachers who retire before the graduation of the classes they currently teach are mainly replaced by newly hired veterans, while fresh college graduates usually start from grade 1. The existing teachers are re-assigned to new classes or grades mainly when the new veterans cannot fill all the non-grade-1 vacancies left by those who retire, leave the school or are selected as middle-level managers, which takes place infrequently.[28]

[26]Unfortunately, information on the middle-level leaders is not available in my data. However, I can infer possible taking-up of such positions from (permanent) teaching workload changes.

[27]Change to a new class is uncommon, as the students are assigned to classes at the beginning of grade 1 according to their stated preference for liberal arts or sciences. Unless they change their mind, they would stay in their grade-1 class. If a student changes class, she is usually placed in a class that matches her end-of-grade-1 test scores in the category of subjects she chooses.
The College Entrance Exams include the subjects Chinese, math and English for all students, in addition to which the liberal arts category tests politics, history, geography and the sciences category tests physics, chemistry and biology. At the end of grade 2, students in a category stop taking the 3 subjects required in another.

[28]The probability of such change is 9.8% in the sample I study.

1.2.3 Administrative Data

The main administrative dataset I use is the newly digitized records of teachers' senior rank promotion application CVs, as well as the promotion results, in 112 public high schools in the four largest cities in a Chinese province from years 2001-2017 (see Table 1.1).[29] It covers all the public high schools in these cities which enrolled around 0.8 million students during the sample period. The application CVs include information on the applicants' demographics and their work performance measures in various aspects within the past 6 years prior to application, which I categorize into 6 types:

$$\mathbb{G} = \{\text{Demographics, Experience, Workload, Research, Teaching, Other}\}. \tag{1.1}$$

Variables on the application CVs, denoted as $\mathbf{X} = \{\mathbf{X}_g\}_{g \in \mathbb{G}}$, are listed in Table 1.2.

I supplement the application profiles with three other datasets. The first is the teachers' personnel records (1993-2017), which cover all the teachers in each school and provide information on their gender, ethnicity, city and year of birth, Communist Party membership status, college and/or graduate school attended, subject taught, past rank-elevation history in the Professional Title Evaluation System, etc. This dataset overlaps with a subset of the application profiles for the promotion applicants, while it also covers the non-applicants with less detailed information.

The second dataset is the students' test scores records (1995-2017), which include the class-subject average test scores in the High School Entrance Exams, the end-of-year exams and the College Entrance Exams. Information on class size, class head teacher and teacher assignment by subjects is also available, enabling me to match classes to the teachers who taught them in both the application profiles and the personnel records.[30] The test scores are mainly used to estimate the year-specific value-added of each teacher, which I discuss in Sub-section 1.2.4.2.

[29] The total population of these 4 cities was 18.9 million in 2015, which accounts for 36.7% of the provincial population.

[30] Only the end-of-year teachers of each class are recorded, therefore in the rare cases where there are changes of teachers during a school year, I credit the last one as the teacher who has taught the class for the whole year.

The last one is the profiles of school principals (1994-2017), which record the principals of schools in each year, their gender, age, city and year of birth, and the college and/or graduate school they attended.

I only include the years for which the promotion application profiles are available, and the teachers who teach one of the 9 subjects tested in the College Entrance Exams including Chinese, math, English, physics, chemistry, biology, politics, history and geography, as there is no uniform test score information on other subjects.[31] The sample covers 35,714 teachers, 210,424 teacher-year observations, 20,528 promotion applicants and 59,121 promotion applications filed. The mean promotion success rate within each school is 21.7% per year, and the average number of applications filed by a teacher who has applied at least once is 2.88, with an ultimate success rate of 51.6% for each teacher. Other summary statistics about the schools and the teachers are shown in Tables 1.3 and 1.4.

1.2.4 Construction of Variables

In this section, I explain how I construct two of the three primary variables used in the empirical analysis: the measures of social connections via which favoritism takes place, and the time-varying value-added measures of teachers which capture both their individual teaching quality and their productivity fluctuations over time. The third key variable, perceived promotion unfairness, will be discussed later in Section 1.4.

1.2.4.1 Social Ties

I consider social ties between a teacher and her principal or her colleagues that are predetermined at the time they are paired in a school: college alumni ties and hometown ties, which have well-established precedence in the Chinese context in earlier work.[32]

[31]The distribution of CEE subjects taught by applicants is shown in Figure A1. Non-CEE subjects include PE, music, arts, computer science, etc.

[32]See for example Cai (2014); Jia et al. (2015); Wang (2016); Johnston (2017); Fisman et al. (2017, 2018).

For teachers (or principals) i and j, HomeTie_{ij} (CollegeTie_{ij}) takes value 1 if they were born in the same city (graduated from the same college). SocialTie_{ij} is a dummy indicating that i and j are connected through at least one of these two ties.

The cities in my sample are the four largest ones in the province with a large population of migrant workers, including teachers and principals, from smaller places. 93% of the teachers and the principals graduated from one of the 6 colleges located in these cities (three in city A, two in each of cities B and C, and one in city D). 45% of them were born in the city they currently work in, and 42% graduated from a college located there.

The mean values of the between-principal-teacher measures of social ties, $\text{HomeTie}_{i,P(i,t)}$, $\text{CollegeTie}_{i,P(i,t)}$ and $\text{SocialTie}_{i,P(i,t)}$, are 0.231, 0.197 and 0.327 respectively.[33]

1.2.4.2 Teachers' Value-Added

To investigate whether there exists bias in the promotion recommendations by principals and their possible impacts on the behavior of school teachers, the first step is to construct a performance measure that is comparable both across different teachers to proxy individual *quality*, and within the same teacher over time to portray her *effort* provision, or performance. I choose value-added, which is defined as a teacher's impacts on students' test scores, mainly for two reasons. First, it is a standard measure of teacher quality in the education literature (Hanushek and Rivkin, 2010), used in practice for teacher evaluations in many school districts in the US and is shown to be a good predictor of students' long-term outcomes (Chetty et al., 2014b). Moreover, test scores in the highly competitive College Entrance Exams are virtually the sole college eligibility determinant of Chinese high school students, and high school teachers are valued by parents, principals, and more generally the Chinese society, mainly through their achievements in helping students earn high scores in these exams.

Unlike the student-level data used in most previous studies, my dataset provides test scores only at the class-subject-year level. Nevertheless, the commonly-used value-added

[33]Table 1.5 provides more detailed summary statistics of the social tie statuses between teachers and principals.

model is still applicable. Specifically, I estimate the following empirical model for the average test scores of class c in subject k, school year t and grade $g(c,t)$, noted A_{ckt}:

$$\begin{aligned} A_{ckt} &= f^A_{k,g(c,t)}(A_{c,k,t-1}) + \phi^A \mathbf{x}_{ct} + g^A_{h(c)}(t) + \nu_{ckt}, \\ \text{where} \quad \nu_{ckt} &= \mu_{i(c,k,t),t} + \varepsilon^A_{ckt}. \end{aligned} \tag{1.2}$$

A_{ckt} is standardized to have zero mean and unit standard deviation within each subject-city-year cell.[34] $\mathbf{x}_{ct}$ is a vector of year-specific class characteristics, such as class size and an indicator for being an honor class. $f^A_{k,g(c,t)}(A_{c,k,t-1})$ is a subject-grade-specific control function of the end-of-last-year class average test scores in subject k, and is parameterized using cubic polynomials following Kane and Staiger (2008). $g^A_h(t)$ is a school-specific quadratic time trend. The error term ν_{ckt} is decomposed into two parts: a teacher-year effect for teacher i, $\mu_{i(c,k,t),t}$, and a class-subject-level idiosyncratic shock, ε^A_{ckt}. The estimate of the teacher-year-specific component is defined as the value-added (VA) of teacher i in year t.

$$\text{VA}_{it} := \hat{\mu}_{it}.$$

Measuring teachers' quality.

In order to study whether the better applicants are selected in the promotions, my first aim is to obtain a consistent measure of each teacher's quality, or their individual-specific VA. This is also the goal of most previous studies, where the VA is assumed to be time-invariant within each teacher (Kane and Staiger, 2008) or to drift smoothly over time allowing for auto-covariance across years (Chetty et al., 2014a). As there might exist complementarities between schools and teachers and it is the within-school performance that should be more relevant in promotion evaluations, I compute the teachers' individual within-school average VA as the measure of their quality:

$$\text{VA}_{ih} := \overline{\text{VA}_{it}}^{t \in \{t: h(i,t)=h\}}.$$

[34]Each class-subject-year observation is weighted by the class size. For grade 1, the end-of-last-year test scores refer to the scores in the High School Entrance Exams.

The underlying assumption for the above measure to represent teachers' true teaching quality is that classes in a school are not sorted to teachers on unobservable determinants of test scores ($\mathbb{E}\left[\varepsilon^A_{ckt}|i,h\right]=0$). However, it is conceptually not a serious issue even if this assumption does not strictly hold, as the main purpose I use this measure for is to compare promotion applicants, and it is reasonable that the teachers' actual teaching performance, rather than their innate ability, is presumably more relevant in promotion evaluations.[35]

Measuring teachers' effort.

The main purpose of this paper is to study changes in teachers' behavior in response to the promotion results in their schools, and the major outcome variable I consider is the level of teaching effort exerted, which I proxy using the within-teacher fluctuations in VA across years in the same school. For within-teacher-school variation in VA_{it} to capture changes in teacher i's teaching effort, it is required that $\mathbb{E}\left[\varepsilon^A_{ckt}|i,h,t\right]=\mathbb{E}\left[\varepsilon^A_{ckt}|i,h\right]$, that is, unobservable determinants of class average test scores do not correlate with a teacher's real input in the production function of scores within a school.[36] I cannot test for this assumption directly, but it does not cause omitted variable bias in the estimates of the impact of promotion results on the estimated VA measures if these unobservables do not themselves correlate with the promotion results. I will discuss this problem in the robustness checks in the empirical analysis.[37]

[35] The variation in VA_{ih} accounts for 63.4% of the total variance of VA_{it}, and the within-teacher-school residuals grow with experience for the first 5-6 years and then remain stable afterwards (see Figure A3), which parallels the findings in Rockoff (2004).

[36] As I include controls in the empirical estimations that are at higher levels than teacher-school fixed effects, such as teacher-principal fixed effects, I do not take out within-teacher-school means (VA_{ih}) from the variable VA_{it} in advance.

[37] The major drawback of the VA estimation I perform is that I cannot observe the students' individual test scores or demographics. To check whether this causes serious issues, I cross-check how well my VA estimates match with alternative estimates using the Chetty et al. (2014a) method in a subsample for which individual test scores are available: all the schools in city A in years 2009-2017. The two VA estimates correlate nicely with each other with a correlation coefficient of 0.96 (see Figure A2), providing suggestive evidence that the main VA measures capture the teachers' true impacts on test scores relatively well in the schools studied in this paper. Information on student demographics are not available in the subsample, but it might not cause substantial bias if Chinese high schools are comparable to schools in the US, where Chetty et al. (2014a) show that most of the sorting of students to teachers that is relevant for future test achievement is captured by prior test scores.

1.3 Favoritism in Principals' Promotion Decisions

In this section, I present the first main result of the paper: the estimate of the extent to which principals favor applicants who are socially tied to them in their promotion recommendations.

1.3.1 What Signals High Value-Added?

Before answering the question whether the principals tend to promote their socially connected applicants, I first investigate whether and what characteristics of an applicant on her application CV and/or her social ties to the principal can help predict her VA. Specifically, I estimate:

$$\mathrm{VA}_{jt}^{-6} = \sum_{g \in \mathbb{G}} \mathbf{X}_{jt}^{g} \gamma_{g}^{\mathrm{VA}} + \alpha_{H}^{VA} \mathrm{HomeTie}_{j,P(j,t)} + \alpha_{C}^{\mathrm{VA}} \mathrm{CollegeTie}_{j,P(j,t)} + f_{h(j,t)}^{VA}(t) + \varepsilon_{jt}^{\mathrm{VA}}, \quad (1.3)$$

where VA_{jt}^{-6} is applicant j 's average VA in the past 6 years prior to her application in year t,[38] $\mathbf{X}^{g}$ denotes the vector of variables in category $g \in \mathbb{G}$ on the application CVs (see Definition (1.1)), and $f_{h}^{VA}(t)$ is a school-specific time trend. Standard errors are clustered at the applicant level.[39]

The estimation results are reported in Table 1.6. Of the 6 categories of applicant characteristics, only the teaching awards explain more than 10% of the variation in VA (58%). Social ties to the principal do not help predict an applicant's teaching performance.[40]

It is not surprising that the teaching awards are the most informative on the application CVs in terms of signaling the applicants' VA, as they are designed to honor teachers who produce bigger increase in the CEE scores of their students compared to their HEE scores

[38] I use the 6 year average VA as the performance measure because the performance-related information listed on the application CVs covers the same period of time. While the compulsory requirement is 4 years, over 96% of promotion applications are filed after at least 6 years in the current school, therefore VA^{-6} covers (a part) of the individual within-school teaching performance of an applicant.

[39] I cluster standard errors at the teacher level in most of the regressions. I also cluster standard errors at the principal level for robustness checks, which does not change the results substantially.

[40] Although $\hat{\alpha}_{H}^{VA}$ and $\hat{\alpha}_{C}^{VA}$ are statistically significant, their magnitudes are below 0.1SD of the outcome variable, and their partial R^2 is very small (0.0032).

per se.[41] The combination of the 2 highest teaching awards received in the past 6 years is indicative of an applicant's VA during the period (see Figure 1.1).[42] The applicants' average VA increases monotonically with the level of the highest teaching award they received, holding the second highest award fixed, and vice versa. An applicant who receives the highest possible awards (two 1st Prizes) is on average 3.1 standard deviations higher in her VA compared to one who receives the lowest awards (two Excellence Prizes).

1.3.2 Principals' Promotion Recommendation Decisions

Favoritism by principals via social ties.

I analyze how the principals decide whom to promote based on characteristics shown on the applicants' CVs and whether they are socially tied to the principal. Specifically, I run:

$$\mathbb{E}\left[\text{Promoted}_{jt}\right] = F\left(\mathbf{X}_{jt}\gamma^{P} + \alpha^{P}\text{SocialTie}_{j,P(j,t)} + \beta^{P}\text{Controls}_{jt}\right), \tag{1.4}$$

where $\mathbf{X}_{jt}$ denotes the vector of variables on the application CVs, and SocialTie_{jt} is an indicator for applicant j being connected to the principal through either hometown ties or college ties. Controls_{jt} includes applicant-irrelevant promotion probability determinants

[41] According to an official in the provincial department of education, these awards are based on "the within-city-year ranking of the difference between the average standardized (SD=1 within subject-city-year) College Entrance Exams scores and the average standardized High School Entrance Exams scores of the students (whose grade 3 is) taught by a teacher. From low to high levels the awards include the Excellence Prize (20%) which almost every teacher who teaches the 3rd grade in the year receives, the 3rd Prize (40%), the 2nd Prize (25%) and the 1st Prize (15%). To examine how well the award-assigning rules described above are followed, I construct the corresponding running variable, a crude 3-year value-added measure, using $\Delta A_{jt} := A_{jt} - A_{j,t-3}$, where A_{jt} is the mean CEE scores of the graduating classes teacher j teaches in year t and $A_{j,t-3}$ is the mean HEE score of these classes. The relationship between this running variable and teaching awards is presented in Figure A5. The discontinuity in teaching award levels at the crude VA cutoffs should be sharp (that is, one) if the running variable is computed without errors and the rules of teaching award assignment are strictly followed. The estimated effect of surpassing the corresponding crude VA cutoffs on the probability of receiving a higher-level teaching award is 0.913. The crude 3-year VA measure has a 0.714 correlation with the 3-year average of teacher-year-specific VA (VA_{it}), the measure I construct, and indeed the 3-year average VA is a fuzzy predictor of teaching awards: surpassing the corresponding mean VA cutoffs increases the probability of an elevation in teaching award by 0.618 (see Figure A6).

[42] The application CVs list teaching awards received by the applicant in the past 6 years, during which period it is typical for a teacher to go through the CEE twice with 2 cohorts. Therefore, most applicants list 2 teaching awards, and I construct $4 \times 4 = 16$ dummies on each combination of the two highest-level teaching awards. There are a few who list a 3rd teaching award, for which I include dummies separately in the estimation.

including a school-year fixed effect, $\lambda_{h(j,t),t}$, which controls for the expected success rate pre-determined by the number of available slots and the number of applicants in each promotion, and the share of applicants teaching the same subject as applicant j in her school, $\text{share}_{k(j),h(j,t),t}$, which gauges the within-subject competition for promotion slots. The coefficient α^P in Equation (1.4) characterizes the principals' favoritism towards applicants with hometown or college ties.

Table 1.7 reports the regression results. Columns (1) and (2) show estimates using a logit model[43], and the rest of the table presents results using a linear probability model. Columns (2), (4) and (6) add controls of the applicants' school-specific VA (VA_{jh}), which captures possible private information the principals might have about the applicants' quality that is not reflected on their application CVs.[44] Columns (5) and (6) include a individual fixed effect, λ_j, fixing the comparison of promotion results within the same teacher who applies multiple times to address possible sorting of teachers to schools or principals. The estimates are robust to different specifications and controls.[45] An applicant who is socially tied to the principal is on average twice as likely to be promoted as an untied counterpart (see Panel (A)).[46] When considering hometown ties and college ties separately, the effect on promotion rates is 80% of the former and 60% of the latter (see Panel (B)).[47]

To present more explicitly the influence principals have on the promotion results in their schools, I use event studies to investigate whether the entry of a new principal to a school

[43] $F(x) = \frac{e^x}{1+e^x}$.

[44] This is probable as the principals receive reports on the class-subject average test scores in the city-level end-of-year exams in their schools as well as the city-wide score distributions, and in principle they are able to compute crude value-added measures of a teacher using this information. However, the extent to which principals are willing and/or able to do such computations is in question. Jacob and Lefgren (2008) show that principals in a western US school district can generally identify teachers who produce the largest and smallest VA but have far less ability to distinguish between teachers in the middle.

[45] Although including the applicant fixed effects boost the standard errors, the coefficient estimates are still statistically significant.

[46] The point estimate $\hat{\alpha}^P$ is around 21 percentage points while the average success rate is 22%.

[47] In this case, the estimation equation is

$$\mathbb{E}\left[\text{Promoted}_{jt}\right] = F\left(\mathbf{X}_{jt}\gamma^P + \alpha^P_H \text{HomeTie}_{j,P(j,t)} + \alpha^P_C \text{CollegeTie}_{j,P(j,t)} + \beta^P \text{Controls}_{jt}\right). \tag{1.5}$$

The point estimates $\hat{\alpha}^P_H$ and $\hat{\alpha}^P_C$ are around 18 and 14 percentage points, respectively.

who comes from a different hometown or college than the old one has an impact on the promotion rates of applicants in the school who belong to different hometown or college groups. Upon such an event, the applicants can be divided into three types in terms of their social tie connectedness to the old and the new principals:

$$\mathbb{Q} = \{\text{Tied before \& untied after, Untied before \& tied after, Untied before \& after}\}.$$

I estimate:

$$\text{Promoted}_{j,t+s} = \sum_{\tau=-3}^{3} \sum_{q \in \mathbb{Q}} \mu_{q\tau} \mathbb{I}\left[q\left(j\right) = q, s = \tau\right] + \mathbf{X}_{j,t+s}\gamma^{\mu} + \beta^{\mu}\text{Controls}_{j,t+s} + \varepsilon^{\mu}_{j,t+s}. \quad (1.6)$$

The coefficients $\left\{\mu_{q\tau}\right\}_{q,\tau}$ represent the expected promotion probability of applicant group q in year τ relative to the entry of the new principal, after residualizing out within-school-year average promotion rates and the applicants' characteristics on their CVs.

Figure 1.3 plots the results. The horizontal axis displays the years relative to the principal entry, and on the vertical axis the coefficient estimates $\left\{\hat{\mu}_{q\tau}\right\}_{q,\tau}$ are shown. We can see that the arrival of a new principal immediately raises the probability that her socially tied applicants get promoted, and the loss of social connections upon the departure of a principal takes away at once the advantage these connections bring to the promotion prospects. $\left\{\hat{\mu}_{\text{untied to tied},\tau}\right\}_{\tau\geq 0}$ and $\left\{\hat{\mu}_{\text{tied to untied},\tau}\right\}_{\tau<0}$ are similar in magnitudes to the coefficient estimates $\hat{\alpha}^{P}_{H}$ and $\hat{\alpha}^{P}_{C}$ in Equation (1.5), as expected (see Table A4). The results also show that there is trivial heterogeneity in the extent of favoritism by how long the principals have stayed in their current schools: they exhibit such discrimination as soon as they start their term, and getting to know the teachers better does not correct their bias.

Principals' evaluation criteria.

Apart from social ties, it is worth looking at how the principals select applicants in general. The first finding is that they still value other information on the application CVs including teaching awards. The estimated coefficients on the teaching award combinations in Equation (1.4) are plotted in Figure 1.2, where we can see that winning better teaching awards increases an applicant's chance of receiving a promotion, with the winners of the highest awards on average 63 percentage points more likely to succeed compared to the recipients of the lowest awards. Higher experience, heavier workload, more journal publications and other awards also better an applicant's promotion prospect (see Tables A1 and A2).

I also investigate whether the principals evaluate applicants within the socially tied group and the untied group respectively in similar ways in terms of other characteristics on their application CVs . For clarity I first reduce the dimensionality of $\mathbf{X}$, which includes a lot of variables, by partitioning it into the 6 categories $g \in \mathbb{G}$ (see Definition (1.1)) and constructing the applicants' categorical promotability indices (as valued by the principals):

$$\begin{cases} \hat{\eta}_{jt}^{P,g} := & \mathbf{X}_{jt}^{g} \hat{\gamma}_{g}^{P}, \ g \in \mathbb{G}, \\ \hat{\eta}_{jt}^{P,Tie} := & \hat{\alpha}_{H}^{P} \text{HomeTie}_{j,P(j,t)} + \hat{\alpha}_{C}^{P} \text{CollegeTie}_{j,P(j,t)}, \end{cases} \tag{1.7}$$

where $\left\{\hat{\gamma}_{g}^{P}\right\}_{g\in\mathbb{G}}$, $\hat{\alpha}_{H}^{P}$ and $\hat{\alpha}_{C}^{P}$ are the coefficient estimates from a regression of Equation (1.4).[48] I then estimate:

$$\mathbb{E}\left[\text{Promoted}_{jt}\right] = F\left(\sum_{g\in\mathbb{G}}^{\omega\in\{0,1\}} \rho_{g}^{\omega} \mathbb{I}\left[\text{SocialTie}_{j,P(j,t)} = \omega\right] \hat{\eta}_{jt}^{P,g} + \rho^{Tie} \hat{\eta}_{jt}^{P,Tie} + \beta^{\rho} \text{Controls}_{jt}\right). \tag{1.8}$$

The difference between ρ_{g}^{1} and ρ_{g}^{0} displays the extent to which the principals put higher weight on category-g characteristics in selecting whom to promote within the socially tied group in comparison with the untied group. I also replace the categorical promotability indices with a single social-tie-irrelevant composite index:

$$\hat{\eta}_{jt}^{P,\mathbf{X}} := \sum_{g\in\mathbb{G}} \hat{\eta}_{jt}^{P,g},$$

[48] I use the coefficient estimates from the logit model separating hometown ties and college ties without extra controls (see Panel (B), Column (1) of Tables 1.7 and Column (1) of A2).

to study how the principals refer to the applicants' CVs in general.[49] Results are shown in Table 1.8. We can see that when evaluating applicants with different social connectedness, the principals use information from the application CVs in a very similar way ($\hat{\rho}_g^0 \approx \hat{\rho}_g^1$ for $g \in \mathbb{G}$). This is also true if we think of the principals as selecting applicants by their VA: within each promotion evaluation the gaps between promotees and unpromoted applicants in their average teacher-school-specific VA are similar in both the socially tied and the untied groups (see Table 1.9):

$$\hat{\mathbb{E}}_{ht}\left[\text{VA}_{jh}|\text{Tied, Promoted}\right] - \hat{\mathbb{E}}_{ht}\left[\text{VA}_{jh}|\text{Tied, Denied}\right] = 0.996,$$
$$\hat{\mathbb{E}}_{ht}\left[\text{VA}_{jh}|\text{Untied, Promoted}\right] - \hat{\mathbb{E}}_{ht}\left[\text{VA}_{jh}|\text{Untied, Denied}\right] = 0.942.$$

To conclude, in promotion evaluations the principals act as if they build a promotability index for each applicant using some uniform criteria which correlate positively with the applicants' VA, and show favoritism by setting a differentially low promotion threshold in this index for those applicants who are socially connected to them.

1.3.3 Robustness Checks

To interpret the adjusted difference in promotion rates between socially tied and untied applicants as evidence for the principals' favoritism, two underlying assumptions need to hold.

The first assumption is that the difference does not come from the principals' private information. That is, social ties must not correlate with determinants of the applicants' quality that are observable to the principals but not to me, otherwise the omitted variable bias will occur. Apart from controlling for the rich set of variables on the application CVs and the applicants' estimated VA, which has substantially helped in addressing such concerns, I provide several further robustness discussions here.

[49]In this case, the estimation equation is

$$\mathbb{E}\left[\text{Promoted}_{jt}\right] = F\left(\sum^{\omega\in\{0,1\}} \rho^{\omega}\mathbb{I}\left[\text{SocialTie}_{j,P(j,t)} = \omega\right]\hat{\eta}_{jt}^{P,\mathbf{X}} + +\rho^{Tie}\hat{\eta}_{jt}^{P,Tie} + \beta^{\rho}\text{Controls}_{jt}\right). \quad (1.9)$$

First, balance tests in Table A3 and Figure A7 show that applicants who are socially tied to their principals are not stronger, and, if anything, weaker than their untied competitors along observable characteristics. Given these patterns, it is not very likely that the socially tied applicants are so much better in the principals' private information set as to justify their starkly higher promotion rates. Second, as shown in the event studies, the principals exhibit bias in promotion evaluations throughout their term in a school. Even if it is true that the principals learn about their connected subordinates better over time and choose to promote those whom they know well, it should not show up in the differential promotion rates at the very beginning of their term when they have presumably very limited information about the applicants' within-school performance. On the other hand, even if it is the case that the principals promote their connected teachers at the beginning because they learn about their (good) performance faster, it cannot explain the persistence of their bias over years. Third, if a principal relies substantially on her private information which tells very different things about the connected and the unconnected applicants, it is not probable that she treats their application CVs in homogeneous ways as evidenced by the estimation results of Equation (1.8).

The second assumption is that a successful or a failed promotion does not lead to differential changes in how much applicants of different social connectedness to the principal contribute to the total output of each school. This issue relates to the question whether promoting the best-teaching-quality applicants is better for the whole school than promoting the principal's friends.[50] There are several stories in which this might not be the case. First, if the low-quality friends of a principal, once promoted, return the principal's favor by working harder, while the unpromoted high-quality untied applicants do not retaliate (or at least not as much as the friends reward), then there is rationale for the principal to favor the former group. Another possibility is that principals might cooperate better with the

[50]For example, Benson et al. (2018) show that firms which prioritize current job performance when making promotion decisions, at the expense of other observable characteristics that better predict managerial quality, suffer from substantial costs of managerial mismatch.

promoted senior-ranked teachers who are socially tied to them. As described in Sub-section 1.2.2, some senior-ranked teachers in each school are selected as middle-level leaders who assist the principal in school management. If a principal works better in a management team consisting of those who share the same hometown and/or college background with her, she might promote her socially connected applicants out of efficiency concerns rather than favoritism, even though these applicants might not produce the highest VA in students' test scores.

The answer to the above question is empirical. In fact, the findings presented in the next section show that the opposite is true: favoritism violates the fairness views of teachers, causes worsened teaching performance and leads to productivity costs for each school.

1.3.4 Possible Mechanisms

It is worth pointing out that principals' bias is likely driven by their intrinsic social preferences for their social groups (see e.g. Bandiera et al., 2009),[51] but not necessarily so. The favors could also be handed out in exchange for unobserved bribes, which social ties are likely to facilitate (see e.g. Wang, 2016),[52] or future rewards from the favored subordinates on other occasions than the schools (see e.g. Jia et al., 2015).[53] With the empirical results presented, I cannot distinguish between these possible mechanisms and therefore do not hold a strong stand on whether the principals' social preferences, corruption, or gift exchanges, play a major rule in driving the observed favoritism.

[51] Bandiera et al. (2009) show that when managers in a fruit-picking worksite are paid fixed wages, they favor workers to whom they are socially connected irrespective of the workers' ability.

[52] Wang (2016) argues that connections enable effective and safe communication among corrupt Chinese military officers through communication, exchange and neutralization.

[53] Jia et al. (2015) show that connections foster loyalty of junior officials to senior ones in Chinese bureaucracy, which is not necassarily reflected in the local GDPs of the provinces governed by the junior officials.

1.3.5 Who shows higher favoritism?

In this sub-section, I explore whether and how the weights placed by the principals in their promotion evaluations on the applicants' characteristics and social ties vary with principal types. Specifically, I estimate:

$$\mathbb{E}\left[\text{Promoted}_{jt}\right] = F\left(\sum_{g\in\{\mathbb{G},Tie\}}\left(\tilde{\rho}^{D}_{1g}+\tilde{\rho}^{D}_{2g}D_{P(j,t),t}\right)\hat{\eta}^{P,g}_{jt}+\beta^{\tilde{\rho}}\text{Controls}_{jt}\right), \qquad (1.10)$$

where $\left\{\hat{\eta}^{P,g}\right\}_{g\in\mathbb{G}}$ are the categorical promotability indices in Definition (1.7), and D represents principal demographics including age and gender. The coefficients of interest are $\left\{\tilde{\rho}^{D}_{2g}\right\}_{g\in\{\mathbb{G},tie\}}$, which characterize the heterogeneity in how much principals value applicant characteristics in the 6 different categories.

Results are shown in Table 1.10. The most pronounced heterogeneities lie in teaching awards and social ties. Compared to their female counterparts, male principals put 60% higher weight on the applicants' social ties.[54] A 10-year-older principal values social ties 50% more and teaching achievements 40% less compared to a younger one.[55] Male and older principals give more favoritism towards promotion applicants with social ties, and the latter also emphasize teaching quality less.[56]

1.3.6 Social Ties and Application Decisions

The analysis in this section has focused on the impacts of possessing social ties to the principal on the success rates of promotion applicants. Social connections can have influence on whether eligible middle-ranked teachers decide to apply or not.[57] If lacking connections

[54] $\frac{0.487}{0.783}\approx 60\%$, see Columns (3) and (4) of Row (7) in Table 1.10.

[55] $\frac{0.452}{0.987}\approx 50\%$, see Columns (1) and (2) of Row (7); $\frac{-0.385}{0.974}\approx -40\%$, see Columns (1) and (2) of Row (5) in Table 1.10.

[56] A related work is Bagues et al. (2017), in which the authors show that male evaluators are more favorable toward their own sex than their female counterparts in evaluating applications to associate and full professorships in Italy and Spain.

[57] See for example Bagues et al. (2015), in which the authors show that connections are an important determinant of application decisions in the Italian academia, where selection on social connections is negative

to the principal discourages high-quality teachers from applying, then the overall (negative) selection effects of favoritism might be even larger than what shows up in promotion evaluations themselves.

I define eligible applicants as the middle-ranked teachers who satisfy the mandatory experience and seniority requirements for applying for the senior rank, and then compare the differences in VA between the socially tied and the untied teachers among the eligible applicants, the actual applicants and the promotees:

$$\Delta_p := \hat{\mathbb{E}}_{ht}[\mathrm{VA}_{jh}|\mathrm{Untied},\, p] - \hat{\mathbb{E}}_{ht}[\mathrm{VA}_{jh}|\mathrm{Tied},\, p],$$

where $p \in \{\text{Promoted, Applied, Eligible}\}$.

Table 1.9 reports the results. The socially untied teachers are of higher VA compared to the tied ones in all the three groups, while the differences are widened as we move from the eligibles to the promotees,[58] indicating the existence of both self-selection on social connections by prospective applicants in their application decisions and selection by principals in their choices over applicants in terms of VA, while the magnitude of the former is around 17% as large as the latter.[59]

1.4 Teachers' Perceptions of Unfairness

In this section, I build the bridge between the two main parts of empirical analysis in the paper: documenting the existence of favoritism by school principals via social ties (Section 1.3), and studying the teachers' responses to their perceived unfairness in promotion results which is a direct consequence of the principals' favoritism (Section 1.5). Coefficient α^P in Equation (1.4) characterizes the principals' *preferences* for their social connections,[60] which

in that prospective candidates are significantly less likely to apply when the committee includes a colleague or a co-author.

[58] $\Delta_{\text{Promoted}} > \Delta_{\text{Applied}} > \Delta_{\text{Eligible}} > 0$.

[59] $(\Delta_{\text{Applied}} - \Delta_{\text{Eligible}}) \,/\, (\Delta_{\text{Promoted}} - \Delta_{\text{Applied}}) \approx 17\%$, see Table 1.9.

[60] Strictly speaking, the coefficients $\left(\gamma^P, \alpha^P\right)$ in a reduced-form logit model of promotion decisions do not characterize structural preference parameters. However, under a fixed policy environment (e.g., under

is not directly observed by the school teachers. It is the principals' promotion decisions, or their *behavior*, that the teachers observe and potentially respond to.

I first discuss conceptually how the differences between the principals' (biased) preferences and the teachers' fairness preferences lead to the teachers' perceived unfairness in promotion results, and explain the source of variation in the perceptions of unfairness that identifies their impacts on the behavior of teachers and consequently the aggregate productivity of schools. Then I turn to implementation and explain how I use the results from a survey to infer the teachers' revealed fairness notions and the extent of unfairness they might perceive in past promotions in their schools.

1.4.1 Conceptual Framework

Denote the promotion results in school h in year t, which are observable to the school teachers, as Ω_{ht}.[61] It can be viewed as the image of the characteristics of the applicants in that year, $\left\{\left(\mathbf{X}_{jt}, \mathrm{SocialTie}_{j,P(j,t)}\right)\right\}_{h(j,t)=h}$, which are observable to peer teachers after the transparency reform, under a mapping function that is parameterized using the preferences of the principal, $\left(\gamma^P, \alpha^P\right)$.

$$\left\{\left(\mathbf{X}_{jt}, \mathrm{SocialTie}_{j,P(j,t)}\right)\right\}_{h(j,t)=h} \overset{\Omega\left(\gamma^P,\alpha^P\right)}{\longmapsto} \Omega_{ht}, \tag{1.11}$$

where the mapping function, $\Omega\left(\gamma^P, \alpha^P\right)$, guided by the specification of Equation (1.4), is given by:

$$\text{Promote } j \text{ iff } \mathrm{Ranking}_{ht}\left(\eta_{jt}^P\right) \leq N_{ht},$$

where N_{ht} is the number of promotion slots, and the latent variable is defined as:

the transparency policy), they can proxy the principals' preferences. I abuse terminology here and refer to $\left(\gamma^P, \alpha^P\right)$ as preference parameters.

[61] Ω_{ht} is a N_{ht}-dimensional vector of binary variables indicating the promotion status of each applicant, where N_{ht} is the number of applicants in school h in year t.

$$\eta_{jt}^{P} := \mathbf{X}_{jt}\gamma^{P} + \alpha^{P}\text{SocialTie}_{j,P(j,t)}.$$

To measure unfairness in the promotion results Ω_{ht} perceived by teacher i in the school, it is necessary to understand what the fair results under her fairness views, noted Ω_{it}, would have been. The difference between Ω_{it} and the actual promotion results,

$$\Delta\Omega_{it} := \Omega_{it} - \Omega_{h(i,t),t},$$

is the unfairness perceived by teacher i.

Conceptually, Ω_{it} can be viewed as the image of applicant characteristics $\{(\mathbf{X}_{jt}, \text{SocialTie}_{ji})\}$ under a mapping function that takes the same functional form as Ω but uses the preference parameters of teacher i, (γ^{i}, α^{i}):

$$\{(\mathbf{X}_{jt}, \text{SocialTie}_{ji})\}_{h(j,t)=h(i,t)} \overset{\Omega\left(\gamma^{i},\alpha^{i}\right)}{\longmapsto} \Omega_{it}. \tag{1.12}$$

Existence of perceived unfairness.

Notice that if $\alpha^{P} = \alpha^{i} = 0$ and $\gamma^{P} = \gamma^{i}$, teacher i's perceived fair promotion results Ω_{it} , will exactly coincide with the actual results $\Omega_{h(i,t),t}$, regardless of the characteristics of the applicants. That is, if teachers in a school evaluate the applicants' CVs in the same way as their principal and neither of them have preferences for their social ties in the workplace, there will not exist unfairness perceived by the teachers. Therefore, perceived unfairness arises from at least one of the following cases: (i) the principal and the teachers hold different opinions regarding which information on the applicants' CVs is important, (ii) they both value social ties but they belong to different social groups, or (iii) they are socially tied to each other but they do not value social ties equally heavily.

Variation in perceived unfairness.

If preference parameters $\left\{\left(\gamma^{P}, \alpha^{P}\right), (\gamma^{i}, \alpha^{i})\right\}$ are constant over time, the variation in perceived unfairness $\Delta\Omega_{it}$ comes from the variation in $\left\{\left(\mathbf{X}_{jt}, \text{SocialTie}_{j,P(j,t)}, \text{SocialTie}_{ji}\right)\right\}_{h(j,t)=h(i,t)}$, the composition of applicants in each year. For example, if an applicant who is socially tied to the principal is of higher quality compared to her untied competitors, the school teachers might not disagree with the principal promoting her; but if she is relatively weak but chosen over more qualified untied applicants, teachers in the school might view the promotion results as unfair. Notice that under these two scenarios the preferences of the principal do not change, and it is the relative quality of the principal's social connections compared to the other applicants that causes the variation in the extent to which the teachers perceive unfairness.

Although Ω_{it} is not directly observable, it can be derived if parameters (γ^i, α^i) are known. I use a survey in which I asked teachers to select anonymous peers to promote from a pool of applicants applying for promotion to infer each teacher's revealed preferences $(\hat{\gamma}^i, \hat{\alpha}^i)$. The survey is described in the next sub-section.

1.4.2 Survey

In early September 2018, I collaborated with the local education bureau of 2 of the 4 cities in the sample (cities A and B) and conducted surveys in 6 high schools (3 in each city), in order to collect information on the teachers' views of fairness regarding what type of applicants they think should be promoted.

Design.

We pulled the application CVs of all the applicants in year 2017 in these 6 schools and erased the names of the applicants, the schools and the cities. Then we printed these processed CVs of year-2017 applicants of each high school h, and put them into a folder $\mathscr{F}_h$. Then we presented $\mathscr{F}_h$ to teachers from another school h' ($h' \neq h$), and asked them to evaluate the CVs of these anonymous applicants and pick N_h ones they would choose to promote, where N_h is the number of applicants actually promoted in school h in year 2017. I refer to the applicants whose CVs were used in the survey as "virtual" applicants, and their schools “virtual" schools.

Implementation.

We produced 300 copies of each folder $\mathscr{F}_h$ for the survey. At the beginning of the weekly teaching staff meeting of each high school in city r ($r \in \{A, B\}$),[62] we prepared equal number of folders of the three virtual schools from the other city, $\{\mathscr{F}_h\}_{r(h)\neq r}$, and distributed one randomly selected folder to each teacher[63]. The teachers were given an hour to complete the selection task. Surveyed respondents, 684 in total, were identifiable and linked to the teachers' personnel records via a one-time ID number.

My goal is to collect the respondents' revealed preferences over virtual the characteristics of applicants on their CVs in deciding whom they think should be promoted, or how they map from applicant characteristics to promotability according to their fairness views. The subsequent step is to use these mapping functions to infer which actual applicants they might have thought deserved to be promoted in the past promotion evaluations in their own schools, to be contrasted with actual promotion decisions made by the then principal.

This approach has both advantages and limitations. Instead of asking the teachers to report retrospectively which of their colleagues they thought should have been promoted in the past years, in which case one might expect the realized promotion results to affect their answers, we placed the teachers in a disinterested context and asked them to solve a similar problem to that which is faced by the principals, which reduces the cost of truth telling and memory loss. However, in the virtual applicants we cannot simulate the teachers' real-life interactions with their peers and the private information about each other they might gather through these interactions.

[62]This is to make sure that in principle all the teachers were present and responded so as to minimize the sample selection problem. In fact, 96.6% of the teachers in these schools took the survey, and they are representative of teachers in the whole sample in terms of individual characteristics, see Column (C) of Table 1.4.

[63]The reason why we used the CVs of applicants from a different city in the survey is that it could reduce the probability that the surveyed teachers were able to infer the identities of the virtual schools or the virtual applicants. We randomly distributed the folders of three different schools so as to reduce the probability that teachers discussed with peers sitting next to them during the survey and provided collective answers.

1.4.3 Inference of Fairness Views and Past Perceived Unfairness

In this sub-section, I explain in detail how I use the survey to infer each teacher's fairness preferences and construct measures of their perceived unfairness in past promotions by contrasting their "ideal" promotion results with what actually took place in their schools.

Matching respondents and teachers by professional rank and VA.

As the survey covers only a small subset of teachers in the sample, I match respondents in the survey and teachers in the administrative data on observable characteristics so as to assign the revealed preferences of the former to the matched ones in the latter. Specifically, I divide the respondents into groups by their professional ranks (junior, middle and senior) and whether their individual-school-specific VA is above the within-rank median in their schools ($3 \times 2 = 6$ groups, noted $\mathbb{F}$). I group the teacher-year observations in the administrative data using the same method: for teacher i in year t, her group identity $f(i,t)$ is determined by the professional rank she holds in year t and the within-rank ranking of her school-specific teaching quality, $\text{VA}_{i,h(i,t)}$.

Estimating respondents' preferences.

I estimate the preferences of respondents in each group $f \in \mathbb{F}$ separately, using a model that parallels the one used for the principals (Equation (1.5)). For virtual applicant l evaluated by survey respondent j' in group $f(j')$, I run:

$$\mathbb{E}\left[\text{Yes}_{j'l}\right] = F\left(\mathbf{X}_l \gamma^{f(j')} + \alpha_H^{f(j')}\text{HomeTie}_{j'l} + \alpha_C^{f(j')}\text{CollegeTie}_{j'l} + \beta^{f(j')}\text{Controls}_l\right). \quad (1.13)$$

where $\text{Yes}_{j'l} = 1$ if respondent j' picks applicant l as one that she thinks deserves promotion. In Controls_l I include a fixed effect for the virtual school, $\lambda_{h(l)}$, and the share of same-subject applicants, $\text{share}_{k(l),h(l)}$. I estimate a logit model and obtain the coefficient estimates $\left\{\left(\hat{\gamma}^f, \hat{\alpha}_H^f, \hat{\alpha}_C^f, \hat{\beta}^f\right)\right\}_{f\in\mathbb{F}}$.

Inferring perceived fair promotion results.

These group-specific coefficient estimates are used to infer teacher i's perceived promotability of actual applicant j in her school in year t.

$$\hat{\eta}_{ijt} = \mathbf{X}_{jt}\hat{\gamma}^{f(i,t)} + \hat{\alpha}_H^{f(i,t)}\text{HomeTie}_{ij} + \hat{\alpha}_C^{f(i,t)}\text{CollegeTie}_{ij} + \hat{\beta}_1^{f(i,t),share}\text{share}_{k(j),h(i,t),t}. \quad (1.14)$$

The inferred preferred promotion results in year t by teacher i under her fairness views, noted $\hat{\Omega}_{it}$, is defined as:

$$\text{Promote } j \text{ iff } \text{Ranking}_{h(i,t)t}\left(\hat{\eta}_{ijt}\right) \leq N_{h(i,t),t},$$

where N_{ht} is the number of actual promotees in school h in year t.

Applicant types by actual and perceived fair promotion results.

Contrasting the inferred perceived fair promotion results $\hat{\Omega}_{it}$ and the actual results $\Omega_{h(i,t),t}$, the applicants in year t can be grouped into 4 types in teacher i's views:

$$\begin{aligned} \mathbb{M} = \ & \{\text{Undeservingly Promoted, Deservingly Promoted,} \\ & \text{Deservingly Denied, Undeservingly Denied}\}, \end{aligned}$$

where

$$m\left(i,j,t\right) = \begin{cases} \text{Undeservingly Promoted,} & \text{if Ranking}_{h(i,t),t}(\hat{\eta}_{ijt}) > N_{h(i,t),t} \text{ \& Promoted}_{jt} = 1, \\ \text{Deservingly Promoted,} & \text{if Ranking}_{h(i,t),t}(\hat{\eta}_{ijt}) \leq N_{h(i,t),t} \text{ \& Promoted}_{jt} = 1, \\ \text{Deservingly Denied,} & \text{if Ranking}_{h(i,t),t}(\hat{\eta}_{ijt}) > N_{h(i,t),t} \text{ \& Promoted}_{jt} = 0, \\ \text{Undeservingly Denied,} & \text{if Ranking}_{h(i,t),t}(\hat{\eta}_{ijt}) \leq N_{h(i,t),t} \text{ \& Promoted}_{jt} = 0. \end{cases} \quad (1.15)$$

Of the 22% of applicants who are promoted, 21.8% are perceived as undeservingly promoted (see Table 1.11).

Measuring perceived unfairness.

Constructing a straightforward scalar measure to characterize the deviation of the actual promotion results from teacher i's perceived fair ones ($\Delta\hat{\Omega}_{it} := \hat{\Omega}_{it} - \Omega_{h(i,t)}$), is helpful for both the empirical estimation and the interpretation of the impacts of perceived unfairness in promotions. I use the fraction of promotees regarded as undeserving by teacher i in her school in year t:

$$\text{Undeserving\%}_{it} = \frac{\#\text{ Undeservingly Promoted}_{h(i,t)t}}{\#\text{ Promoted}_{h(i,t)t}}. \tag{1.16}$$

The sample mean of this perceived unfairness measure is 22.1% and its standard deviation is 12.8%.

1.4.4 What Does an Unfair Promotion Look Like?

This sub-section describes how the surveyed teachers evaluate the virtual applicants, as opposed to the principals, in deciding who should receive promotion, which also speaks to the characteristics of a promotion which violates the teachers' fairness preferences. Similar to the heterogeneity analysis in Sub-section 1.3.5, I estimate the following equation:

$$\mathbb{E}\left[\text{Yes}_{j'l}\right] = F\left(\sum_{g\in\{\mathbb{G},Tie\}} \varrho_g \hat{\eta}_l^{P,g} + \beta^{\varrho}\text{Controls}_l\right) \tag{1.17}$$

where j' denotes a survey respondent, variables $\left\{\hat{\eta}_l^{P,g}\right\}_{g\in\{\mathbb{G},Tie\}}$ represent the categorical promotability indices of virtual applicant l in the principals' opinions, constructed using Definition (1.7). The included control variables are the same as in Equation (1.13). The relative sizes of coefficients $\{\varrho_g\}_{g\in\{\mathbb{G},Tie\}}$ compared to each other show how much the surveyed teachers value certain applicant characteristics differentially more than the principals: if $\varrho_g > \varrho_{g'}$, then the teachers emphasize characteristics in category g more than category g', compared to the principals.[64]

[64] Statistically, I test whether the coefficient estimate $\hat{\varrho}_g$ is significantly different from the mean $\frac{1}{7}\sum_{g\in\{\mathbb{G},Tie\}}\hat{\varrho}_g$.

The estimation results are reported in Table 1.12, for the 6 groups of respondents respectively in Columns (1) to (6) and for the whole sample in Column (7). We can see that the surveyed teachers value teaching awards around 50% more and publications 20% less, and most importantly, social ties over 70% less than principals. These patterns are fairly homogeneous across all respondent groups. The finding suggests that the major source of difference between the teachers' and the principals' promotability notions lies in that the teachers do not take into account social connections like the principals do, and they stress teaching performance more heavily.[65]

From an alternative perspective, I compare the four types of applicants by their actual promotion results and perceived deservingness (see Definition (1.15)) in terms of their teaching quality and the probability that they share social ties with the principal. The results are reported in Table 1.13. Among promoted applicants, the perceived undeserving ones have a very high probability of being socially ties to the principal (80%), compared to the deserving group (30%). On the contrary, only around 3% of denied applicants who are viewed as treated unjustifiably are socially connected to their principals. In addition, the undeservingly promoted applicants are also significantly lower in their school-specific VA in the current school than other promotees, and the undeservingly denied applicants have on average higher VA compared to their deservingly unpromoted counterparts.

In summary, the perceived undeservingly promoted applicants consist mainly of those who have social ties to the principal but low teaching quality, a direct consequence of the principals' favoritism.

[65] As the survey respondents do not have real interactions with the virtual applicants, by using the survey results to infer the teachers' fairness notions I might underestimate the importance of social ties between two fellow teachers in the real workplace. This can cause the perceived unfairness measure, Undeserving%, to overstate the extent to which those who are socially tied to the principal disagree with the actual promotion results, and understate the extent of unfairness perceived by those who are socially tied not to the principal but to some peer applicants. In fact, when decomposing the variable Undeserving$\%_{it}$, principal-school effects account for 21% of the total variance, and teacher-principal-school fixed effects 27%, implying that there is an average perceived unfairness level which is specific to each teacher under a given principal. To address this potential measurement bias, I include in the empirical analysis in Section 1.5 the teacher-principal fixed effects.

1.5 Impacts of Perceived Promotion Unfairness

I now present this paper's most important findings: how the teachers' perceived unfairness in the promotion results, resulting from favoritism by their school principals, affects their performance at work and the overall efficiency of schools. I first discuss the incentive effects of unfairness on the teaching effort and the job quitting probability of both the applicants themselves (Sub-section 1.5.1) and their non-applicant colleagues (Sub-section 1.5.2). Then I proceed to the overall impacts on the worker turnover patterns and the productivity and of schools (Sub-section 1.5.3). Throughout this section, I use the subsample consisting of the post-transparency-reform years, as the teachers' unfairness perceptions are formed based on the information provided by the applicants' CVs.

1.5.1 Own Incentive Effects on Applicants

I first study the impacts of unfairness on the applicants themselves, whose self-interest is directly affected by the promotion results.

1.5.1.1 Individual Effects of Personal Promotion Results

I perform event studies of promotion success or failure on the performance of the applicants who either think they are fairly treated or not. Specifically, I run:

$$\begin{aligned} Y_{j,t+s} = \; & \textstyle\sum_{\tau=-3}^{2} \sum_{m\in\mathbb{M}} \varphi_{m\tau}\mathbb{I}\left[m\left(j,j,t\right)=m, s=\tau\right] + g^{\varphi}_{h(j,t)}\left(t+s\right) + \mathbf{Z}_{j,t+s}\beta^{\varphi}_{j} \\ & +\lambda^{\varphi}_{j,P(j,t)} + \lambda^{\varphi}_{j,P(j,t+s)} + \varepsilon^{\varphi}_{j,t+s}. \end{aligned} \tag{1.18}$$

where Y is an outcome variable of interest, $m \in \mathbb{M}$ indicates the type of applicants by their promotion results and self-perceived deservingness (see Definition (1.15)), and $g^{\varphi}_{h}\left(t+s\right)$ is school-specific time trends. The fixed effect for applicant j and the current principal in the school, $\lambda^{\varphi}_{j,P(j,t+s)}$, absorbs the teacher-principal-specific component in Y, and the fixed effect for applicant j and the principal who decides her promotion result, $\lambda^{\varphi}_{j,P(j,t)}$, takes care of a potential measurement bias (see note [65]). I also include a set of variables of job characteristics:

$$\mathbf{Z}_{j,t+s} = \left\{ \lambda_{g(j,t+s)}, \text{workload}_{j,t+s}, \text{headteacher}_{j,t+s}, \text{break}_{j,t+s}, \bar{A}^{j(c,k,t+s)=j}_{c,k,t+s-1} \right\}, \tag{1.19}$$

where $\lambda_{g(j,t+s)}$ is a dummy indicating the grade(s) applicant j teaches in year $t+s$, $\text{workload}_{j,t+s}$ is the number of sessions taught by j per week in year $t+s$, $\text{headteacher}_{j,t+s} = 1$ if j is a class head teacher in year $t+s$, $\text{break}_{j,t+s} = 1$ if j leaves any of the current classes she teaches in year $t+s-1$ before graduation,[66] and $\bar{A}^{j(c,k,t+s)=j}_{c,k,t+s-1}$ is the average end-of-last-year test scores of the students taught by j in year $t+s$. I allow individual-specific coefficients on $\mathbf{Z}$ to account for possible complementarities between teachers and job characteristics. I include only the applicant-year observations where the applicant works in the same school as in the application year, and for a denied applicant the years in which she has not been subsequently promoted. The coefficients $\{\varphi_{m\tau}\}_{m,\tau}$ represent the adjusted mean levels of the outcome variable of applicant group $m \in \mathbb{M}$ in year τ relative to the application year.

Teaching effort.

Figure 1.4 plots the estimation results where the outcome variable is teaching effort, proxied by VA ($Y_{j,t+s} = \text{VA}_{j,t+s}$). The horizontal axis displays the years relative to the application year, and on the vertical axis the coefficient estimates $\left\{\hat{\phi}_{q\tau}\right\}_{q,\tau}$ are plotted. We can see that those who think they are deservingly promoted lower their VA by around 0.26 standard deviations in the first year after promotion,[67] and the effect persists over at least three years while slowly fading out; those who think they are fairly denied promotion increase their VA by 0.41 standard deviations in the following year,[68] and continue working hard over the next few years. I do not find statistically significant changes in the teaching effort exerted by applicants who think they are unfairly evaluated, either favored or discriminated against.

[66]Mathematically, $\text{break}_{i,t+s} = 0$ iff $\forall (c, t+s)$ s.t. $j(c,k,t+s-1) = i$ and $g(c,t+s-1)$ is not the graduation grade of subject k of class c, we have $j(c,k,t+s) = i$.

[67]The within-applicant-principal-school standard deviation in VA is 0.621. $\frac{-0.163}{0.621} \approx -0.26$, see Panel (A), Row (1), Column (3) of Table A5.

[68] $\frac{-0.254}{0.621} \approx -0.41$, see Panel (A), Row (3), Column (3) of Table A5.

Compared to a fair treatment, the incentive effect of a self-perceived favor is positive, while a self-perceived wrong harms productivity.

I provide one of the possible mechanisms that can explain the findings. Among the applicants who think they are fairly treated, those who are promoted might lose to some extent the incentives to work hard as they have already completed the final step in the salary-determining evaluation system, and those who fail might be motivated to work harder upon receiving the signal that they are not qualified for promotion yet. On top of this, the promotees who think they are favored might reciprocate by exerting higher effort, and the denied applicants who think they are mistreated might reduce effort either out of an adverse morale effect or as a punishment of the principal to signal that her subjective evaluation is unacceptable and needs to be corrected in the future.

Job quitting.

Another consequence I study is whether the applicants quit their jobs ($Y_{j,t+s} = \text{Leave}_{j,t+s}$), the results on which are plotted in Figure 1.5.[69] It is shown that compared to the promoted, denied applicants are around 50% more likely to quit after a promotion failure; and those who think they are unfairly denied are even more likely to leave, with a probability twice as large as that of the promotees. As the unfairly denied applicants have higher teaching quality than the fairly denied ones on average (see Table 1.13), unfair promotion can lead a school to lose good teachers.

1.5.1.2 Robustness Checks

For Equation (1.18) to consistently estimate the incentive effects of (un)fair personal promotion results on different types of applicants, the underlying assumption is that there are no unobservable promotion-associated changes in their job characteristics or working environment that can affect the applicants' teaching performance or their incentives to quit. I cannot directly test for this, but examining whether observable job characteristics change

[69]Job quitting is defined as leaving the current school before retirement age.

after a promotion success or failure can provide suggestive evidence on possible unobservable confounders: if we do not observe stylized changes in the former, issues with the latter might be less severe of a concern. I replace the outcome variable with the job characteristics variables in $\mathbf{Z}_{j,t+s}$ (see Definition (1.19)) and re-estimate Equation (1.18).

The results are shown in Figure A8. I do not see significant changes in teaching workload, probability of being separated from the current classes, or students' prior test scores of an applicant after she is promoted or denied, either fairly or unfairly. The only significant change is that the newly promoted senior teachers are less likely to hold a class head teacher position than before (see Panel (B)), and this change is homogeneous across deserving and undeserving promotees. If anything this should alleviate their burden and relax their time constraints for teaching courses, nonetheless I find on average negative effects of receiving the senior-rank title on their VA.

1.5.1.3 Average Effects of School-Level Unfairness

From the perspective of a school, it is the *average* effects of school-level extent of unfairness on the biasedly-treated teachers, rather than the *individual* effects of bias on the favored and/or discriminated against teachers, that matter. As the number promotion slots are fixed, the number of victims and beneficiaries of discrimination is equal; and if they respond in opposite directions of the same magnitude, school-level bias does not harm the average productivity of applicants. To test for the average incentive effects of biased promotion results on the promotion applicants, I estimate the following equation:

$$\begin{aligned} Y_{i,t+s} = \; & \sum_{\tau=-3}^{2} \theta_\tau \text{Undeserving\%}_{it} \times \mathbb{I}\left[s=\tau\right] + \sigma^\theta Y_{i,t-1} + g^\theta_{h(i,t)}\left(t+s\right) + \mathbf{Z}_{i,t+s}\beta^\theta_i \\ & + \lambda^\theta_{i,P(i,t)} + \lambda^\theta_{i,P(i,t+s)} + \varepsilon^\theta_{i,t+s}, \end{aligned} \tag{1.20}$$

$\text{Undeserving\%}_{it}$ is the fraction of promotees perceived as undeserving by applicant i in her school in her application year t (see Definition (1.16)). The set of control variables are the same as in Equation (1.18), except that I also add the lagged outcome variable when estimating the impacts on VA. Coefficients $\{\theta_\tau\}_\tau$ represent the average effects of the applicants' perceived school-level unfairness on their performance in the neighboring years. θ_0 shows the immediate impacts of perceived unfairness on the teachers' performance in the same school year, θ_1, θ_2 depict persistence of these impacts, and θ_{-3}, θ_{-2} are used as placebo tests.[70] I pool all lagged and future observations relative to a promotion year in one single regression to impose common coefficients on the control variables.

Results are plotted in Figures 1.6 and 1.7. We can see that school-level unfairness in promotion results does not affect the promotion applicants' average VA or job quitting rates. This finding implies that if it is only the applicants' productivity that is affected by biased promotion results, it does not hurt for the principal to exercise favoritism, as the the gain from the favored applicants offsets the loss from the biased against ones.[71]

1.5.2 Spillover Incentive Effects on Non-Applicant Teachers

Given the zero average productivity effect of school-level promotion unfairness on promotion applicants, an important question is whether and how the non-applicant peers in each school, accounting for around 78% of the whole teaching staff, respond to promotion results they view as unfair. Such potential *spillover* effects determine whether biased promotions have school-wide consequences.

1.5.2.1 Main Results

I re-estimate Equation (1.20) on the teacher-year observations where the teacher is a non-applicant in the reference promotion year and currently works in the same school as in the reference year.

Teaching effort.

[70] Notice that as the lagged outcome variable $Y_{i,t-1}$ is controlled for as a covariate, $\theta_{-1} = 0$ by construction.

[71] The effect on job quitting is adverse at the school level though, even if the average quitting rate is not affected. This is because higher-quality denied applicants are more likely to leave and lower quality promoted applicants are more likely to stay.

The blue dots in Figure 1.8 plot the estimated effects of perceived promotion unfairness on the non-applicant peers' VA in teaching. Working under the same principal, a 10 percentage point increase in the fraction of undeserving promotees decreases a non-applicant teacher's VA in the same year by around 0.15 standard deviations on average.[72] Contrasted against the ideal case where there exists no perceived unfairness, the average level of existing unfairness leads to 0.35-standard-deviation lower VA of each non-applicant teacher.[73] The impact is persistent, with each teacher's VA level 0.16 and 0.09 standard deviations lower than in the no-bias scenario in the second and third following years.

There is no impact of perceived unfairness in future promotions on the teachers' current productivity, suggesting that the correlations between current unfairness and current (and future) VA of the non-applicant teachers are unlikely to result from smoothly-varying confounding determinants of students' test scores.

Job quitting.

The estimation results on quitting incentives are plotted in Figure 1.9. A 10 percentage point higher fraction of undeserving promotees as perceived by a non-applicant teacher leads to a 9% increase in the probability that she leaves the school in the same year, and a school-level promotion evaluation with average level of perceived unfairness makes non-applicant teachers 16% more likely to quit than a fair one.[74] Like teaching productivity, the effect on job quitting persists through the second year (12%) and the third year (8%).

Taken together, perceived unfairness in promotions has adverse spillover incentive effects on peer teachers in that it leads some of them to leave the school and lowers the productivity of those who stay.

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[72]The within-teacher-principal-school standard deviation in VA is 0.61. $\frac{0.1\times(-0.938)}{0.61} \approx -0.15$, see Panel (A), Column (3) of Table A7.

[73]The average level of Undeserving$\%_{it}$ is 0.22.

[74]The mean probability that a teacher leaves her current school is 0.052 per year. $\frac{0.10\times 0.0456}{0.052} \approx 0.09$ for a 10 percentage points increase in Undeserving%, and $\frac{0.22\times 0.0456}{0.052} \approx 0.16$ for average level of Undeserving% compared to zero Undeserving%. See Panel (C), Column (3) of Table A7.

1.5.2.2 Robustness Checks

To interpret the estimates in the previous sub-section as the incentive effects of perceived promotion unfairness on non-applicant teachers, it is required that the extent to which they think a promotion is unfair does not proxy or lead to unobservable changes in individual job assignments and school-wide working environment that can affect their productivity and/or their incentive to quit.

One concern is that promotions based on favoritism might lead to less productive job assignments.[75] It is possible that the rank a teacher holds affects her class assignment and as a result the class assignment of other teachers, given the set of classes fixed. If, for instance, there are complementarities between students' ability and teacher's quality in the production of test scores, and promoted teachers are assigned to classes of better-achieving students, then promoting low-quality teachers is costly due to the mis-matching between high- (low-) quality teachers and low- (high-) ability students. To address this, I first show that observable patterns of matching between job characteristics and differently-ranked non-applicant teachers do not correlate with perceived promotion unfairness. The estimation results of Equation (1.20) using $Y_{i,t+s} \in \mathbf{Z}_{i,t+s}$ (see Definition (1.19)) as the outcome variables for the senior, the middle and the junior-ranked teachers are displayed in Figure A9, where I do not find noticeable correlations between the teachers' perceived unfairness and their workload, their probability of being assigned to new classes unexpectedly, or the average prior test scores of their students. In addition, in the regression of VA on perceived unfairness I also limit the sample to teacher-year observations where the teacher teaches exactly the same classes as in the year prior to the reference promotion year, ruling out cases in which a teacher is either asked to leave her current classes, or goes back to grade 1 and teach a new cohort when she is potentially assigned to unobservably better or worse classes.[76] The estimated effect using this no-change subsample, plotted using the red dots in Figure 1.8, is

[75] Prendergast and Topel (1996) model this explicitly as one source of the costs of favoritism in subjective evaluations.

[76] Mathematically, the subsample satisfies that $C_{i,t+s} = C_{i,t-1}$, where $C_{it} := \{c | j(c, k(i), t) = i\}$.

very similar to the one using the whole sample of non-applicants.

Another concern is that unfair promotion might result in school-wide productivity changes that can affect individual teacher's VA measures even if they work equally hard. Although it is impossible for me to exhaust all such possibilities, I discuss one plausible case explicitly. As the middle-level leader positions are primarily held by the senior-ranked teachers, if teachers' managerial ability is positively correlated with their teaching quality, then unfairly promoting low-VA teachers might result in a group of poor performing leaders, potentially affecting the efficiency of all teachers in the school.[77] However, as I focus on the short-run effects of a single promotion event, this is not an issue if the newly promoted teachers do not take up such managerial positions immediately. A middle-level leader position is accompanied by reduced teaching workload and consequent departure from (some of) the current classes, but I do not see such events take place for the promoted teachers with increased probabilities immediately after their promotion to the senior rank (see Panels (A) and (C) of Figure A8).

1.5.2.3 Evidence on Mechanisms

I first list several hypotheses explaining why unfair treatments by principals toward promotion applicants can reduce the incentives of non-applicant peers at work in the same school, and discuss their predictions regarding heterogeneities in the incentive effects (noted θ). Then I provide empirical evidence to assess how well each of these theories applies in the empirical context.

Theories.

It is worth pointing out that teacher-principal fixed effects are included in all of the estimation specifications on the effects of perceived promotion unfairness, which means that the estimates do not capture the influence of any (undesirable) individual characteristics of

[77]This hypothesis is the opposite to the one discussed in the second identification threat in the estimation of favoritism in Sub-section 1.3.3.

a principal associated with her favoritism as long as these characteristics are stable and fully revealed to the teachers over the whole time period that she runs the school.

I) Learning about principal.

Teachers in a school learn about their principal's preferences or management styles as her promotion decisions are revealed each year, and they might react accordingly in their work performance. A universal prediction of such learning theories is that teachers update their beliefs about the principal more in the early stage of their principal's term when their prior beliefs are imprecise, as predicted by a Bayesian model, resulting in larger changes in their behavior.

Prediction I.1): $|\theta|$ decreases with the length of time that the promotion-evaluating principal has stayed in the school.

Moreover, if it is the principal's revealed traits that matter to teachers, one might expect the effects to disappear, or at least fade out rapidly, if the principal has left the school.

Prediction I.2): $|\theta_{\text{Same Principal}}| > |\theta_{\text{Changed Principal}}| \geq 0$.

Teachers can make inferences from unfair promotion results about their principal, either as a promotion evaluator who can affect their (future) promotion prospects and consequently their pay, or as a manager who lead and interact with them inmore general circumstances (other than promotion evaluations).

Ia) Principal as a biased promotion evaluator.

Biased promotion results might change the teachers' expectations about how the current principal will evaluate applicants in the future, which might affect the behavior of those who plan to apply in the next few years under the same principal. A straightforward prediction of this theory is that peer teachers who are more likely to go through promotion evaluation(s) by the current principal are more heavily affected. Middle-ranked teachers are the prospective applicants in the coming years, as the junior-ranked need to wait at least 4 years to be eligible to apply,[78] and the senior-ranked have already successfully passed the evaluation system. This prediction can be stated as the following:

Prediction Ia.1): $|\theta_{\text{Middle}}| > |\theta_{\text{Junior}}| \geq |\theta_{\text{Senior}}| = 0$.

Teachers know that principals have access to application profiles and can relatively well observe the qualification of applicants. When observing a low-quality but tied-to-principal applicant receives promotion while a high-quality but untied-to-principal one gets denied, a peer teacher in the school might conclude that the principal sets a lower promotion requirement for his connected subordinates than others. This might lead non-top-quality prospective applicants (middle-ranked teachers) without social ties to shirk or leave, if they think they cannot meet the discriminatively high promotion requirements even if they work hard. Reduced effort could also arise when the teachers would like to signal the principal that his current bias is unacceptable and should be corrected in future evaluations, if they have bargaining power in the school. Meanwhile, average-quality teachers with social ties might choose to stay and work harder, if they think they face a favorably low threshold for promotion that is approachable through hard work. If this is the case, one might expect:

Prediction Ia.2): $\theta^{\text{VA}}_{\text{Untied, Middle}} < 0 < \theta^{\text{VA}}_{\text{Tied, Middle}}$, and $\theta^{\text{Leave}}_{\text{Untied, Middle}} > 0 > \theta^{\text{Leave}}_{\text{Tied, Middle}}$.

On the other hand, the double standards might lower the effort of upper-middle-quality teachers with social ties and induce them to stay, if their interpretation of the principal's biased promotion decision is that they need not work so hard as to qualify for promotion, given their quality and current performance. Meanwhile, non-top untied teachers might be motivated to work harder in order to meet the higher promotion requirements, and leave if they think their prospects of success are poor. In this case, the difference between socially tied and untied prospective applicants should satisfy:

Prediction Ia.3): $\theta^{\text{VA}}_{\text{Untied, Middle}} > 0 > \theta^{\text{VA}}_{\text{Tied, Middle}}$, and $\theta^{\text{Leave}}_{\text{Untied, Middle}} > 0 > \theta^{\text{Leave}}_{\text{Tied, Middle}}$.

[78] They usually wait much longer, as the mean time interval between receiving the middle rank and first applying for the senior rank is 7.8 years, longer than the average length of a principal's term (6.13 years).

Predictions *Ia.2)* and *Ia.3)* are the same in terms of job quitting and the opposite of each other in terms of effort provision.

Ib) Principal as an undesirable manager.

If teachers learn from unfair promotion results that the principal is discriminating in favor of his friends against those who are not socially connected to him, they might expect him to behave likewise on other occasions in the workplace, even if that has not yet happened. Under this circumstance, their working morale could be eroded, resulting in underperformance and quitting.[79] If this is the case, one might expect that the discriminated against group should be (more) affected than the favored group (see e.g. Cohn et al., 2014; Glover et al., 2017; Breza et al., 2017).[80]

Prediction Ib): $\theta^{\text{VA}}_{\text{Untied}} < \theta^{\text{VA}}_{\text{Tied}} \leq 0$, and $\theta^{\text{Leave}}_{\text{Untied}} > \theta^{\text{Leave}}_{\text{Tied}} \geq 0$.

II) Fairness norms and social preferences for peer workers.

It is also likely that it is their peers being unfairly treated by the principal, rather than the principal showing bias, that the teachers care about and negatively respond to. In this hypothesis, the morale effect does not subside as teachers know their principal better, as long as the principal keeps delivering unfair promotion decisions in each year; neither does the effect disappear as long as the teachers still work together with colleagues who are mis-evaluated and consequently suffer from persistently lower earnings, even if the perpetrating principal has been replaced by a new one.

Prediction II.1): $|\theta|$ does not decrease with the length of time that the promotion-decision-making principal has stayed in the school.

[79] See for example Arasli and Tumer (2008) who show that nepotism, favoritism and cronyism create job stress in the workplace and this increases dissatisfaction of the staff about their organizations using a survey on employees in the Cyprus banking industry.

[80] Cohn et al. (2014) conduct a field experiment in a firm that hires workers for one-off sales promotions and find that a wage cut of the same size decreases the performance of workers whose group members do not receive the wage cut differentially more than those whose group members' wages are also lowered. Glover et al. (2017) show that manager bias negatively affects minority job performance in a French grocery store chain. Breza et al. (2017) show in a field experiment in India that when coworkers' productivity is difficult to observe, pay inequality reduces the output and lowers the attendance of the lower-paid, given absolute pay fixed.

Prediction II.2): $|\theta_{\text{Same Principal}}| = |\theta_{\text{Changed Principal}}|$.

Moreover, if teachers care about peer workers rather than the principal, being socially tied to the principal would not make unfairness more acceptable.

Prediction II.3): $\theta^{\text{VA}}_{\text{Untied}} = \theta^{\text{VA}}_{\text{Tied}} \leq 0$, and $\theta^{\text{Leave}}_{\text{Untied}} = \theta^{\text{Leave}}_{\text{Tied}} \geq 0$.

Results.

Teaching effort.

Several findings regarding the heterogeneities in the estimated effects of perceived promotion unfairness on teachers' productivity, $\hat{\theta}^{\text{VA}}$, are listed below:

(i) $|\hat{\theta}^{\text{VA}}|$ does not decrease as the principal stays longer in the school (see Panel (A) of Table 1.14), supporting Prediction *II.1)* against Prediction *I.1)*;

(ii) $|\hat{\theta}^{\text{VA}}_{\text{Same Principal}}| \approx |\hat{\theta}^{\text{VA}}_{\text{Changed Principal}}|$ (see Figure 1.10), supporting Prediction *II.2)* against Prediction *I.2)*;

(iii) $|\hat{\theta}^{\text{VA}}_{\text{Senior}}| > |\hat{\theta}^{\text{VA}}_{\text{Junior}}| > |\hat{\theta}^{\text{VA}}_{\text{Middle}}|$ (see Figure 1.12), inconsistent with Prediction *Ia.1)*;

(iv) $\hat{\theta}^{\text{VA}}_{\text{Untied}} \approx \hat{\theta}^{\text{VA}}_{\text{Tied}} < 0$ (see Panel (A) of Figure 1.14), supporting Prediction *II.3)* against Prediction *Ib)*;

(v) $\hat{\theta}^{\text{VA}}_{\text{Untied, Middle}} \approx \hat{\theta}^{\text{VA}}_{\text{Tied, Middle}} < 0$ (see Panel (B) of Figure 1.14), in line with Predictions *Ib)* and inconsistent with Predictions *Ia.2)* and *Ia.3)*.

In summary, Theory *II)* explains the empirical patterns the best: the adverse spillover incentive effect of perceived promotion unfairness on teachers' productivity appears to be driven primarily by the teachers' social preferences for their colleagues: they care about them their peers fairly treated in promotion evaluations and are demoralized when peers suffer unfairness

I provide further suggestive evidence for this proposed mechanism. First, teachers who are likely to have more frequent interactions with the "victims" (that is, the undeservingly denied applicants) tend to react more negatively (see Figure 1.16). Such interactions between a non-applicant and a victim include teaching the same cohort(s) (see Panel (A)) or the same subject (see Panel (B)), and sharing hometown or college ties with each other (see Panel (C)). Moreover, past treatment received by the already-promoted senior-ranked teachers play a role in determining the magnitude of their productivity response: compared to a senior-ranked teacher who was favored in her own past promotion, one who views herself as previously fairly promoted is less tolerant of current unfairness inflicted upon their peers and lowers effort even more (see Figure 1.17).[81]

Job quitting.

In terms of the effects on quitting behavior, $\hat{\theta}^{\text{Leave}}$, the following patterns are documented:

(i) $|\hat{\theta}^{\text{Leave}}|$ decreases as the principal stays longer in the school (see Panel (B) of Table 1.14), supporting Prediction *I.1)* against Prediction *II.1)*;

(ii) $|\hat{\theta}^{\text{Leave}}_{\text{Same Principal}}| > |\hat{\theta}^{\text{Leave}}_{\text{Changed Principal}}|$ (see Figure 1.11), supporting Prediction *I.2)* against Prediction *II.2)*;

(iii) $|\hat{\theta}^{\text{Leave}}_{\text{Senior}}| < |\hat{\theta}^{\text{Leave}}_{\text{Junior}}| < |\hat{\theta}^{\text{Leave}}_{\text{Middle}}|$ (see Figure 1.13), consistent with Prediction *Ia.1)*;

(iv) $\hat{\theta}^{\text{Leave}}_{\text{Untied}} \approx \hat{\theta}^{\text{Leave}}_{\text{Tied}} > 0$ (see Panel (A) of Figure 1.15), supporting Prediction *II.3)* against Prediction *Ib)*;

(v) $\hat{\theta}^{\text{Leave}}_{\text{Untied, Middle}} > \hat{\theta}^{\text{Leave}}_{\text{Tied, Middle}} > 0$ (see Panel (B) of Figure 1.15), consistent with Predictions *Ia.2)* and *Ia.3)*.

The heterogeneity patterns in the job quitting effect are the most consistent with Theory *Ia)*: when the principal's friends are chosen over other applicants with better performance,

[81] The adverse effect of unfairness on non-applicant teachers is mostly likely driven by pure social preferences (or altruism) toward co-workers, rather than by social pressure or reputation concerns under which workers might be selfish but able to show “support" for each other through repeated interaction, as there are no externalities among workers due to either the production function (see e.g. Mas and Moretti, 2009) or the compensation scheme (see e.g. Bandiera et al., 2005), and it is conceptually not very likely that not lowering one's performance in response to unfairness suffered by co-workers is viewed as a bad behavior towards them.

prospective applicants who are not socially tied to the principal might choose to leave as they learn that their good work will likely not be rewarded in future promotion decisions made by the principal. Figure 1.18 shows that among the prospective applicants, those of higher quality are even more likely to leave, an undesirable consequence from the perspective of the current school.

Summarizing the above results, I find the strongest support for the teachers' productivity effect to work through social preferences for their peers at work, under which unfair treatment suffered by peers impose negative morale costs. This is consistent with previous findings on the effects of social incentives on productivity generated by preferences for peers (e.g. Jones and Kato, 1995; Hamilton et al., 2003; Bandiera et al., 2005, 2010; Breza et al., 2017) and the impacts of (the violations of) fairness norms on effort provision (e.g. Krueger and Mas, 2004; Bracha et al., 2015; Breza et al., 2017; Dube et al., 2018b). The quitting effect seems more related to self-interest concerns: high-quality teachers who belong to the biased against group as the unfairly treated peers tend to leave as they learn that good teaching performance cannot better their future promotion prospects in current school.

1.5.3 Impacts on School-Wide Performance

Apart from studying how teachers' incentives are affected, assessing the aggregate impacts of unfair promotions on school performance is by itself important, as in general it is the overall performance of an organization that its shareholders and/or the market value. In this sub-section, I study the human capital, productivity and school market consequences of principals' unfair promotion decisions.

1.5.3.1 Teacher Turnover

As shown in Sub-sections 1.5.1 and 1.5.2, both the own and the spillover incentive effects indicate that unfairness in promotions leads good teachers to leave. However, how detrimental the overall impact is to a school depends on whether the quitters can be replaced with high quality new hires.

Turnover teachers can be divided into 3 groups:

$$\mathbb{O} := \{\text{Retired teachers, Job quitters, New hires}\}.$$

I first examine how the size and the average quality of these turnover workers in a school changes in each year in response to the school-year average perceived promotion unfairness, Undeserving$\%_{ht}$.[82] Specifically, I run:

$$Y_{h,t+s} = \sum_{\tau=-3}^{2} \vartheta_\tau \text{Undeserving}\%_{ht} \times \mathbb{I}\left[s=\tau\right] + \sigma^{\vartheta} Y_{h,t-1} + g_h^{\vartheta}\left(t+s\right) + \lambda^{\vartheta}_{P(h,t),h} + \varepsilon^{\vartheta}_{ht}. \quad (1.21)$$

where $Y_{h,t}$ represents school-level measures of turnover and includes: i) the number of teachers in turnover group $o \in \mathbb{O}$, noted N^o_{ht}; ii) the average individual-school-specific VA of these groups, noted VA^o_{ht};[83] and iii) the school-level flow in total VA:

$$\text{Change}^{VA}_{ht} := N^{\text{New}}_{ht}\text{VA}^{\text{New}}_{ht} - N^{\text{Retired}}_{ht}\text{VA}^{\text{Retired}}_{ht} - N^{\text{Quit}}_{ht}\text{VA}^{\text{Quit}}_{ht}.$$

I control for the lagged outcome variable, school-specific time trends and principal-school fixed effects and cluster standard errors at the school level.

The estimation results are plotted in Figure 1.19. From Panel (A) we can see that perceived promotion unfairness does not affect the number of retiring teachers,[84] while the number of job quitters increases after unfair promotions take place under a given principal in a school, resulting in more new hires to fill the vacancies. A school of average promotion unfairness loses 0.5 more teachers than when its principal promotes applicants fairly.[85]. Panel (B) shows the impacts on the average individual-school-specific VA of the turnover teachers. Again, although unfair promotion does not correlate with the teaching quality of the retiring

[82] Undeserving$\%_{ht} := \overline{\text{Undeserving}\%_{it}}^{h(i,t)=h}$.
[83] $\text{VA}^o_{ht} := \overline{\text{VA}_{ih}}^{o(i,t)=0,h(i,t)=h}$.
[84]The retirement ages are fixed for public employees in China: 60 for male and 55 for female.
[85] Average level of Undeserving$\%_{ht}$ is 0.22. $0.22 \times 2.21 \approx 0.5$, see Panel (A), Row (2), Column (3) of Table A14. The average number of job quitters per school-year is 6.3 (SD=2.0).

group, it leads high-quality teachers to leave the school, and attracts new workers who will later prove to have if anything lower teaching performance in the school. Compared to a perceived fair one, an average promotion decision made by a principal in a year during her term at a school leads teachers of around 0.2 standard deviations higher individual-school-specific VA to quit.[86] Unsurprisingly there is a negative effect of unfair promotion on the school-level change in total teacher VA in the following years (see Panel (C)).

1.5.3.2 Students' Test Scores

To investigate possible effects of unfairness in teacher promotions on the test scores of students in a school, I estimate the following equation:

$$Y_{c,k,t+s} = \sum_{\tau=-3}^{3} \vartheta_\tau \text{Undeserving\%}_{h(c),t} \times \mathbb{I}\left[s = \tau\right] + g^{\vartheta}_{h(c)}\left(t+s\right) + \lambda^{\vartheta}_{P(c,t),h(c)} + \varepsilon^{\vartheta}_{c,k,t+s}, \quad (1.22)$$

where $Y_{c,k,t+s}$ is class-subject-year level test score measures.

Annual productivity.

I first consider a crude VA measure:

$$Y_{c,k,t+s} := A_{c,k,t+s} - A_{c,k,t+s-1},$$

where A_{ckt} is the end-of-year-t average test scores of class c in subject k. Unadjusted for assignment of classes to teachers within a school, this variable measures the school-wide average productivity in a year rather than the teachers' individual effort provision, capturing possible mismatching between the students and the teachers. The results are plotted in Figure 1.20. Under a given principal, unfair promotions on average lower the class-subject average test scores in the school by around 0.13 standard deviations in the same school year,[87] and this school-wide productivity impacts persist over time like the incentive effects on individual teachers.

[86] The standard deviation of teacher-school-specific VA is 0.56. $\frac{0.22\times0.560}{0.56} \approx 0.2$, see Panel (B), Row (2), Column (3) of Table A14.

[87] $-0.576 * 0.22 \approx 0.13$, see Panel (A), Column (4) of Table A15.

CEE scores of graduating cohort.

Now I switch the subject of analysis from a school-year to a school-cohort. The impacts of unfair promotion on the College Entrance Exams (CEE) scores ($Y_{c,k,t+s} = A^{\text{CEE}}_{c,k,t+s}$) of the graduating cohort ($g(c, t+s) = 3$) in a school is plotted using the red dots in Figure 1.21. The average cumulative influence of unfair promotion on the class-subject average CEE scores of a fully affected cohort is a decrease of 0.25 standard deviations.[88,89]

HEE scores of newly enrolled cohort.

By hurting the performance of the graduating cohorts, unfair promotion also makes a school less attractive to prospective students in a competitive school market and enroll new students ($g(c, t+s) = 1$) with lower High School Entrance Exams (HEE) scores ($Y_{c,k,t+s} = A^{\text{HEE}}_{c,k,t+s}$), as plotted using the blue dots in Figure 1.21.[90] This implies the overall influence imposed by unfair promotion on a school can be even more detrimental and lasting that shown in the productivity impacts alone: it lowers the average student quality of future school cohorts, and might lead to a vicious circle if parents respond to *absolute* school performance measures in making their school choices (see e.g. Black, 1999; Hastings and Weinstein, 2008).[91]

[88]A fully affected cohort is one which enrolls in the reference promotion year $\tau = 0$ and graduates in year $\tau = 2$. $0.22 * (-1.147) \approx 9.25$, see Panel (B), Column (6) of Table A15.

[89] In Chinese public high schools, CEE performance of students could be important for the school principals and the teachers: it is an important aspect in the local education bureau's evaluation of principals and its decision to move them to better schools or to promote them to higher positions in the public education system; it is also valued by school principals (of better schools) in recuiting new teachers, and by parents in choosing private tutors which pay relatively high rates.

[90]In the Chinese public high school system, the local bureau of education assigns enrollment quota to each school, therefore schools compete for *better* (rather than *more*) students.

[91] In Chinese public high schools, having better students could be beneficial for school principals, other school leaders and teachers for the following reasons. First, the enrollment quota for a school in each year is decided by the local bureau of education, with some slots that can be used by the school to enroll unqualified students at a much higher tuition fee ("sponsorship" fee) which is at the disposal of the principal and the other school leaders and oftern shared by them; as parents value high peer quality for their children, attracting high-scoring qualified students could also attract more and/or higher-willingness-to-pay sponsorship-fee students. Second, better incoming students imply higher-achieving graduates holding value-added fixed, which is beneficial for the school and the teachers as explained in note [89].
Black (1999) shows that parents are willing to pay more for US elementary schools that produce test scores by comparing the prices of houses located on attendance district boundaries within school district. Hastings and Weinstein (2008) show using a natural experiment and a field experiment that provided direct information on school test scores to lower-income families in a public school choice plan in the US significantly increases

As one might have expected from the negative incentive effects documented in Subsection 1.5.2, unfair promotion results as perceived by teachers in a school adversely affect the operation of the school as a whole: it will consequently lose good quality teachers while failing to replace them with equally able ones, produce lower test scores for the existing students, and become less attractive to high-quality prospective students in the school market.

1.6 Impacts of Transparency Reform

In Section 1.5, I held the policy environment constant in which the CVs of promotion applicants are disclosed to peer teachers within the school. I used the variation in the extent to which the promotion results are perceived as unfair by peer teachers, which is generated mainly from variation in the relative quality of the favored-by-principal applicants to their competitors, to estimate the detrimental effects of unfairness perceptions on the peer teachers' performance and the consequent productivity costs suffered by schools. In this section, I vary whether the transparency reform mandating the disclosure of applicant information is in effect, and study how that changes both the principals' and the teachers' behavior as well as school-wide output. I do this for two reasons. First, it provides empirical tests of a model (displayed in Appendix A3) featuring the key empirical findings regarding the preferences of the principal and the teachers presented in Sections 1.3 and 1.5, which can serve as further robustness checks for the findings themselves. Moreover, studying the welfare implications of the transparency reform points towards possible policy tools that can help address the adverse productivity consequences of favoritism in employee promotions.

1.6.1 Teachers' Response to Promotion Unfairness

Absent the transparency reform, peer teachers can not see the application CVs and have noisy beliefs about the applicants' quality. Therefore, when a principal unfairly promotes

the fraction of parents choosing higher-test-score schools.

her hometown or college fellows who are outperformed by other applicants not socially tied to her, teachers do not know for sure if unfairness exists: without a clear idea of the applicants' quality, they cannot rule out the possibility that the socially-tied promotees are actually better and more promotable. Teachers can only *infer* that the promotion results are unfair with a positive but smaller-than-one probability from the fact that the principal's friends get promoted while his non-friends do not. As a result, actual unfairness, or the teachers' perceived unfairness when they are able to observe the applicants' quality, should not have adversely affected their behavior as heavily before the transparency reform as after.[92]

I empirically test whether this is the case by performing the following difference-in-difference-in-differences estimation on the whole sample covering both the pre- and post-reform years, exploiting the different timing of the transparency reform in the 4 cities:

$$\begin{aligned} \mathrm{VA}_{it} = \quad & \theta^{Post}\mathrm{Post}_{r(i,t),t} \times \mathrm{Undeserving\%}_{it} + \theta^{Pre}\left(1 - \mathrm{Post}_{r(i,t),t}\right) \times \mathrm{Undeserving\%}_{it} \\ & + \pi^{\theta}\mathrm{Post}_{r(i,t),t} + \sigma^{\theta}\mathrm{VA}_{i,t-1} + g^{\theta}_{h(i,t)}(t) + \mathbf{Z}_{it}\beta^{\theta}_{i} + \lambda^{\theta}_{i,P(i,t)} + \varepsilon^{\theta}_{it}, \end{aligned} \tag{1.23}$$

where $\mathrm{Post}_{rt} = 1$ if the transparency reform is effective in city r in year t. Without loss of generality and for the sake of clarity, I only study the immediate VA effect on non-applicant teachers in the reference promotion year. The same set of controls as in the main estimation equation of the incentive effects (Equation (1.20)) are included. If the hypothesis stated above is true, we shall expect $\theta^{Post} < \theta^{Pre} < 0$.

The results are presented in Column (1) of Table 1.15. We can see that $\hat{\theta}^{Post} = -0.953$ (SE=0.029), which is similar to the estimate when using only the post-reform sample, and $\hat{\theta}^{Pre} = -0.673$ (SE=0.024). The policy change led teachers to adversely respond to principal's bias around 40% more harshly in their effort choices. This pre- and post-reform heterogeneity in the effect of promotion unfairness on teaching productivity shows that increasing the observability of unfairness makes it more costly in terms of the teachers' lowered performance, providing additional supportive evidence that the documented reduction in the

[92] This idea is formalized in Proposition 1 of the principal-agent model in Appendix A3.

non-applicant teachers' VA associated with biased promotions indeed comes from their perceptions of unfairness.

1.6.2 Extent of Unfairness in Promotions

As shown in the last sub-section, the productivity cost of unfairness becomes larger after the transparency reform, as teachers punish unfairness more when it can be precisely observed. A straightforward prediction is that principals become less biased after the reform if they care about the performance of their schools or how their subordinates view them.[93]

I test this hypothesis by investigating whether the policy change made principals less biased in favor of their hometown and college fellows and whether it lowered the probability of promotions being unfair. Specifically, I estimate a difference-in-difference-in-differences model on the principals' promotion decisions:

$$\begin{aligned}\mathbb{E}\left[\text{Promoted}_{jt}\right] = \quad & F(\mathbf{X}_{jt}\gamma^P + \alpha^{Post}\text{Post}_{r(j,t),t} \times \text{SocialTie}_{j,P(j,t)} \\ & +\alpha^{Pre}\left(1 - \text{Post}_{r(j,t),t}\right) \times \text{SocialTie}_{j,P(j,t)} + \beta^P\text{Controls}_{jt}),\end{aligned} \tag{1.24}$$

and a difference-in-differences model on the teachers' perceived unfairness:

$$\text{Undeserving\%}_{it} = \delta\text{Post}_{r(i,t)t} + g^{\delta}_{h(i,t)}(t) + \lambda_{i,P(i,t)} + \varepsilon^{\delta}_{it}. \tag{1.25}$$

Controls in Equation (1.24) are the same as in the main estimation equation of favoritism (Equation (1.5)). School-specific time trends and teacher-principal fixed effects are included in the estimation of promotion unfairness.

Columns (2) and (3) of Table 1.15 report the results. The transparency reform made principals put 50% lower weight on their social ties to the applicants in promotion evaluations, and it led to a 50% decrease in the fraction of promotees who are perceived as unfairly promoted by peers.[94] Figure 1.22 plots event studies of the transparency reform

[93] This idea is formalized in Proposition 2 of the principal-agent model in Appendix A3.

[94] $\frac{0.135-0.264}{0.264} \approx -0.5$, see Column (2) of Table 1.15. $\frac{-0.247}{0.461} \approx -0.5$, see Column (3) of Table 1.15.

on promotion unfairness,[95] where we can see the number of unfair promotions fell after the reform was launched in each city. The transparency reform proved helpful in correcting the principals' favoritism in employee promotions.[96]

1.6.3 School Productivity

The overall impact of the transparency reform on school productivity depends on the relative strengths between its two counter-acting components discussed in the last two sub-sections: making principals act less unfairly while at the same time leading teachers to punish unfairness more heavily.[97]

I examine the changes in the average College Entrance Exams (CEE) test scores earned by the graduating cohorts in each of the four cities brought by the transparency reform,[98] by estimating:

$$A_{ckt}^{\text{CEE}} = \delta \text{Post}_{r(c),t} + g_{h(c)}^{\delta}(t) + \lambda_{P(c,t),h(c)}^{\delta} + \varepsilon_{ckt}^{\delta}. \tag{1.27}$$

for the graduating cohorts ($g(c,t) = 3$). School-specific time trends and principal-school fixed effects are included to account for school-level productivity drifts over time and possible matching effects between principals and schools. Changes in relative CEE scores between

[95] The event-study estimation equation is given by:

$$\text{Undeserving\%}_{it} = \sum_{\tau=-3}^{3} \chi_{\tau} \mathbb{I}\left[t - t_{r(i,t)}^{0} = \tau\right] + g_{h(i,t)}^{\chi}(t) + \lambda_{i,P(i,t)}^{\chi} + \varepsilon_{it}^{\chi}, \tag{1.26}$$

where t_r^0 is the year the reform is launched in city r.

[96] Principals might respond to information disclosure for various reasons. For example, although not monetarily rewarded for the test scores produced by their schools, the principals might care about the academic performance of students graduating from their schools either out of intrinsic concern for the students' future well-being or through the extrinsic benefits the students' good test performance could bring (see note [89]). Also, the principals might feel social pressure or stigma when their favoritism is exposed to their subordinates. While transparency as a package deal is effective in alleviating favoritism, I cannot distinguish between these potential mechanisms.

[97] This idea is formalized in Proposition 3 of the principal-agent model in Appendix A3.

[98] The High School Entrance Exams (HEE) and other end-of-year tests are organized within each city, therefore they cannot be used to evaluate the reform whose timing varied at the city level. The College Entrance Exams (CEE) are organized at the provincial level, in which the performance of students from different cities are comparable.

cities can proxy changes in school performance, as long as other determinants of scores, such as the average innate ability of different cohorts in each city, are not correlated with the timing of the transparency reform itself.

As shown by the estimation results of Equation (1.27) presented in Column (4) of Table 1.15, the reform raised high school graduates' class-subject average CEE scores in a city where it is in effect, compared to the other of the 4 cities.[99] The positive effect is also evident in the event study results shown in Figure 1.23.[100]

To conclude, the findings presented in this section suggest that requiring principals to disclose the profiles of promotion applicants to their peers is welfare-enhancing in the high schools I study. The threat from the teachers to punish the principals' unfair promotion evaluations is stronger when they can better observe their peer applicants' true quality, and this monitoring effect limits the extent to which discriminating principals act unfairly and improves school-wide performance. Mandatory disclosure of workers' performance in this study functions in similar fashions to the introduction of compensation schemes that link managers or job referrers' pay to the performance of their subordinates or referrals (Bandiera et al., 2009; Beaman and Magruder, 2012): both lead to selection of high ability workers rather than socially connected ones and improve the overall productivity of an organization.

1.7 Conclusion

I have presented evidence that in the Chinese public schools studied in this paper, when observing biased promotion decisions made by school principals in favor of their socially

[99]The point estimates should not be interpreted as the size of the causal effect of the transparency reform, as the 4 cities account for a large fraction of the CEE takers in the province, and higher CEE scores of one city have a mechanical negative effect of the scores in the other cities after the scores are normalized to have zero mean and unit standard deviation.

[100] The event-study estimation equation is given by

$$A_{ckt}^{\text{CEE}} = \sum_{\tau=-3}^{3} \chi_\tau \mathbb{I}\left[t - t_{r(c)}^{0} = \tau\right] + g_{h(c)}^{\chi}(t) + \lambda_{P(c,t),h(c)}^{\chi} + \varepsilon_{it}^{\chi}. \tag{1.28}$$

connected promotion applicants, peer teachers reduce their effort at work and become more likely to quit their jobs. Although the positive effects of favoritism on the favored and the negative effects on the biased against applicants offset each other, the adverse spillover effects on non-applicant teachers impose substantial costs on the overall productivity of schools.

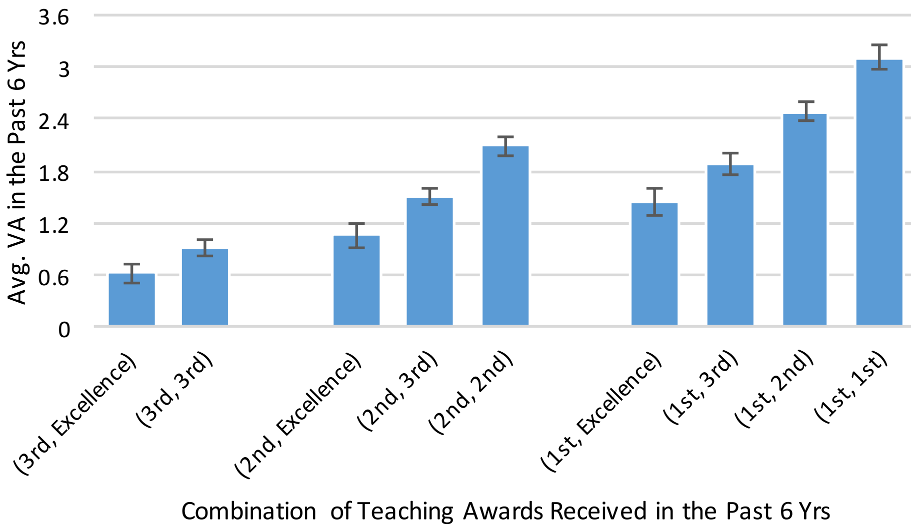
These findings have important implications on the costs of giving leaders discretion in promotion decisions when they exercise favirotism. First, they suggest that these costs lie not only in that biased decisions made by bosses can lead to misallocation of human capital resources, but also in that it can demoralize employees and lower their effort and performance. Second, the spillover incentive effects suggest that when evaluating the potential consequences of (bad) management practices, focusing only on their effects on the directly targeted agents might understate the true costs imposed on the whole organization.

The suggestive mechanisms through which the incentive costs of favoritism operate also shed light on the nature of the social preferences that underpin them. The adverse productivity effect of principal's biased promotion decisions is the most pronounced among non-prospective-applicants who have frequent social interactions with the victims of favoritism, suggesting that teachers hold fairness norms in the workplace and have social preferences for their co-workers: treatment received by peers at work that violates fairness norms can erode the workers' morale and increase their marginal cost of effort, leading to underperformance.

The welfare enhancement brought by the mandatory disclosure of promotion applicants' profiles to peer teachers suggests that information transparency can be adopted by higher-level authorities as an effective policy tool to combat favoritism exhibited by biased principals and its adverse impacts on organizational performance, when workers can act upon such information in ways that are relevant to the principal.

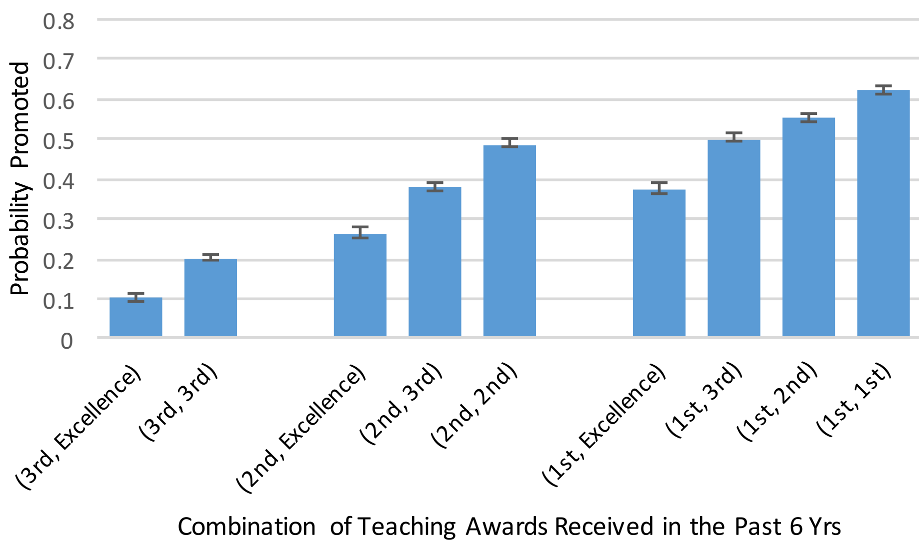
1.8 Figures

Figure 1.1: Teaching Awards and Teachers' VA



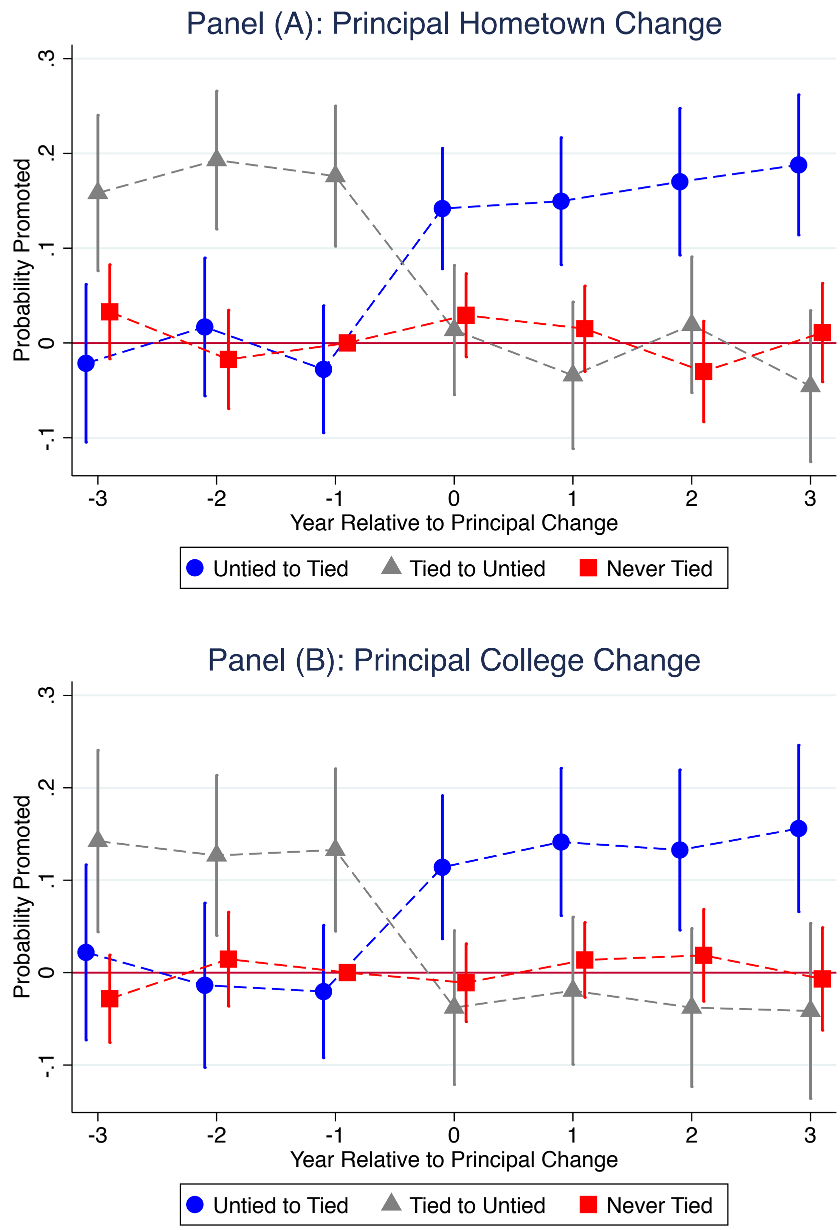
Notes: This graph plots the expected average value-added in the past 6 years of applicants with different teaching award combinations in the past 6 years (see their distribution in Figure A4), conditional on other application profile characteristics (see Table 1.2 for a detailed description of these variables) and social ties with the principal. The unit of analysis is the applicant-year. Each point shows the coefficient estimate on an teaching award combination dummy in Equation (1.3). School-specific time trends are controlled for. The outcome variable is scaled to have unit standard deviation. N=57,613. For information on the other covariates, see Table 1.6. Standard errors are clustered at the applicant level. The vertical bars show the 95% confidence intervals. The omitted teaching awards combination is (Excellence, Excellence).

Figure 1.2: Teaching Awards and Promotion Rates



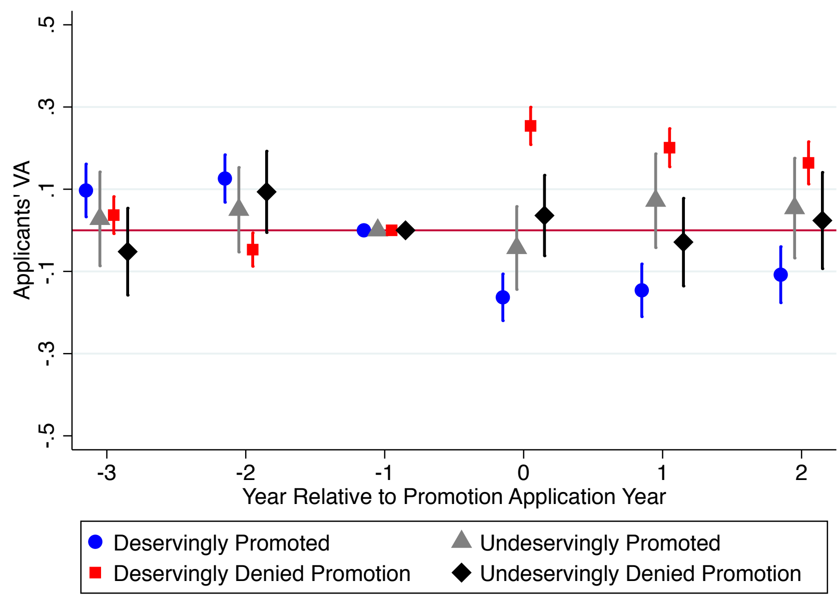
Notes: This graph plots the expected promotion probabilities of applicants with different teaching award combinations in the past 6 years (see their distribution in Figure A4), conditional on other application profile characteristics (see Table 1.2 for a detailed description of these variables) and their social ties to the principal. The unit of analysis is the applicant-year. Each point shows the coefficient estimate on an teaching award combination dummy from a logit regression in Equation (1.4). Share of same-subject applicants and school-year fixed effects are included. N=57,613. For coefficient estimates on the other covariates, see Panel (B), Column (1) of Table 1.7, or Column (1) of Table A2. Standard errors are clustered at the applicant level. Column (1) of Table 1.7). Coeffients are in terms of average marginal effects. The vertical bars show the 95% confidence intervals. The omitted teaching awards combination is (Excellence, Excellence).

Figure 1.3: Event Studies of Principal Entries: Promotion Probabilities of Differently-Tied Applicants



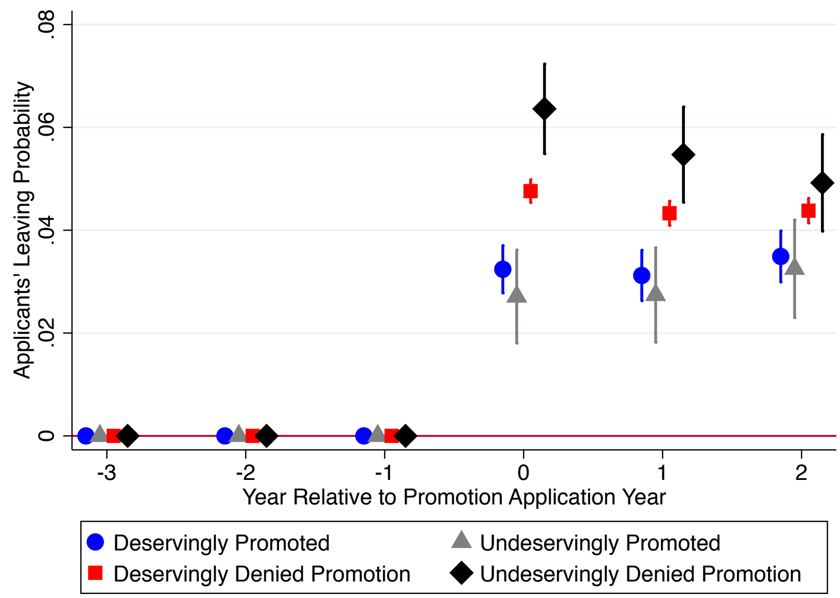
Notes: This graph plots event studies of the applicants' promotion rates before and after the entry of a new principal of different hometown or college background from the previous one. The applicants can be divided into 3 types $\mathbb{Q}$={ tied before & after, tied before & untied after, untied before & tied after}. The estimated coefficients on the relative year dummies ($\{\hat{\mu}_{q\tau}\}_{\tau=-3}^{3}$) from the regressions of Equation (1.6), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the applicant level are reported in Table A4). Controls of applicant characteristics $\{\mathbf{X}_g\}_{g\in\mathbb{G}}$ ($\mathbb{G}$ ={demographics, experience, workload, research, teaching, other}, see Table 1.2 for a detailed description of these variables), the share of same-subject applicants, and school-year fixed effects are included. $\mu_{\text{never-tied},-1} = 0$ by construction. Mean promotion probability is 0.221 for Panel (A) and 0.212 for Panel (B).

Figure 1.4: Event Studies of Personal Promotion Results: Applicants' Value-Added



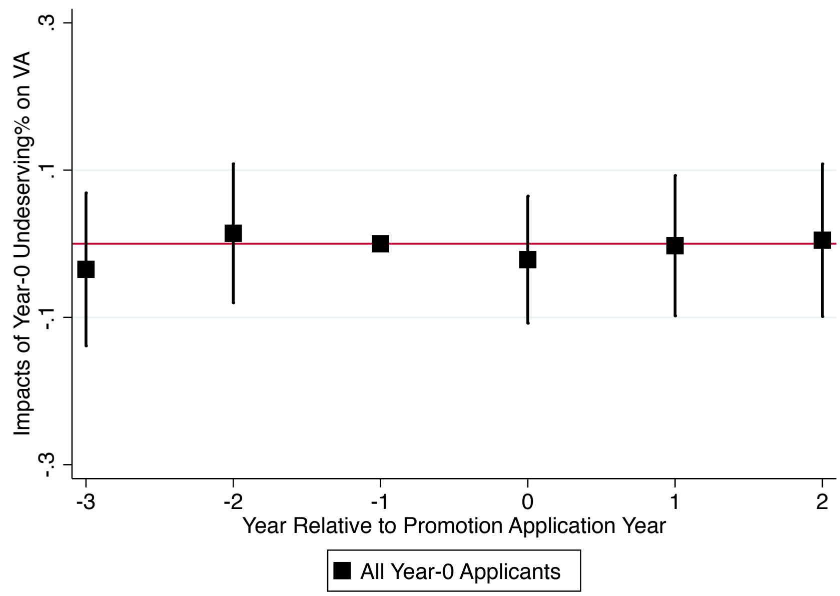
Notes: This graph plots event studies of the applicants' value-added before and after the application year of applicant type $m \in \mathbb{M}$ ={Undeservingly Promoted, Deservingly Promoted, Deservingly Denied, Undeservingly Denied}. The estimated coefficients on the relative year dummies ($\{\hat{\varphi}_{m\tau}\}_{\tau=-3}^{2}$) from the regressions of Equation (1.18), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the applicant level are reported in Panel (A) of Table A5). Only the applicant-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Applicants' job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0)-principal fixed effects, applicant-(current)-principal fixed effects are controlled for. $\varphi_{m,-1} = 0$ by construction. The standard deviation of teacher-school-specific VA (VA_{jh}) is 0.621.

Figure 1.5: Event Studies of Personal Promotion Results: Applicants' Job-Quitting Probability



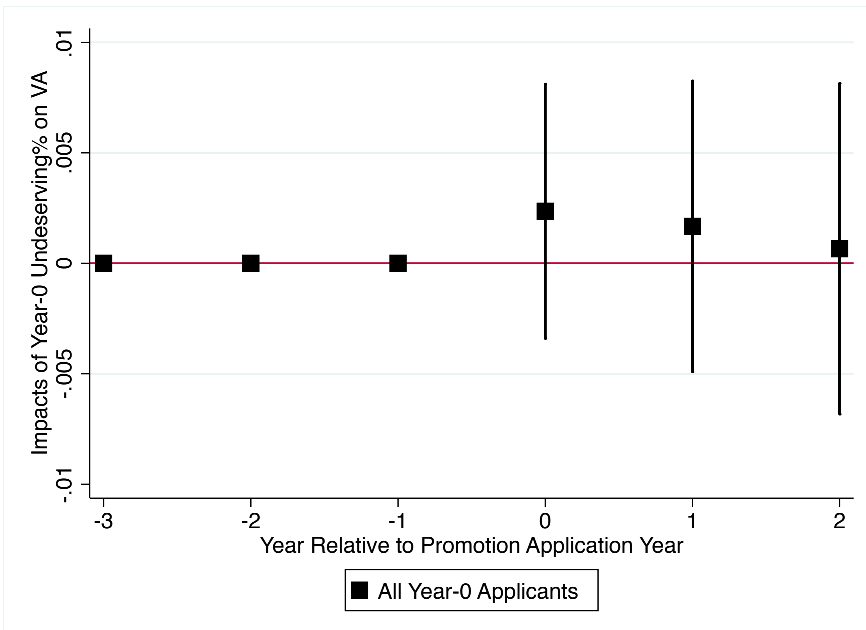
Notes: This graph plots event studies of the applicants' probability of leaving the currenty school before and after the application year of applicant type $m \in \mathbb{M} =\{$Undeservingly Promoted, Deservingly Promoted, Deservingly Denied, Undeservingly Denied$\}$. The estimated coefficients on the relative year dummies ($\{\hat{\varphi}_{m\tau}\}_{\tau=-3}^{2}$) from the regressions of Equation (1.18), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the applicant level are reported in Panel (B) of Table A5). Only the applicant-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Applicants' job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0)-principal fixed effects, applicant-(current)-principal fixed effects are controlled for. $\varphi_{m,\tau} = 0, \forall \tau < 0$, by construction.

Figure 1.6: Impacts of Perceived Promotion Unfairness on Applicants' Value-Added



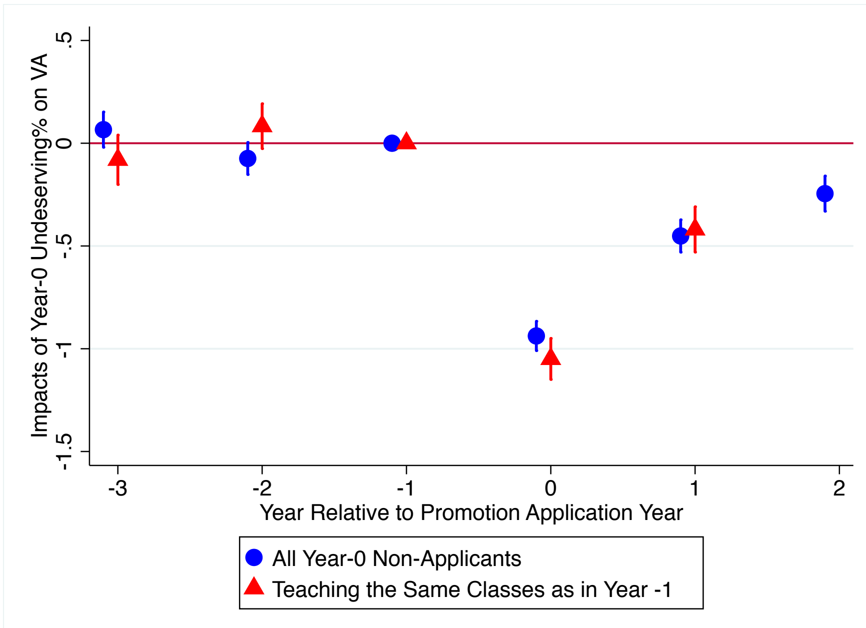
Notes: This graph shows the impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged VA of the promotion applicants in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. The estimated coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (A) of Table A6. Only the applicant-year observations where the applicant works in the same school as the application year ($h(j, t+s) = h(j, t)$), and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. $\theta_{-1} = 0$ by construction.

Figure 1.7: Impacts of Perceived Promotion Unfairness on Applicants' Job Quitting Probability



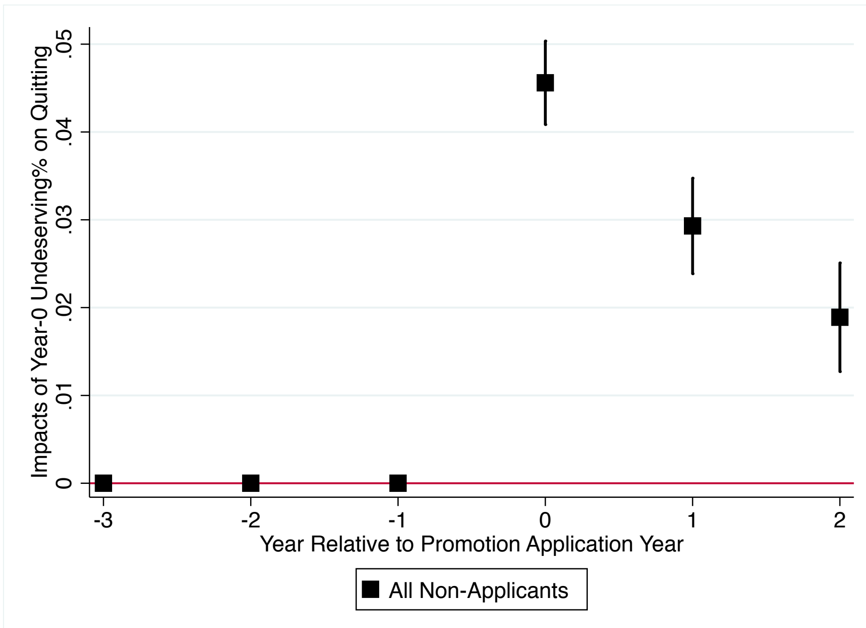
Notes: This graph shows the impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the promotion applicants in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. The estimated coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (B) of Table A6. Only the applicant-year observations where the applicant works in the same school as the application year ($h\left(j,t+s\right) = h\left(j,t\right)$), and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. $\theta_{-1} = 0$ by construction.

Figure 1.8: Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Value-Added



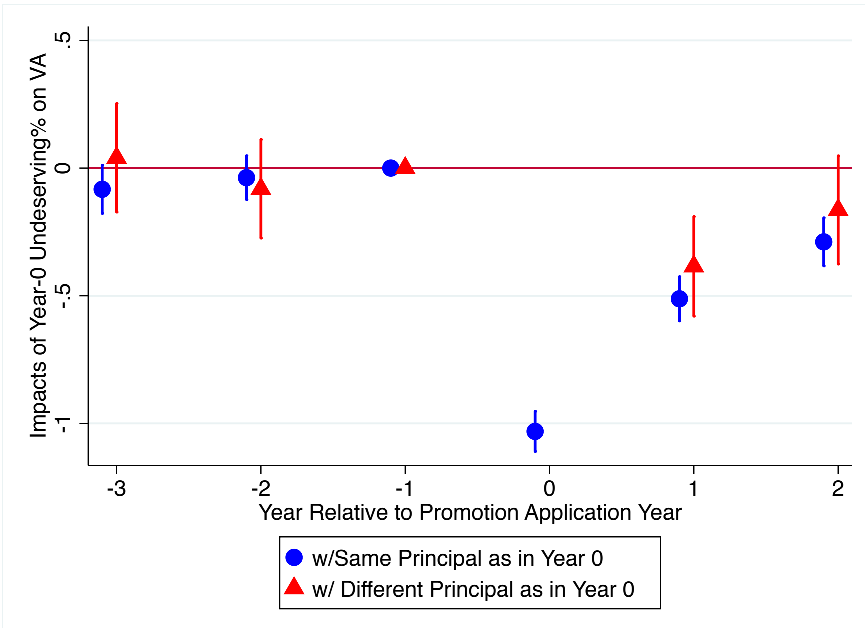
Notes: This graph shows the impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged VA of the non-applicant teachers in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. The estimated coefficients along with their associated standard errors clustered at the teacher level are reported in Panels (A) and (B) of Table A7. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h\left(i,t+s\right) = h\left(i,t\right)$), are included. The blue dots show the results using the whole sample, while the results shown in the red dots include only the observations where the teacher teaches the same set of classes as in year -1. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. $\theta_{-1} = 0$ by construction.

Figure 1.9: Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Job-Quitting Probability



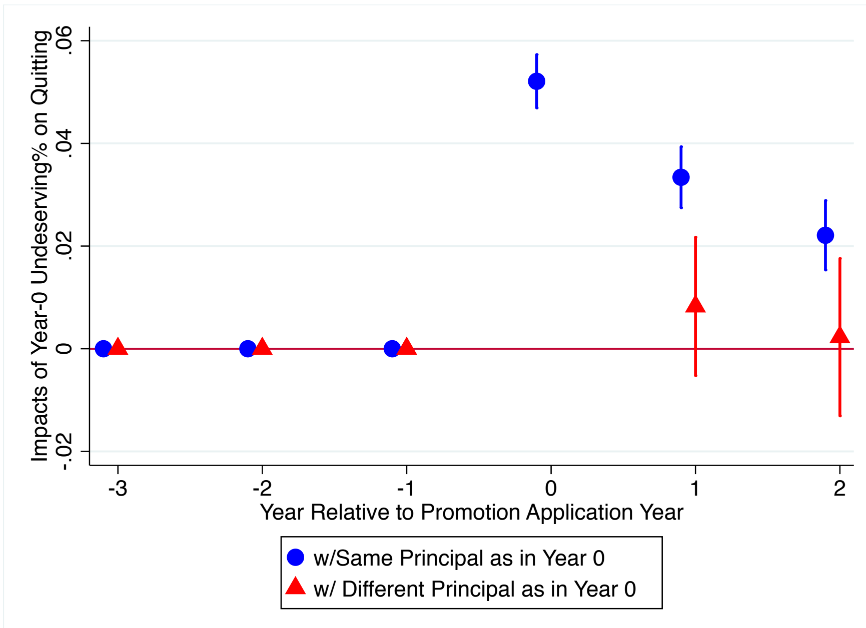
Notes: This graph shows the impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the non-applicant teachers in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regression of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (C) of Table A7. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i, t)$), are included. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. $\theta_\tau = 0, \forall \tau < 0$, by construction.

Figure 1.10: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Principal's Presence



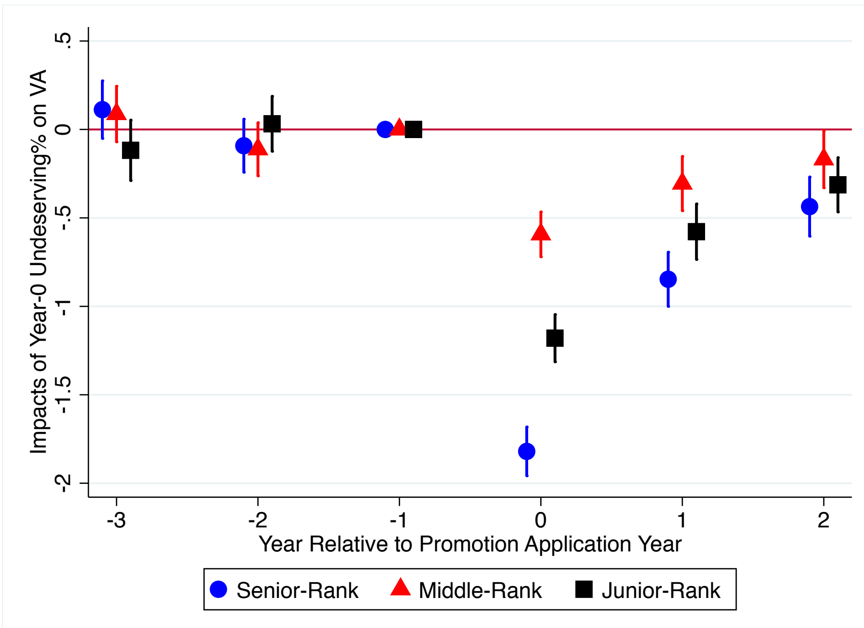
Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged VA of non-applicant teachers when the promotion-decision-making principal is either still in the school or not. The estimated coefficients on current Undeserving% (interacted with relative year dummies) $\left(\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}\right)$ from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (A) of Table A8. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year $(h(i, t+s) = h(i, t))$, are included. The blue dots show the the results using the subsample where the principal who makes the promotion decision in year 0 is in the school $(P(i, t+s) = P(i, t))$; the red dots show the results using the subsample where the promotion-decision-making principal has left (not arrived in) the school $(P(i, t+s) \neq P(i, t))$. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of years when the principal is not the same as in year 0 is 16% in the sample. $\theta_{-1} = 0$, by construction.
This graph presents the empirical test results of Predictions I.2) and II.2) in Sub-section 1.5.2.3.

Figure 1.11: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Quitting Probability: Principal's Presence



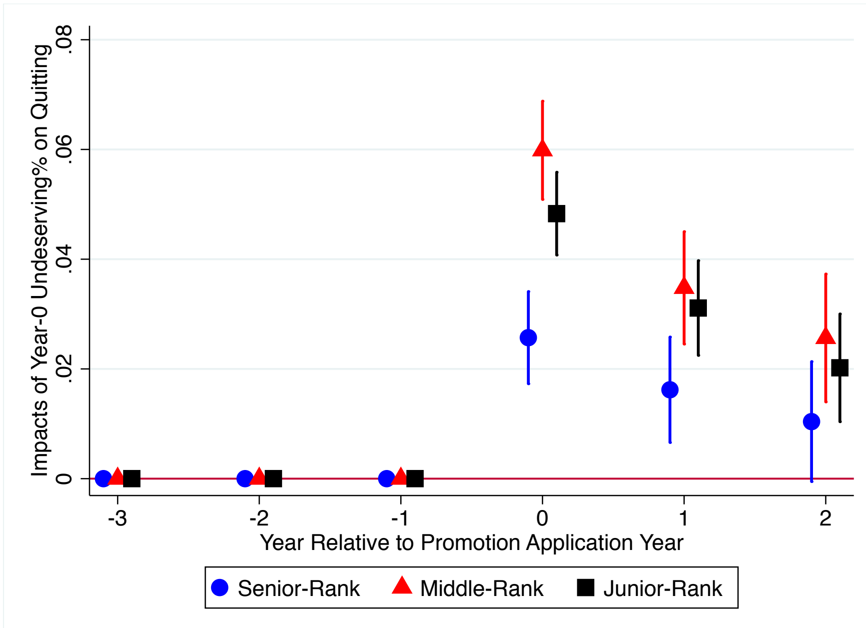
Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the non-applicant teachers when the promotion-decision-making principal is either still in the school or not. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (B) of Table A8. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i, t)$), are included. The blue dots show the the results using the subsample where the principal who makes the promotion decision in year 0 is in the school ($P(i, t+s) = P(i, t)$); the red dots show the results using the subsample where the promotion-decision-making principal has left (not arrived in) the school ($P(i, t+s) \neq P(i, t)$). Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of years when the principal is not the same as in year 0 is 16% in the sample. $\theta_\tau = 0, \forall \tau < 0$, by construction.
This graph presents the empirical test results of Predictions I.2) and II.2) in Sub-section 1.5.2.3.

Figure 1.12: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Professional Ranks



Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged VA of the non-applicant teachers of different professional ranks in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (A) of Table A9. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year $(h(i, t+s) = h(i, t))$, are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. Senior, middle and junior teachers account for 27%, 41% and 32% of the sample respectively. $\theta_{-1} = 0$ by construction. This graph presents the empirical test results of Prediction *Ia.1)* in Sub-section 1.5.2.3.

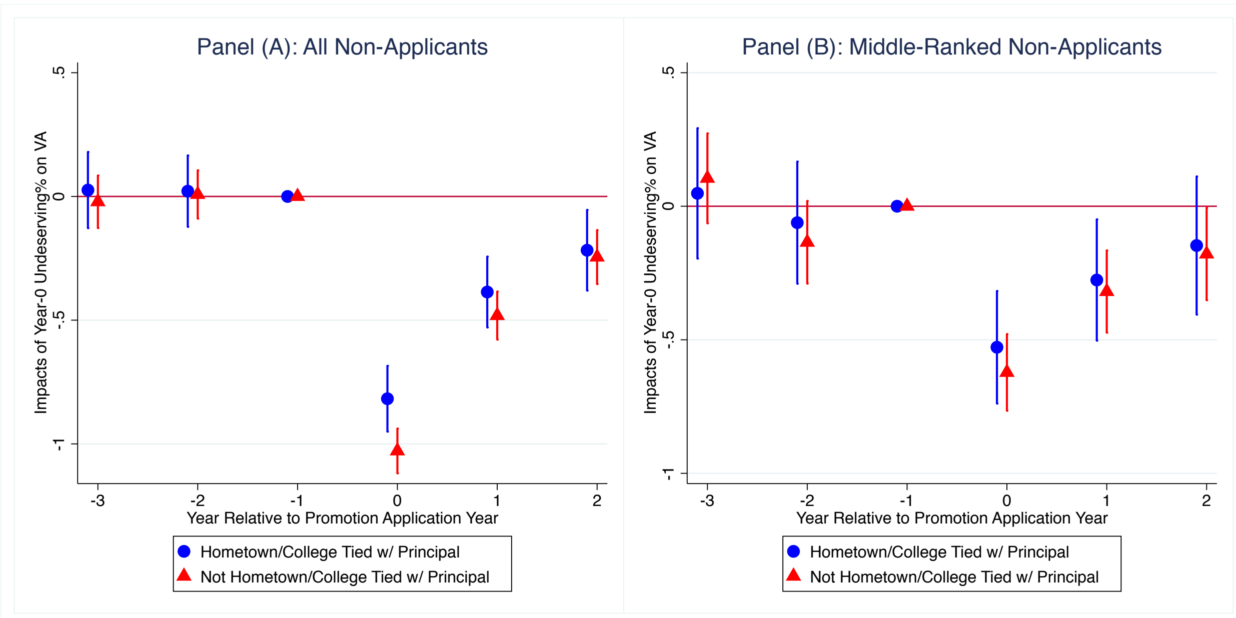
Figure 1.13: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Quitting Probability: Professional Ranks



Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the non-applicant teachers of different professional ranks in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (B) of Table A9. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i,t)$), are included. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. Senior, middle and junior teachers account for 27%, 41% and 32% of the sample respectively. $\theta_\tau = 0, \forall \tau < 0$, by construction.

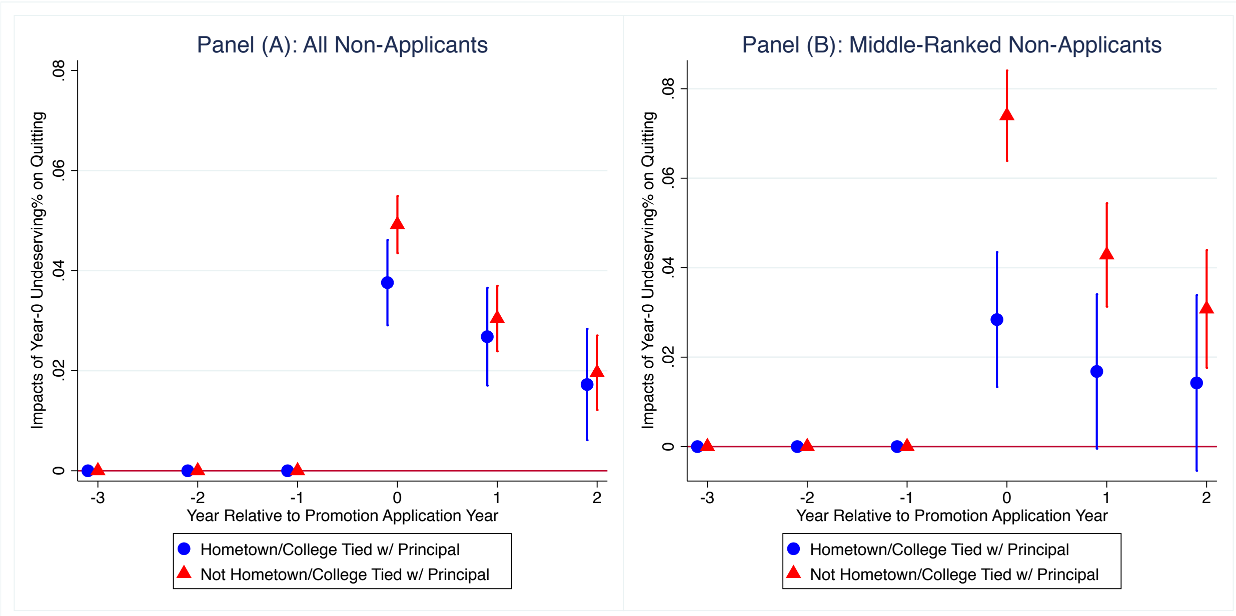
This graph presents the empirical test results of Prediction *Ia.1)* in Sub-section 1.5.2.3.

Figure 1.14: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Social Ties w/ Principal



Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged VA of the non-applicant teachers who are either socially tied to the promotion-decision-making principal or not. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panels (C) and (D) of Table A10. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i, t)$), are included. The blue dots show the results using the subsample where the teacher is socially tied to the principal who makes the promotion decision in year 0 is in the school ($\text{SocialTie}_{i,P(i,t)} = 1$); the red dots show the results using the subsample where the teacher is not socially tied to the principal in year 0 ($\text{SocialTie}_{i,P(i,t)} = 0$). Panel (A) shows the results for all non-applicant teachers, and Panel (B) shows the results for the middle-ranked teachers (prospective applicants in the next few years) only. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of teachers socially tied to principal is 33% in the sample. $\theta_{-1} = 0$ by construction.
Panel (A) presents the empirical test results of Predictions *Ib)* and *II.3)*, and Panel (B) presents the test results of Predictions *Ia.2)* and Ia.3) in Sub-section 1.5.2.3.

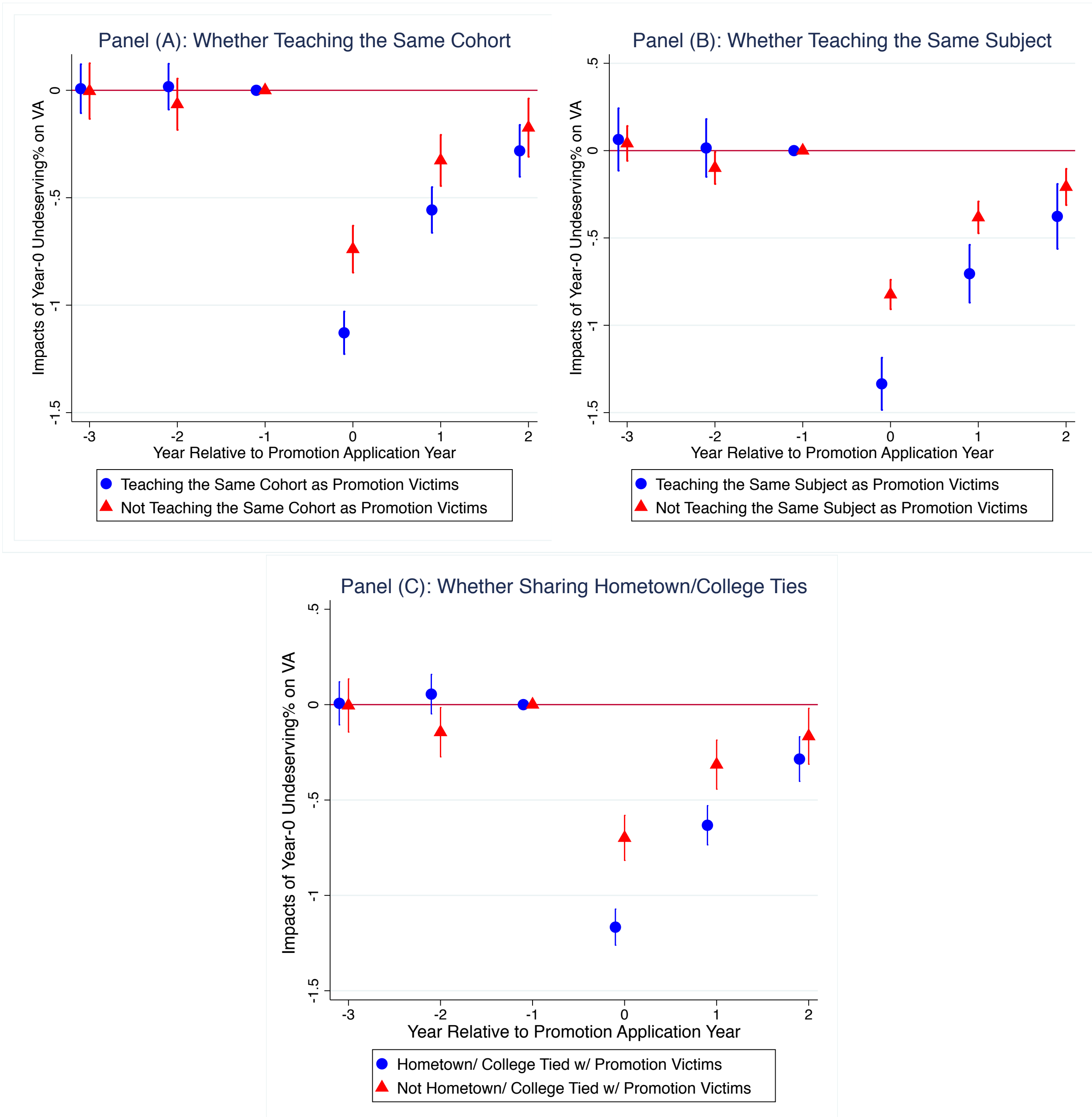
Figure 1.15: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Quitting Probability: Social Ties w/ Principal



Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the non-applicant teachers who are either socially tied to the promotion-decision-making principal or not. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panels (C) and (D) of Table A10. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i,t)$), are included. The blue dots show the results using the subsample where the teacher is socially tied to the principal who makes the promotion decision in year 0 is in the school ($\text{SocialTie}_{i,P(i,t)} = 1$); the red dots show the results using the subsample where the teacher is not socially tied to the principal in year 0 ($\text{SocialTie}_{i,P(i,t)} = 0$). Panel (A) shows the results for all non-applicant teachers, and Panel (B) shows the results for the middle-ranked teachers (prospective applicants in the next few years) only. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of teachers socially tied to principal is 33% in the sample. $\theta_\tau = 0, \forall \tau < 0$, by construction.

Panel (A) presents the empirical test results of Predictions *Ib)* and *II.3)*, and Panel (B) presents the test results of Predictions *Ia.2)* and Ia.3) in Sub-section 1.5.2.3.

Figure 1.16: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Social Interactions with Victims



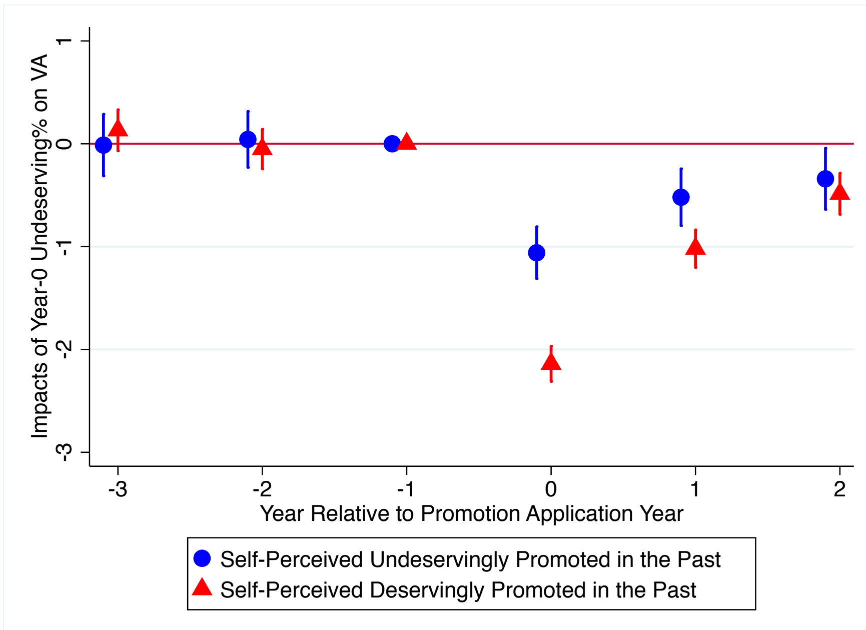
Notes: This graph shows the heterogeneous impacts of current promotion unfairness (Undeserving%) on the current, future and lagged VA of teachers who have different levels of social interactions with the "victims" (i.e., undeservingly denied promotion). The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_{\tau}\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Table A11. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$), are included. The blue dots show the results using the subsample of teachers who have more often social interactions with the promotion victim(s): teaching the same cohort(s) as the victims (Panel (A))[a], teaching the same subject as the victims (Panel (B))[b], and sharing social ties with the victims (Panel (C))[c]; the red dots show the results using the subsample of teachers with less often social interactions with the promotion victim(s). Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. Teachers with student-cohort-interaction, subject-interaction and social ties with promotion victims account for 57%, 22% and 61% of the sample respectively. $\theta_{-1} = 0$ by construction.

[a] $\exists i'$ s.t. $m(i,i',t) = \text{Undeservingly Denied}$ and $G_{i,t+s} \cap G_{i',t+s} \neq \emptyset$, where $G_{i,t+s} := \{g(c,t+s) \,|\, j(c,k(i),t+s) = i\}$.

[b] $\exists i'$ s.t. $m(i,i',t) = \text{Undeservingly Denied}$ and $k(i) = k(i')$.

[c] $\exists i'$ s.t. $m(i,i',t) = \text{Undeservingly Denied}$ and $\text{SocialTie}_{ii'} = 1$.

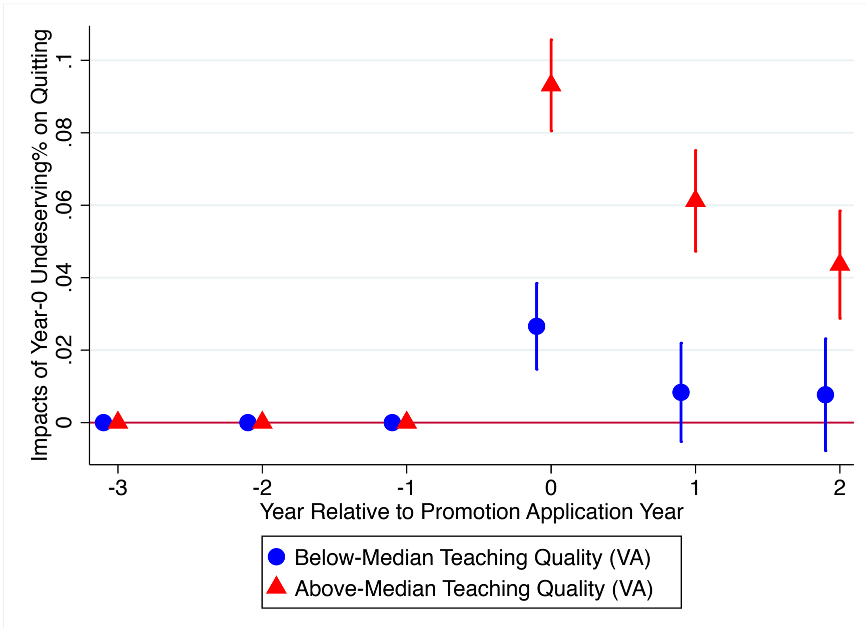
Figure 1.17: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Past Promotion Experience



Notes: This graph shows the heterogeneous impacts of current promotion unfairness (Undeserving%) on the current, future and lagged VA of senior-ranked teachers who perceive themselves as either deservingly or undeservingly promoted in the past. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Table A12. Only the teacher-year observations where the teacher has been promoted to the senior rank before the reference application year (year 0) and works in the same school as in the reference application year, are included. The blue dots show the results using the subsample of senior-ranked teachers who were self-perceived undeservingly promoted before year 0[a]; the red dots show the results using the subsample of senior-ranked teachers who were self-perceived deservingly promoted before year 0[b]. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of senior-ranked teachers who perceive themselves as deservingly promoted in the past is 76% in the sample. $\theta_{-1} = 0$ by construction.

[a] $\exists t' \leq \min\{t, t+s\}$ s.t. $m(i,i,t') =$ Undeservingly Promoted.
[b] $\exists t' \leq \min\{t, t+s\}$ s.t. $m(i,i,t') =$ Deservingly Promoted.

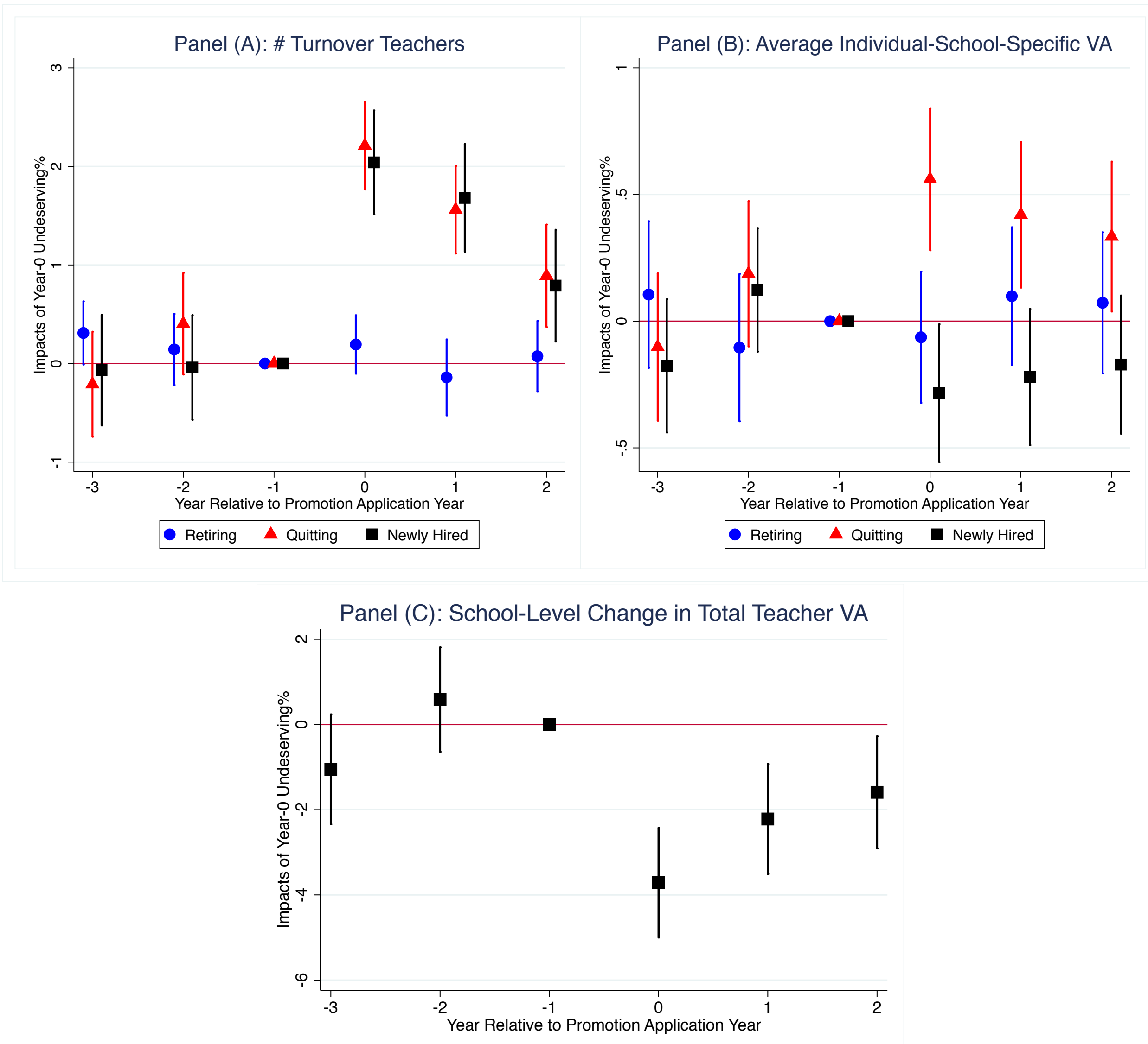
Figure 1.18: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Quitting Probability: Teacher Quality



Notes: This graph shows the heterogeneous impacts of current perceived promotion unfairness (Undeserving%) on the current and future job quitting probabilities of the non-applicant middle-ranked teachers of high or low teaching quality. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regression of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Table A13. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0), works in the same school as in the reference application year ($h\left(i, t+s\right) = h\left(i, t\right)$) and is currently middle-ranked, are included. The blue dots show the results using the subsample where the teachers' individual-school-specific average VA is below median in the school-year[a], and the red dots plot the results for the above-median-VA teachers. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for. The share of years when the principal is not the same as in year 0 is 16% in the sample. $\theta_\tau = 0, \forall \tau < 0$, by construction.

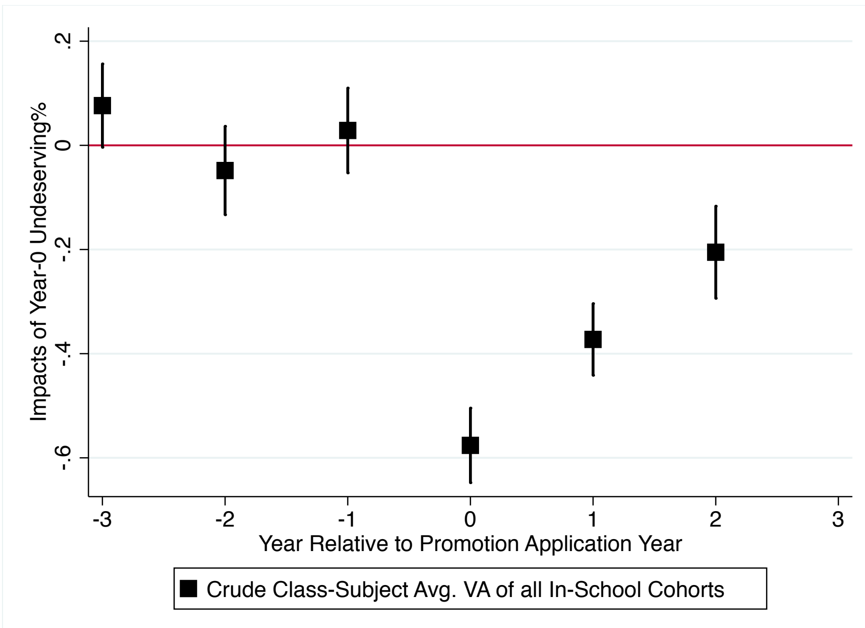
[a] $\mathrm{VA}_{i,h(i,t)} < \mathrm{Median}_{h(i,t),t+s}\left[\mathrm{VA}_{i',h(i,t)}\right]^{h\left(i',t+s\right)=h(i,t)}$.

Figure 1.19: Impacts of Perceived Promotion Unfairness on Teacher Turnover



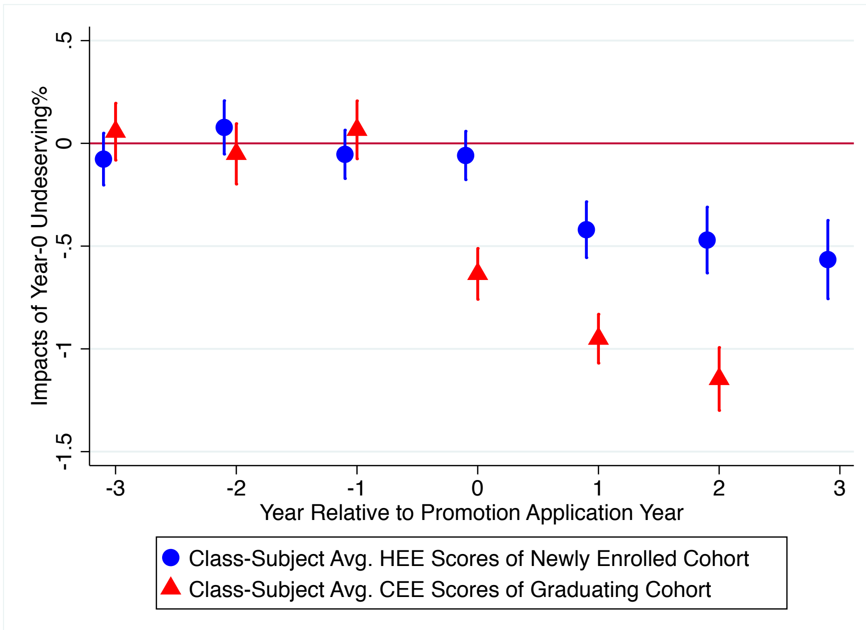
Notes: This graph shows the impacts of current promotion unfairness (Undeserving%) on current, future and lagged number and quality of turnover teachers of three types: retiring, quitting and newly hired. The unit of analysis is the school-year. The estimated coefficients on current Undeserving% (interacted with relative year dummies) from the regressions of Equation (1.21), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the school level are reported in Table A14. Lagged outcome variable, school-specific time trends and principal-school fixed effects are controlled for. The mean number of retired teachers per school-year is 3.30 (0.51); the mean number of quitting teachers per school-year is 6.31 (2.03); the mean number of new hires per school-year is 10.3 (2.83). The standard deviation of teacher-school-specific VA (VA_{ih}) is 0.56.

Figure 1.20: Impacts of Perceived Promotion Unfairness on Crude (Class-Level) VA



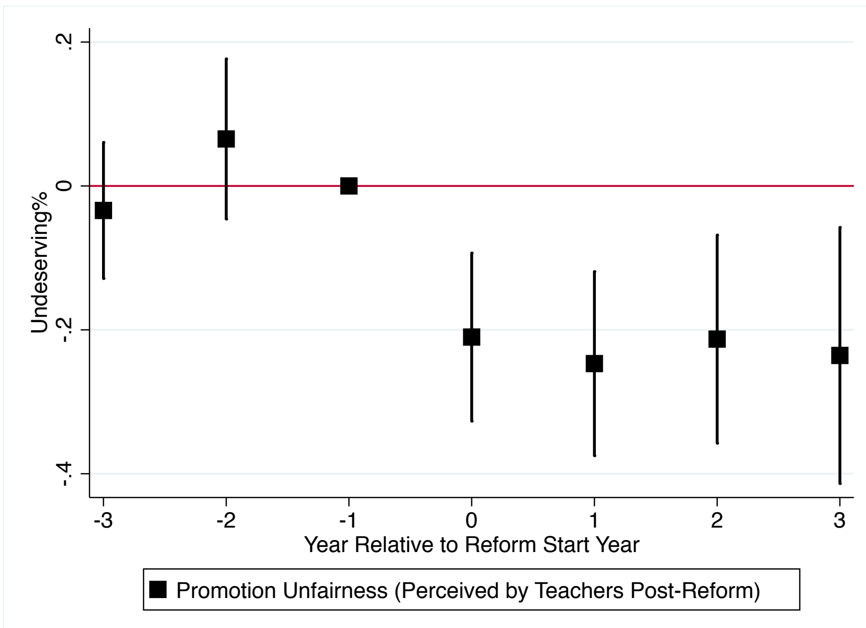
Notes: This graph shows the impacts of current promotion unfairness (Undeserving%) on a crude VA measure which takes the difference between the end-of-year and the end-of-last-year test scores of each class. The unit of analysis is the class-subject-year. The estimated coefficients from the regressions of Equation (1.22) on current Undeserving% (interacted with relative year dummies) , as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the school level are reported in Panel (A) of Table A15. School specific time trends and principal-school fixed effects are controlled for.

Figure 1.21: Impacts of Perceived Promotion Unfairness on Students' CEE and HEE Scores



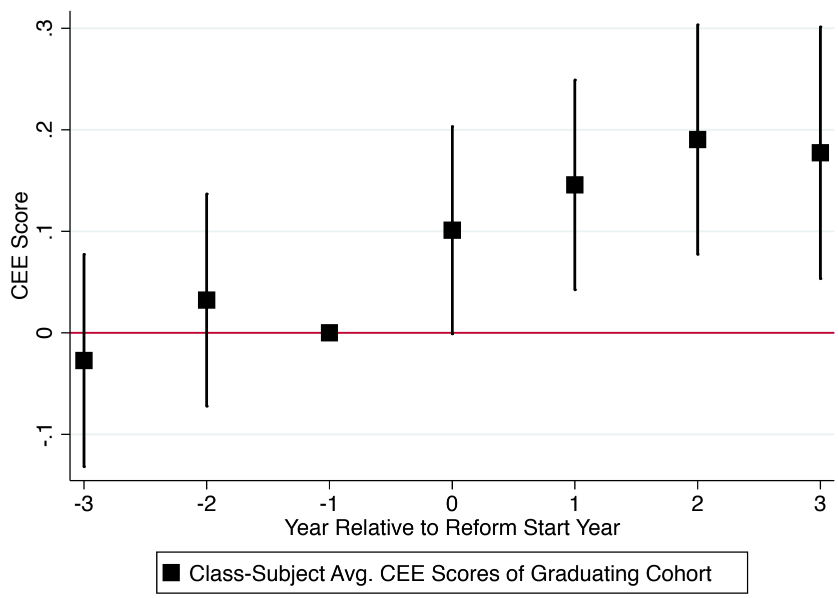
Notes: This graph shows the impacts of current promotion unfairness (Undeserving%) on the class-subject average College Entrance Exams (CEE) scores of the graduating cohort and the class-subject average High School Entrance Exams (HEE) scores of the newly enrolled cohort in a school. The unit of analysis is the class-subject-year. The estimated coefficients from the regressions of Equation (1.22) on current Undeserving% (interacted with relative year dummies) , as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the school level are reported in Panels (B) and (C) of Table A15. School specific time trends and principal-school fixed effects are controlled for.
The composition of the graduating cohorts in year $\tau \leq 2$ is determined before the promotion results are revealed in the reference year 0. Therefore the impacts of unfairness on the CEE scores of these cohorts reflects the impacts on the VA they receive rather than school selection. The graduating cohort in year $\tau = 2$ is the cohort for which the reference (unfair) promotion takes place at the beginning of their grade 1, and it undergoes its potential impacts throughout their 3 years of high school.
The composition of the newly enrolled cohort in year $\tau \leq 0$ is determined before the promotion results come out in year 0. Therefore conceptually there should be no impact of promotion unfairness on the HEE scores of these newly enrolled cohorts in these years. The effects on the newly enrolled cohorts in year $\tau > 0$ shows the influence of unfairness in promotion on the quality of future incoming classes.

Figure 1.22: Event Studies of Transparency Reform on Promotion Unfairness (Perceived by Teachers)



Notes: This graph shows the impacts of the transparency reform, which requires disclosing the applicants' application CVs to peer teachers, on the perceived promotion unfairness by teachers. The estimated coefficients from the regression of Equation (1.26) on the relative year dummies the reform start years ($\{\hat{\chi}_\tau\}_{\tau=-3}^{3}$), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Panel (A) of Table (A16). The unit of analysis is the teacher-year. School-specific time trends and principal-school fixed effects are controlled for.

Figure 1.23: Event Studies of Transparency Reform on College Entrance Exams Performance



Notes: This graph shows the impacts of the transparency reform, which requires disclosing the applicants' application CVs to peer teachers, on the College Entrance Exams (CEE) scores of high school graduates. The estimated coefficients from the regression of Equation (1.28) on the relative year dummies to the reform start years ($\{\hat{\chi}_\tau\}_{\tau=-3}^{3}$), as well as the 95% confidence intervals, are plotted. These coefficients along with their associated standard errors clustered at the school level are reported in Panel (B) of Table (A16). The unit of analysis is the class-subject-year. School-specific time trends and principal-school fixed effects are controlled for.

1.9 Tables

Table 1.1: Panel Structure of Application Profile Dataset and Timing of Transparency Reform

City	Transparency Reform Launched in	Years Available in Data	# High Schools
A	2006	2001-2017	34
B	2010	2002-2017	31
C	2012	2003-2016	23
D	2013	2003-2016	24
Total	-	2001-2017	112

Table 1.2: Variables in Categories in Application CVs

Category	Variables
Demographics	Gender, ethnicity, city of birth, year of birth, Communist Party membership status, college/grad school attended, subject taught, etc.
Experience	Career teaching experience, years as middle-ranked, years in current school.
Workload	Average number of sessions taught per week, years as a class head teacher, etc.
Research	Publications on national/provincial-level journals, etc.
Teaching	(Value-added-based) teaching awards of different levels.
Other	Awards of different levels from teaching demonstration contests, extra-curriculum activities, honours received by the head-teachered classes, etc.

Notes: This table lists the categorized variables on promotion applicants' formatted CVs, see Definition (1.1). It is required that items an applicant lists under categories of Workload, Research, Teaching and Others cover the past 6 years prior to application.

Table 1.3: Descriptives of High Schools and Promotion Applications

	Mean	Std Dev
Panel (A): School Descriptives (per School-Year)		
# Teachers	119.3	(14.4)
# Classes	35.8	(3.72)
Class Size	48.2	(4.12)
% Junior-ranked Teachers	0.22	(0.014)
% Middle-ranked Teachers	0.53	(0.016)
% Senior-ranked Teachers	0.25	(0.015)
Panel (B): School Principals		
Male	0.472	(0.499)
Mean Age during Term	49.3	(4.32)
Length of Term (in yrs)	6.13	(1.26)
# Principals	246	-
Panel (C): Senior Rank Applications		
# Applicants per school-year	26.2	(3.41)
# Success Rate per school-year	0.217	(0.0352)
# Applications Filed (per Teacher)	2.88	(0.821)
Ultimate Success Rate (per Teacher)	0.516	-

Notes: In Panel (A), only the years for which the application profiles dataset is available are included. In Panel (B), only terms that overlap with the years for which the application profiles dataset is available are included. When calculating the length of terms in Panel (B), I use 1994 as the start year of a term for those who served as a principal in 1994, and exclude all the most recent terms (as they might have not end in the last observed year in the sample).

Table 1.4: Summary Statistics of Teachers

	(A) All Teachers		(B) Senior Rank Applicants		(C) Survey Respondents	
	Mean	SD	Mean	SD	Mean	SD
Age	37.9	(8.93)	41.3	(3.71)	37.6	(4.21)
Male	0.378	(0.484)	0.385	(0.486)	0.392	(0.488)
CPC Member	0.389	(0.487)	0.356	(0.478)	0.397	(0.489)
Ethnic Minority	0.261	(0.439)	0.247	(0.431)	0.252	(0.434)
Experience (in yrs)	14.9	(8.21)	17.4	(3.69)	13.8	(9.01)
# Years in Current School	9.92	(5.62)	13.1	(3.25)	8.98	(5.59)
# Classes Taught per Week	12.12	(1.241)	12.21	(1.284)	12.74	(1.31)
Junior-ranked	0.212	(0.408)	0	(0)	0.235	(0.424)
Middle-ranked	0.518	(0.500)	1	(0)	0.497	(0.500)
Senior-ranked	0.270	(0.444)	0	(0)	0.268	(0.443)
Obs.	210,424		59,121		687	

Notes: For Panels (A) and (B), the unit of analysis is the teacher-year. Only the years for which the application profiles dataset is available are included. For Panel (C), the unit of analysis is the respondent in the 6 surveyed schools in September 2018.

Table 1.5: Distribution of Social Tie Types

		Hometown Tied w/ Principal		
		Tie	No Tie	All
College Tie w/ Principal	Tie	10.1%	9.6%	19.7%
	No Tie	13.0%	67.3%	80.3%
	All	23.1%	76.9%	1

Notes: The unit of analysis is the applicant-year. N=57,613.

Table 1.6: What predicts a high value-added teacher?

	Social Ties w/ Principal		Categories of Applicant Characteristics					
			Demographics	Experience	Workload	Research	Teaching	Other
Partial R^2	0.0032		0.0541	0.0391	0.0123	0.0087	0.5842	0.0403
	Hometown	College						
Coefficients	-0.0831***	0.0654***						
	(0.0142)	(0.0153)						

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the applicant level are reported in parentheses. This table presents results from an OLS regression of the value-added of applicants in the past 6 years prior to application (VA^{-6}) on their application profile characteristics and social ties to the principal (Equation (1.3)). School-specific time trends are controlled for. The outcome variable is scaled to have unit standard deviation. N=57,613. The first row reports the partial R^2 of variables in each category of application profile characteristics (see Table 1.2 for a detailed description of these variables). The second row shows the coefficient estimates on social ties dummies. One can see that the variables in the "teaching" category explains around 60% of the variance in the applicants' VA. The coefficient estimates on the teaching awards combination dummies (which are the main variables in the "teaching" category) are shown in Figure 1.1.

Table 1.7: Effect of Social Ties with Principal on Promotion Rates

	Outcome Variable: Promoted					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (A): Pooling Hometown and College Ties					
SocialTie	0.2084***	0.2112***	0.2241**	0.2225***	0.2062***	0.2063***
	(0.0088)	(0.0087)	(0.0043)	(0.0044)	(0.0206)	(0.0210)
Teacher-School-Specific VA		0.0166***		0.0149***		0.0038
(normalized)		(0.0034)		(0.0027)		(0.0037)
(pseudo) R^2	0.721	0.723	0.705	0.710	0.832	0.834
	Panel (B): Seperating Hometown and College Ties					
HomeTie	0.1757***	0.1770***	0.1940***	0.1918***	0.1758***	0.1760***
	(0.0082)	(0.0081)	(0.0053)	(0.0052)	(0.0243)	(0.0246)
CollegeTie	0.135***	0.1376***	0.1390***	0.1383***	0.1316***	0.1309***
	(0.0082)	(0.0083)	(0.0058)	(0.0057)	(0.0231)	(0.0230)
Teacher-School-Specific VA		0.0183***		0.0164***		0.0024
(normalized)		(0.0032)		(0.0026)		(0.0038)
(pseudo) R^2	0.734	0.738	0.713	0.716	0.844	0.848
X controls	Y	Y	Y	Y	Y	Y
School-year FE	Y	Y	Y	Y	Y	Y
Share of same-subject applicants	Y	Y	Y	Y	Y	Y
Individual FE	N	N	N	N	Y	Y
Model	Logit	Logit	Linear	Linear	Linear	Linear
Mean Dep. Var	0.217	0.217	0.217	0.217	0.213	0.213
# Obs	57,613	57,613	57,613	57,613	57,613	57,613

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses.

This table presents the estimated average effect of an applicant's social ties to the principal on her promotion probability (estimation results of Equation (1.4)). Controls of application profile characteristics (see Table 1.2 for a detailed description of these variables), the share of same-subject applicants, and school-year fixed effects are included in all specifications. The coefficients on the teaching-award-combination dummies from the Panel (A) Column (1) specification are shown in Figure 1.2. Columns (2)(4)(6) control for the teacher-school-specific value-added of the applicants (normalized to have unit standard deviation). Columns (5) and (6) include applicant fixed effects. Coefficients are in terms of average marginal effects in the logit models (columns (1) & (2)). The expanded versions of Panel (A) and Panel (B) with coefficients on control variables are presented in Table A1 and Table A2 respectively.

Table 1.8: Do Principals Evaluate Socially Tied and Untied Applicants Differently along Other Characteristics?

Categorical Promotability Indices $\hat{\eta}_g^P$ $(g \in \mathbb{G})$	Socially Tied $\hat{\rho}_g^1$ (1)	Socially Untied $\hat{\rho}_g^0$ (2)	Difference $\hat{\rho}_g^1 - \hat{\rho}_g^0$ (3)
(1) Demographics	0.987***	1.015***	-0.028**
	(0.009)	(0.008)	(0.012)
(2) Experience	1.013***	0.991***	0.022*
	(0.0012)	(0.009)	(0.015)
(3) Workload	1.008***	0.979***	0.029*
	(0.014)	(0.012)	(0.017)
(4) Research	0.968***	1.037***	-0.069***
	(0.010)	(0.009)	(0.013)
(5) Teaching	0.976***	1.016***	-0.040***
	(0.006)	(0.005)	(0.007)
(6) Other	1.054***	0.962***	0.092***
	(0.013)	(0.010)	(0.016)
(7) Composite ($\hat{\eta}^P$)	0.989	1.010	-0.021*
	(0.009)	(0.008)	(0.012)
SocialTie		Y	
School-year FE		Y	
% same-subject applicants		Y	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses.

This table presents the estimates on the weights the school principals put on different charactertistics of applicants who are either socially tied or untied to them. The coefficient estimates $\left\{\hat{\rho}_g^1, \hat{\rho}_g^0\right\}_{g \in \mathbb{G}}$ on the categorical promotability indices (and $\{\hat{\rho}^0, \hat{\rho}^1\}$ on the composite index) of Equation (1.8) are reported. Categories of applicant characteristics include $\mathbb{G}$ ={demographics, experience, workload, research, teaching, other} (see Table 1.2 for a detailed description of these variables). Social tie index ($\hat{\eta}^{P,Tie}$), share of same-subject applicants and school-year fixed effects are included.

Table 1.9: Teachers' VA by Social Ties and Promotion Results/Application Decisions

	SocialTie=0	SocialTie=1	Difference
Promotees	0.996	0.671	0.325
Denied applicants	0	-0.271	0.270
Difference	0.996	0.942	
All applicants	0.147	0.075	0.072
Non-applying eligibles	-0.114	-0.129	0.015
Difference	0.261	0.204	
All eligibles	-0.007	-0.037	0.030

Notes: This table shows the mean teacher-school-specific VA of socially-tied-to-principal and untied teachers in groups defined by promotion results and application decisions. Eligibles are defined as the middle-ranked teachers who satisfy the mandatory experience and seniority requirement for senior rank application. The unit of analysis is the teacher-year, and school-year fixed effects are taken out. Specifically, the numbers in this table are the coefficient estimates from regression

$$\mathrm{VA}_{j,h(j,t)} = \sum_{p \in \mathbb{P}}^{\omega \in \{0,1\}} \mathbb{I}\left[\mathrm{SocialTie}_{j,P(j,t)} = \omega, p_{jt} = p\right] + \lambda_{h(j,t),t} + \varepsilon_{jt}$$

, where $\mathbb{P} = \{$Promotee, Denied applicants, Non-applying eligibles$\}$.

Table 1.10: Heterogeneities in Principals' Preferences over Applicant Characteristics

Categorical Promotability Indices $\hat{\eta}^P_g$ $(g \in \mathbb{G})$	Principal's Age $\hat{\rho}^{\text{age}}_{1g}$ (1)	$\hat{\rho}^{\text{age}}_{2g}$ (2)	Principal is Male $\hat{\rho}^{\text{female}}_{1g}$ (3)	$\hat{\rho}^{\text{male}}_{1g}$ (4)
(1) Demographics	0.991***	0.038***	1.032***	-0.024***
	(0.007)	(0.013)	(0.007)	(0.0010)
(2) Experience	1.013***	0.062***	1.017***	0.026***
	(0.008)	(0.013)	(0.009)	(0.012)
(3) Workload	1.021***	-0.058*	1.014***	-0.032**
	(0.009)	(0.017)	(0.012)	(0.014)
(4) Research	0.976***	0.084***	0.974***	0.051***
	(0.008)	(0.019)	(0.009)	(0.011)
(5) Teaching	0.974***	-0.385***	1.046***	-0.088***
	(0.004)	(0.008)	(0.004)	(0.005)
(6) Other	1.010***	-0.042**	0.990***	0.033***
	(0.010)	(0.019)	(0.009)	(0.012)
(7) Social Ties	0.987***	0.452***	0.783***	0.487***
	(0.003)	(0.008)	(0.004)	(0.005)
School-year FE	Y		Y	
% same-subject applicants	Y		Y	
# Principals	246		246	
Obs.	57,613		57,613	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses.

This table presents the heterogeneities in the principals' preferences over applicants' application profile characteristics in different categories and their social ties when deciding whom to recommend for promotion. Regression results of Equation (1.10) are shown. The coefficient estimates on categorical promotability indices (Definition (1.7)), $\left\{\hat{\hat{\rho}}^D_{1g}, \hat{\hat{\rho}}^D_{2g}\right\}_{g\in\{\mathbb{G},tie\}}$, are reported. Categories of applicant characteristics include $\mathbb{G}$ ={demographics, experience, workload, research, teaching, other} (see Table 1.2 for a detailed description of these variables), and principal types are $D \in$ {principal age, principal gender}. Age is de-meaned and scaled by $\frac{1}{10}$. $\hat{\hat{\rho}}^{\text{age}}_{1g}$ ($\hat{\hat{\rho}}^{\text{male}}_{1g}$) shows the average weight a mean-aged principal (a female principal) puts on the applicants' characteristics in the corresponding category (the weight put on each category by an average principal is normalized to 1), while $\hat{\hat{\rho}}^{\text{age}}_{2g}$ ($\hat{\hat{\rho}}^{\text{male}}_{2g}$) shows the additional weight put on this category by a 10-year older principal (a male principal).

Table 1.11: Distribution of Applicants by Promotion Results and Perceived Deservingness

	Unpromotable	Promotable
Promoted (22%)	Undeservingly Promoted (4.8%)	Deservingly Promoted (17.2%)
Not Promoted (78%)	Deservingly Denied (73.2%)	Undeservingly Denied (4.8%)

Notes: The unit of analysis is the teacher-applicant-year. N=6,797,345.

Table 1.12: What Do Survey Respondents Value in Applicants Compared to Principals?

Respondent Group (f)	Junior		Middle		Senior		Whole Sample
	Low VA	High VA	Low VA	High VA	Low VA	High VA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Demographics	1.094	1.132	0.998	1.109	1.109	1.078	1.095
	(0.142)	(0.143)	(0.113)	(0.115)	(0.140)	(0.138)	(0.077)
(2) Experience	1.151	1.063	1.104	1.043	1.155	1.115	1.106
	(0.171)	(0.173)	(0.138)	(0.136)	(0.163)	(0.164)	(0.092)
(3) Workload	1.107	1.140	1.084	1.168	1.169	1.126	1.131*
	(0.135)	(0.133)	(0.106)	(0.104)	(0.130)	(0.131)	(0.072)
(4) Research	0.832	0.793*	0.834*	0.827*	0.754**	0.768**	0.808***
	(0.121)	(0.123)	(0.098)	(0.097)	(0.115)	(0.114)	(0.063)
(5) Teaching	1.481***	1.543***	1.494***	1.528***	1.549***	1.602***	1.529***
	(0.114)	(0.113)	(0.086)	(0.084)	(0.112)	(0.109)	(0.061)
(6) Other	1.010	1.104	1.189	1.115	1.171	1.054	1.067
	(0.162)	(0.161)	(0.126)	(0.127)	(0.154)	(0.156)	(0.083)
(7) Social Ties	0.269***	0.248***	0.303***	0.290***	0.238***	0.217***	0.269***
	(0.132)	(0.132)	(0.102)	(0.103)	(0.134)	(0.132)	(0.068)
Mean of Coefficients	0.992	1.003	1.001	1.011	1.021	0.994	1.001
% same-subject applicants in virtual school	Y	Y	Y	Y	Y	Y	Y
Virtual School FE	Y	Y	Y	Y	Y	Y	Y
# Respondents	81	81	170	171	92	92	687
Obs.	2,276	2,309	4,801	4,829	2,593	2,584	19,392

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table looks at what categories of applicant characteristics the surveyed teachers value differentially more (or less) compared to school principals. Regression results of Equation (1.17) are shown. The coefficient estimates on categorical promotability indices constructed using the principals' promotion decisions (Definition (1.7)), $\{\hat{\varrho}_g\}_{g\in\{\mathbb{G},tie\}}$, are reported. Categories of applicants' application profile characteristics include $\mathbb{G}$ ={demographics, experience, workload, research, teaching, other} (see Table 1.2 for a detailed description of these variables). p-values are from the likelihood ratio tests of whether the estimated coefficients are equal to the mean of coefficients ($\frac{1}{7}\sum_{g\in\{\mathbb{G},tie\}}\hat{\varrho}_g$) in each column. Fixed effects for the virtual applicants' schools and share of same-subject virtual applicants in the virtual school are included. Columns (1)-(6) show the results for each of the 6 types of survey respondents by their professional ranks and within school-rank VA, and column (7) shows the results for the whole surveyed sample. A coefficient on the principals' promotability index of category g that is larger (smaller) than the mean implies the surveyed teachers value applicants' characteristics in category g more (less) compared to the principals.

Table 1.13: VA and Social Ties of Applicants by Promotion Results and Perceived Deservingness

	Teaching Quality (VA)	Social Tie	Hometown Tie	College Tie	Obs.
(1) Undeservingly Promoted	0.637	0.793	0.563	0.478	326,273
(2) Deservingly Promoted	1.137	0.295	0.213	0.181	1,169,143
p-value: (1)-(2)	[0.000]	[0.000]	[0.000]	[0.000]	-
(3) Undeservingly Denied	0.868	0.025	0.037	0.029	326,273
(4) Deservingly Denied	-0.069	0.327	0.228	0.192	4,975,656
p-value: (3)-(4)	[0.000]	[0.000]	[0.000]	[0.000]	-
All Applicants	0.217	0.329	0.232	0.196	6,797,345

Notes: This table shows the mean teacher-school-specific VA and the probability of sharing social ties with principals of the four types of applicants (Undeservingly Promoted, Deservingly Promoted, Deservingly Denied, Undeservingly Denied). The unit of analysis is the teacher-applicant-year.

Table 1.14: Heterogeneous Impacts of Promotion Unfairness by Principal's Length in Office

Year relative to Promotion Year	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	
Panel (A): Outcome Variable: Teachers' VA ($\text{VA}_{i,t+s}$)							
Undeserving%	0.023*	-0.031		-1.054***	-0.546***	-0.307***	435,998
	(0.053)	(0.048)		(0.044)	(0.049)	(0.053)	
Undeserving%×Length	0.014	-0.014	-0.011	0.038*	0.031	0.020	
	(0.023)	(0.021)	(0.019)	(0.020)	(0.023)	(0.024)	
Panel (B): Outcome Variable: Teachers' Quitting before Retirement ($\text{Leave}_{i,t+s}$)							
Undeserving%				0.0637***	0.0572***	0.0305***	435,998
				(0.0032)	(0.0037)	(0.0042)	
Undeserving%×Length				-0.0059***	-0.0091***	-0.0038*	
				(0.0018)	(0.0019)	(0.0022)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at teacher level are reported in parentheses. This table presents the heterogeneous impacts of perceived promotion unfairness on teachers' VA ($\text{VA}_{i,t+s}$) and job quitting probability ($\text{Leave}_{i,t+s}$) by how long the principal has stayed in the school at the promotion year ($\text{Length}_{P(i,t),t}$). The estimated coefficients from the regressions of Equation:

$$\begin{aligned} Y_{i,t+s} = \; & \textstyle\sum_{\tau=-3}^{2} \left(\left(\theta_{1\tau} + \theta_{2\tau} z_{i,t+s} \right) \text{Undeserving\%}_{it} + \theta_{3\tau} z_{i,t+s} \right) \times \mathbb{I}\left[s = \tau \right] + \sigma^{\theta} Y_{i,t-1} \\ & + g^{\theta}_{h(i,t)} \left(t+s \right) + \mathbf{Z}_{i,t+s} \beta^{\theta}_{i} + \lambda^{\theta}_{i,P(i,t)} + \lambda^{\theta}_{i,P(i,t+s)} + \varepsilon^{\theta}_{i,t+s}. \end{aligned}$$

on current Undeserving% (interacted with relative year dummies) and its interactions with $\text{Length}_{P(i,t),t}$ ($\left\{ \hat{\theta}_{1\tau}, \hat{\theta}_{2\tau} \right\}_{\tau=-3}^{2}$), are reported. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h\left(i,t+s\right) = h\left(i,t\right)$), are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0) principal fixed effects, applicant-current-principal fixed effects are controlled for.
This table presents the empirical test results of Predictions I.1) and II.1) in Sub-section 1.5.2.3.

Table 1.15: Effects of Applicant Information Disclosure on Favoritism and School Productivity

Outcome Variable	Teachers' VA VA_{it} (1)	Principals' Favoritism $Promoted_{jt}$ (2)	Perceived Unfairness $Undeserving\%_{it}$ (3)	CEE Scores A^{CEE}_{ckt} (4)
Post	0.076**		-0.247***	0.174***
	(0.009)		(0.017)	(0.018)
Post×Undeserving%	-0.953***			
	(0.029)			
(1-Post)×Undeserving%	-0.673***			
	(0.024)			
Post×SocialTie		0.135***		
		(0.0085)		
(1-Post)×SocialTie		0.264***		
		(0.0102)		
Unit of analysis	Teacher-year	Applicant-year	Teacher-year	Class-subject-year
Sample	Non-Applicants	Applicants	All Teachers	Graduating cohorts
Mean Dep. Var.	0.041	0.21	0.314	0
Pre-Reform Mean Dep. Var.			0.461	
SD Dep. Var.	0.932	0.41	0.128	1
Obs.	184,421	57,613	224,421	124,704

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors are reported in parentheses.

Column (1) reports the coefficient estimates from regression of Equation (1.23). It shows the difference in the incentive effects of perceived unfairness before and after the transparency reform. Standard errors are clustered at the teacher level. Lagged VA ($VA_{i,t-1}$), school-specific time trends, job characteristics with teacher-specific coefficients and teacher-principal fixed effects are controlled for.

Column (2) reports the coefficient estimates from regression of Equation (1.24). It shows the difference in the extent of favoritism by principals before and after the transparency reform. Standard errors are clustered at the applicant level. A logit model is used and the coefficients are in terms of average marginal effects. Controls of applicant characteristics, the share of same-subject applicants and school-year fixed effects are included.

Column (3) reports the coefficient estimate on *Post* ($\hat{\delta}$) from regression of Equation (1.25). It shows the difference in the extent of perceived promotion unfairness by teachers before and after the transparency reform. Standard errors are clustered at the teacher level. School-specific time trends and principal-school fixed effects are controlled for.

Column (4) reports the coefficient estimate on *Post* ($\hat{\delta}$) from regression of Equation (1.27). It shows the difference in the College Entrance Exams (CEE) scores of high school graduates before and after the transparency reform. Standard errors are clustered at the school level. School-specific time trends and principal-school fixed effects are controlled for.

Chapter 2

Income Targeting Daily Labor Supply: Evidence from Manufacturing Workers

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[1]I thank W.Bentley MacLeod, Pietro Ortoleva, Miguel Urquiola, Douglas Almond, Supreet Kaur, Miikka Rokkanen, Chris Hansman, Giovanni Paci, Bernard Salanié, Hongbin Li, Jamie Lien and participants of the applied micro theory colloquium and applied micro research method colloquium at Columbia University for invaluable comments and suggestions. All errors are my own.

2.1 Introduction

Knowledge of how individuals' labor supply is determined is important both intellectually and from a policy point of view in evaluating existing and designing future public taxes and transfers. Occupations that are self-employed or employed under piece rate contracts, where workers can adjust their labor supply instantaneously according to circumstances, are a good setting for studying their short-term labor supply decisions and the underlying behavior mechanisms. A seminal study by Camerer et al. (1997) on New York cab drivers proposes that the drivers' shift choices can be better explained by a hypothesis that they quit working upon reaching a target level of earnings, as opposed the neoclassical model of inter-temporal labor supply. There has been a debate on whether the daily income targeting hypothesis is valid and economically important (Farber, 2005, 2008, 2015; Crawford and Meng, 2011; Thakral and Tô, 2018). The main challenges lie in understanding what determines the target and in isolating exogenous variation of a worker's status around that target.

Using a panel dataset of 147 workers employed in six small manufacturing factories in China, this study provides field evidence on a daily-based mental wealth effect on workers' labor supply: unanticipated daily income shocks generated from lunch break card game gambling, which were cashed instantaneously, shifted workers' afternoon labor provision to the opposite direction, although wages were only paid at the end of each monthly pay cycle. The main observational finding is summarized in the left panel of Figure 2.1, which shows that the manufacturing workers increased (reduced) their afternoon productivity, which is equivalent to labor supply as piece rates were fixed, when they lost (won) money in the lunch break card games. This same-day effect, together with the finding that there was no cross-day labor supply response to these income shocks, indicates the existence of negative short-run, daily-bracketed psychological effect of windfall monetary rewards on labor supply, as opposed to the neoclassical model of either life-cycle-based or pay-cycle-based inter-temporal labor supply.

The key identifying assumption of interpreting the correlation between daily labor supply and cash rewards as a causal mental wealth effect is that the latter are (nearly as good as) random. That is, they should be independent of other demand and supply sides confounders that determine the equilibrium labor provision outcome. Such exogenous daily income shifters, as I will elaborate later, are difficult to find or isolate in natural empirical settings, and this is the main feature that distinguishes this study from most previous ones that investigate people's short-run labor supply decisions. I construct a panel dataset in which I can observe the daily production and daily gambling payoffs of 147 manufacturing factory workers in a Chinese city who worked on piece rate wages and voluntarily gambled in regular daily card games with co-workers during lunch breaks, in three periods ranging from 2011 to 2014. Card game gambling payoffs were largely due to luck by the nature of the game where the cards drew were important and there were no strictly dominant strategies for one player given the cards in his hand. In addition, I also conduct a set of robustness checks to validate empirically that card game winnings could be viewed as lotteries.

First, I analyze the gambling income distributions directly, both within-individual across-workdays and within-day across-individuals and find that actual gambling income realizations were unpredictable by observables in the information set and not significantly different from what a strategy-excluded pure-luck lottery would have delivered. Second, the labor provision outcomes in all the other neighboring days, both before and after the reference day, were not correlated with lunch gambling income, which rules out predictable across-day shocks that could have affected gambling performance and productivity simultaneously in opposite directions. In addition, there was a lack of correlation between the morning production and the lunch gambling winnings on the same day (see the right panel of Figure 2.1), excluding the threat of common shocks in an even narrower time window, and this sharp contrast between morning and afternoon production implies the lunch break shocks were highly unanticipated. Lastly, one might also be concerned that the effort a worker out in lunch break gambles might raise his game payoffs while hindering his afternoon working performance. To address this, I look at a period in one of the factories where the pay scheme was shifted to fixed daily rate from piece rate and find that the relationship between afternoon production and lunch break rewards was no longer existent. In summary, it is reasonable to assume that individual card game payoffs were exogenous to potential correlates with daily labor supply.

When focusing on people's labor supply decisions in the short run, the standard neoclassical inter-temporal model predicts that small and transitory income or wage shocks have literally no wealth effect as both the marginal utility of income and cost of effort stay unaffected, and only positive substitution effect exists in the wage elasticities of labor supply. Empirical evidence has casted doubt on whether the neoclassical model can explain people's daily labor supply when it can be freely chosen, starting from the seminal work by Camerer et al. (1997), which finds the working hours of New York taxi drivers decreased when the implied hourly wage rates were shifted up. The neoclassical model fails if short-run transitory earnings enter the marginal cost of effort, which is a form of *non-standard* preferences over leisure. One prevailing such non-standard preference models is reference dependence in how much to work and earn on a given day. Implied by Kahneman and Tversky (1979, 1992)'s prospect theory and formalized by Kőszegi and Rabin (2006), the two key features of this model are a reference point formed from expectation and loss averse preferences. In such models, there is a kink in the utility function at some reference level in terms of income, hours etc., and individuals are more sensitive to changes below that point than above it. Reference dependence in daily income can lead to so-called *targeting* behavior in labor supply, where workers are reluctant to work further hours when wage turns out to be high and their reference income level is met early on a day[2], in contrast to what the neoclassical substitution effect would predict.

Following Camerer et al. (1997), a recent literature has tried to answer the question whether income reference-dependent preferences exist and/or play an importantly role in

[2] Another well tested prediction of reference dependence is endowment effect, see Kahneman et al. (1990), List (2003), List (2004), Sprenger (2010), and DellaVigna (2009) for a review. Evidence of reference dependence is also found in job search (DellaVigna et al. (2014)).

labor supply decisions, as opposed to the standard neoclassical model. Most of the studies examine the short-run (or daily, in most cases) labor supply responses to transitory shocks in earning opportunities (that is, *wage* rates) using data from natural field settings, where high frequency daily variations in both earnings and hours were observed. The types of workers studied include taxi drivers (Farber, 2005, 2008, 2015), Crawford and Meng (2011), Chou (2002), Agarwal et al. (2013)), bike messengers (Fehr and Goette (2007), vendors (Oettinger, 1999), pear packers (Chang and Gross, 2014) and fishermen (Stafford, 2013). Empirical findings are mixed: some give positive evidence on daily reference dependence[3]; while others conclude that such targeting patterns were either non-existent or of very limited importance, both at the intensive margin (Farber, 2005, 2008, 2015; Stafford, 2013) and the extensive margin (Oettinger, 1999; Goldberg, 2016).[4].

There is one important limitation in the empirical target setting literature that studies natural wage-varying settings that hindered a clean test of reference dependence: the limited knowledge of how the target is set and how it interacts with the empirically exploited variations. Ideally one would like to directly observe the target; however such information is unavailable in most studies including this one, with the exception of Dupas and Robinson (2016) in which daily cash needs as a candidate for the targets were self-reported in surveys. Without observable targets we need to examine the theory of reference dependence more closely for possible alternative identifying assumptions. According to Kőszegi and Rabin (2006), the reference point is *expectation-based*, so any anticipated variations do not change one's position relative to the target but the target itself[5]; and when one is *far* away from the target, the kink in utility function is irrelevant and targeting behavior does not exist (see for example discussions in Farber (2015)). Therefore, the key to identifying the existence of

[3] For example Chou (2002), Fehr and Goette (2007), Crawford and Meng (2011), Agarwal et al. (2013) and Chang and Gross (2014).

[4] Some more recent works do not test the reference dependence model against the neoclassical model of labor supply directly, but examine more closely the market structure and learning by doing of New York cab drivers (Frechette et al., 2016; Haggag et al., 2017) and find that models without targeting behavior can also explain the empirical patterns well.

[5] Abeler et al. (2011) test for and support the expectation-based argument, using a lab experiment.

targeting behavior is to exploit (ex-ante) ***unanticipated*** shocks of appropriate size: that is, shocks that do not shift the target, but manipulate subjects' current status to different points in a neighborhood of the target. This issue has not been perfectly addressed in natural experiments with varying wage rates[6].

Experimental studies, in order to walk around the unanticipatedness concern, have been conducted more recently in which explicitly unexpected daily transitory wealth shifters were introduced. In Dupas and Robinson (2016) cash payouts were given on certain workdays to randomly selected bike taxi drivers in Kenya; and in Andersen et al. (2014) vendors in India were presented with windfall payments by mystery shoppers. In both settings, negative daily income effect, which is the direct implication of income reference dependence, is not found. However, it could be the case that the workers were reference-dependent, but these interventions were simply ***irrelevant*** in the sense that they are not components[7] of the latent variable on which the target is set[8], or more generally, the mental account (Thaler et al. (1997)). Relevance of a certain treatment is hardly statistically testable *ex-ante*, but conceptually verifiable *ex-post*, though not falsifiable, based on whether it had a causal impact on labor supply or not.

[6]A positive reduced-form wage elasticity does not rule out reference dependence, as it is possibly a combination of positive neoclassical substitution effect and negative wealth effect resulting from income reference dependence. Therefore it is critical to decompose wage variation into anticipated and unanticipated portions. However, existing decomposition exercises rely, somewhat arbitrarily, on explicit or implicit assumptions made on how people's expectations are formed as well as what is observable to econometricians. For example, Chang and Gross (2014) treat daily and weekly overtime as unexpected and expected wage shocks respectively; Farber (2015) regresses wage variation on controls including hour, week and holiday indicator and treats the predicted values as anticipated and the residuals as unanticipated, but whether workers take into account so many (or so limited) factors when forming their expectations about earning opportunities remains a question. Schmidt (2017) finds that New York cab drivers reduced hours in response to idiosyncratic abnormally large tips but increased labor supply to a positive earnings shock at the market level, but it is unclear whether the latter is anticipated or not.

[7]If the treatment variable on which intervention is made is a component of the latent variable, but the target changes one-to-one with the treatment, the intervention can be equivalently viewed as *irrelevant*. For example, windfall payments can be viewed as either irrelevant or totally absorbed by adjustment of the target. In other words, relevance implies that the target does not adjust one-to-one with the exploited shock.

[8]For example, Dupas and Robinson (2016)'s claims that the targets of the Kenyan bike drivers were over earned income rather than total income should be generalized to broader settings with caution, as it might have been the rareness and passiveness rather than the unearnedness nature of these windfalls that prevented the drivers from incorporating them into their "routine" daily income, which would have likely served as the latent variable for the daily targets (see Schmidt (2017)). More frequent shocks in unearned income, which people might take up actively, are still likely to affect their daily labor supply through targeting behavior.

Apart from unanticipatedness and relevance which are specific to the theory of reference dependence, a third crucial and more general identification assumption is the (ex-post) ***exogeneity*** of the exploited wage or income shocks. That is, the shocks should be uncorrelated with the error terms in the equation of observed labor supply, which can result from unobservables at both the demand and the supply sides. If the way in which the variations in daily wage or unearned income are realized or calculated is related to other unobserved determinants of labor supply, the estimates on the effect of reference dependence will be biased. Measurement error problem is a typical violation of exogeneity: if hourly wage is calculated by dividing income with hours, measurement error in hours will lead to downward division bias, which can yield negative wage elasticities even for workers with neoclassical preferences[9].

Taken jointly, there are three crucial assumptions needed for the exploited treatment variation to distinguish between a neoclassical model and a reference dependence model: **unanticipatedness, relevance** and **exogeneity**[10]. Violation of either relevance (such as infrequent windfall income payments in field experiments) or unanticipatedness (such as natural wage variations) can lead to type II error of failure to reject a false neoclassical model, while violation of exogeneity (such as measurement errors in wage rates) can result in type I error of rejecting a true neoclassical model. Most existing studies in the literature have some difficulty in validating least one of the assumptions.

The empirical context of this paper makes it possible to address all these concerns in a natural setting and make contributions to the literature. First, various empirical examinations suggest that daily lunch break gambling winnings were virtually unpredictable and thus very likely unanticipated ex-ante; also, the gambling income shocks were high-frequency and routinely taken up by the workers, and therefore relatively more likely to be relevant

[9]Solutions include instrumenting for calculated wages (Camerer et al. (1997)), estimating (structural) hazard models (Farber (2005), Farber (2008), Crawford and Meng (2011)) and using higher quality administrative data (Farber (2015)). However, the intrinsic problem remains whenever wage or hours are not directly measured without errors.

[10]While ex-ante unanticipatedness is unique to expectation-based reference dependence models, testing any short-run income-related non-standard cost of effort models in general requires relevance and exogeneity.

than infrequent transitory windfall interventions; moreover, labor supply was unambiguously measured as the workers were paid piece rates and evidence suggests that fatigue from gambling was not the main driving force in reduced afternoon labor provision.

Another advantageous feature of the setting is that the gambling income was realized instantaneously on each workday while labor income was realized on the monthly payday. This inconsistency in time at which labor and unearned income were paid enables me to rule out liquidity constraints or daily cash needs (Dupas and Robinson, 2016) that point to a non-behavioral smoothing failure story, and instead highlight the pure psychological "visceral" effect Loewenstein (1996) of compensating labor supply behavior when placed in gain or loss situations, which is independent of actual income realizations and consumption. It also adds to the narrow bracketing literature by showing that such mental effect was daily-based, a finding that is consistent with many of the previous studies in the short-run labor supply literature discussed above, as well as other studies on bracketing outside labor supply including Simonsohn and Gino (2013)[11].

This study is also related to the lottery literature, looking at the impact of transitory, small stake and high frequency "quasi-lotteries" on short-run labor supply. The previous works looking at the effect of large permanent lottery shocks find income elasticities of both short and long run labor supply that are also negative (Imbens et al., 2001; Cesarini et al., 2013), but their results are consistent with the standard neoclassical inter-temporal model of labor supply, while the negative elasticity in response to transitory shocks here points to the rejection of the model.

After establishing the negative causal impact of lunch break gambling income on afternoon production that rejects a neoclassical model of inter-temporal labor supply and implies psychological daily-bracketed behavior, I explore several possible forms of the non-standard preferences and the empirical patterns are most consistent with an income reference-dependent labor supply model where the reference point (that is, the target) is set on the virtual income

[11] Simonsohn and Gino (2013) show that the admission result of an MBA applicant was correlated with the pool of applicants her admission officer reviews on that certain day.

in one's daily *mental* account, which is defined as the *face-valued* sum of daily income from labor and non-labor sources, and consumption utility is additively separable across income and leisure (as in Kőszegi and Rabin (2006)) and risk neutral in daily and monthly accumulative income (as in Farber (2008, 2015)). This model outperforms a similar reference-dependent model with *real* (discounted) daily income targets as well as one that models daily-bracketing in the form of diminishing marginal utility in either mental or real daily income. The findings suggest that the workers put the face-valued unpaid labor income and (paid) unearned gambling income on the same day into one "*mental* account" (Thaler et al. (1997)) and exhibited income reference dependence over that account when deciding how much to work.

Using the preferred model, I estimate two structural models, one on choice of items to produce and the other a hazard model of stopping, with estimates of the coefficient of loss aversion, the key parameter in reference dependence, at around 1.8-2.0. The estimates lie in the range of 1.5 to 2.5 , as suggested by the literature[12] and are significantly different from the neoclassical value of 1. Furthermore, the individual-specific loss aversion estimates were positively correlated with survey measures of loss aversion replicating Abeler et al. (2011), further corroborating the role of loss aversion in the documented non-neoclassical daily labor supply.

Assuming that workers respond to unanticipated daily wage variation in general the same as to lunch break gambling income in this empirical setting and using the estimation results on loss aversion and Frisch elasticity, I also back out the implied wage elasticities of daily labor supply, the statistic commonly estimated in the literature, as a function of the fraction of the unanticipated component in the total daily wage variations using the model of Farber (2008, 2015).

The rest of the paper is organized as follows. Section 2.2 introduces the institutional backgrounds and data. Section 2.3 discusses gambling income as the treatment variable,

[12] See, for example, Kahneman and Tversky (1992), Abdellaoui et al. (2007) and Tovar (2009).

addressing concerns including sample selection and non-random payoffs. Section 2.4 outlines four competing models of daily labor supply in the spirit of Farber (2015) which are all nested in a uniform presentation of preferences, as well as several testable model predictions. Section 2.5 performs reduced-form tests for the competing models in Section 2.4. In Section 2.6, robustness checks regarding the identifying assumptions and daily bracketing behavior are presented. Section 2.7 shows reduced-form hazard model estimation results. Section 2.8 presents two structural models based on Section 2.4 as well as corresponding estimation results. Section 2.9 discusses wage elasticities and welfare implications based on preference parameter estimates in the previous section. Section 2.10 concludes.

2.2 Background and Data

I collected a panel dataset in 3 periods of a total length of 26 months (March 2011 to March 2012, December 2012 to April 2013, October 2013 to January 2014) of 147 male workers in 6 small private manufacturing factories in a southern city of China. These workers worked at a piece rate wage, and regularly participated in card game gambling with their co-workers during lunch breaks on about 60% of total workdays. Observations are at worker-workday level, consisting of information on individual daily morning and afternoon production, gambling participation status and income.

2.2.1 Factories, Pay Schemes and Production

In all the manufacturing factories, workers were paid for producing mechanical accessories or indoor decoration accessories at a piece rate of 4 to 6 RMB per item[13]. They respectively hired 19, 26, 30, 28, 27 and 17 workers, of whom 11 workers quitted and 8 entered during the periods I study. In each factory the production hours were the same for all workers:

[13]Except for Factory A in April and May 2014, which shifted its pay scheme in April 2014 from piece rate to a fixed daily rate of RMB 150. I exclude this period from the main analysis and use it for robustness check in Section 2.6.1.2

8:30am to 12:00pm in the morning, and 2:30pm to 5:30pm in the afternoon[14]. Managers would patrol the workshops from time to time and supervise the workers. As the hours were fixed and the procedure of manufacturing was standard, the margin at which the workers chose how much to work was how long to rest between steps of producing the same item and between consecutive items. The number of items each worker produced was counted and recorded at the end of the morning and the afternoon on each day, and workers were paid in cash once a month (usually at the end of each month) the amount which equals their monthly total production times piece rate. The period between the workday immediately the last payday and the reference payday is henceforth referred to as a pay cycle. The workers' daily production information comes from the factories' administrative records based on which their monthly pay was calculated. Table B1 gives a summary of the factories; and the first three rows of Table B2 reports summary statistics on the value of individual daily production (in RMB) for morning and afternoon respectively and combined, while the distributions were plotted in Figure B1.

2.2.2 Worker Characteristics

In January 2014, I conducted a survey on 130 of the 147 workers who were still in the factories, obtaining their basic demographic information and working histories including age, marriage, whether they have children or not, when they started working in the factories, when they first became a manufacturing worker, etc. In the survey I also replicated the design of Abeler et al. (2011) to solicit the workers' loss aversion levels. Specifically, I had the workers make six choices, each time between a fixed payment of zero and a small-stake lottery. The lottery involved a half-half chance of winning 6 RMB or receiving X, where X was varied from -7 to -2 RMB in steps of 1 RMB. The workers were told that one of the 6 lotteries would be randomly drawn by a die, and if the lottery was one that he chose over the fixed zero payment, a coin would be flipped to determine the lottery outcome with tail indicating 6

[14] The technological reason was that electricity supply in the workshops was uniformly turned on and off.

RMB. As noted by Rabin (2000), the small stakes mean that "the rejection of lotteries with positive expected value cannot be explained by standard risk aversion" (Abeler et al. (2011)); instead, the number of lotteries rejected gave an indicator of the individual's loss aversion (parameter λ in Section 2.4), which should be positively correlated with targeting behavior (if existing). The summary statistics on the number of lotteries rejected are reported in the last row of Panel B of Table B2.

2.2.3 Card Game Gambling

2.2.3.1 Gambling Participation and Income Recording

There was a 2.5-hour lunch break before production in each factory resumed in the afternoon. The workers usually did not return home during the break and had a meal nearby. After lunch some of the workers returned to the factories and gathered together to play zero-sum card games with stakes, or so-called gambling games. The card games were organized in each factory separately and the participants were mainly the workers in that factory, with the factory's managerial staffs and some outside people joining in occasionally[15]. From an interview with these workers, a major reason many workers cited for gambling participation was "card games were entertaining and there was nothing else to do before production resumed"[16], and they did not seriously expect to earn money from the games. Losing money in card games was not considered a stigma, and gambling was a routine activity for many of these workers. Figure B2 plots the individual specific frequencies of gambling participation[17], where we see that 132 of the 147 workers had a participation probability higher than 0.5.

[15] As I only include the production workers in my dataset, the sum of gambling income of all the gambling participating workers in a factory on a given day was not necessarily zero.

[16] This is an explanation why the loss averse individuals would participate in gambling. Card games were not like lotteries, and the workers might derive happiness from playing them *per se* (compared with staying idle for nearly two hours).

[17] Participation dummy equals one if the worker's gambling income record was found on the reference workday.

There were typically several "tables" and "rounds" in a what I call a "game" (factory-workday unit), and each worker could voluntarily join, leave or switch tables at any rounds in principle. Player payoffs (in cash) after each round were recorded by one of the participants at each table[18] before a final summation was done at the end of last round to determine the total wins and losses of each participant. The individual gambling payoffs of each card game were recorded on a page of a notebook (rather than on one sheet per day) for the purpose of future reference because although money was supposed to be paid instantaneously by losers to winners in the game, there were cases where some participants were short of cash and the balance was settled one or a few days afterwards[19]. I code each gambling participant's cumulative gambling income from all rounds he participated from the notebook pages on which the daily gambling results were put down[20]. Therefore, thanks to the unique method in which the card game payoffs were recorded, the workers' daily gambling wins or losses can be viewed as *administrative* records free of self-reporting issues[21]. Summary statistics are given in the last row of Panel A of Table B2.

2.2.3.2 Card Games

In this section I give a detailed description of how the card games were played. As mentioned in the last section, a game consisted of several tables and rounds, and workers could join and leave at any rounds and switch tables, though uncommon. Workers played approximately 10 rounds on a day.

[18]It was unlikely that the recorder cheated because the payoffs he put down was agreed upon by all, and each participant could remember (at least approximately) his wins and losses when the game closed for day so large inconsistencies would raise doubts.

[19]I was not able to identify such delayed payment cases because there was not a consistent way in which they were marked down in the notebooks. However, in an interview with these workers, they reported that money was paid immediately after the game ended in most cases, and delaying payment was considered unwelcome practice to be avoided.

[20]I was unable to record the player payoffs of each table-round of the games, and this "blackbox" of what actually happened within a game is a possible threat to the unanticipatedness and exogeneity assumptions on gambling income realizations. I argue with indirect evidence that this is not a big concern in Sections 2.3.2.2 and 2.6.1.

[21]It is worth pointing out that I cannot observe other possible gambling or lottery activities and payoffs of a worker outside the factory he worked in.

There were 4 players in each table, playing a deck of poker cards (52 cards) in each round. In each table-round, the cards were shuffled before they were drawn by players in turns, and eventually everyone had 13 cards at hand. Then players chose how to arrange their cards to get a three-row combination (3 (head)-5 (body)-5 (foot)), trying to form each row as some form such as Straight Flush, Four of a kind, Full House, Flush, Straight, Straight Flush, Three of a kind, Two Pairs, One Pair, Separate, etc. There were rules in which the same row (head, body or foot) of each player were pairwise comparable and the player with the "better" row won one point. After everyone finalized their card arrangement, they showed their cards simultaneously, each row of which were compared pairwisely with all the other 3 players. Each player could win 3 (winning all 3 rows), 1 (winning 2 rows and losing 1), -1 (winning 1 row and losing 2), -3 (losing 3 all rows) points from another player, and all the pairwise wins and losses are summed up to determine each player's overall payoff in the round. Each point was worth 2 RMB, so an individual's earnings from each round is two times the total points he won. The rules of such card game ensured that in each table-round there was no dealer[22] and the four players acted upon their individual interest, as there was no point in strategically cooperating with a subset of other players. The payoffs depended on the cards the players drew, which was due to luck, and how they arranged the cards, which involved skills and strategies. An illustrating sample of such a table-round is demonstrated in Appendix A.

From the description above one can see that there are two factors determining the gambling income of each worker in a game: the luck dimension (that is, the random card draws), and the choice dimension (that is, how many rounds one played, which table and therefore opponents he chose, and the way he arranged his cards at hand). Ideally one would like to isolate the luck component so that payoffs from card games are comparable to those from lotteries; however, as the observed gambling income was the sum of individual payoffs from multiple table-rounds of a game, it is infeasible to directly isolate luck from choices. Never-

[22]And there were no tricks in how cards were shuffled.

theless, I conduct a series of analyses in Sections 2.3.2 and 2.6.1, suggesting indirectly that the real gambling income results were not systematically different from pure lotteries.

2.3 Gambling Income as Treatment

As the day-to-day variation in individual gambling income is exploited as the treatment variable to identify reference-dependent versus neoclassical labor supply, we first need a good understanding about how gambling payoffs were assigned to different workers on different workdays. I examine gambling participation patterns (selection into treatment) in Section 2.3.1, and then analyze gambling income distributions conditional on participation (treatment on the treated) in Section 2.3.2, at both within-individual and within-game (workday) levels. Further examination of the gambling income treatment at within-worker-workday level with regard to the validity of unanticipatedness and exogeneity as discussed in Section 2.1, which is of equal if not higher importance, is presented in Section 2.6.1.

2.3.1 Selection into Gambling

Of all the 147 workers in the sample, I code 132 as frequent gamblers whose gambling participation probability was higher than a half. From Figure B2 we can see that there was a clear gap in participation frequencies between frequent gamblers and others. In most specifications of the empirical analysis I include only these frequent gamblers for whom gambling was a routine daily activity, as for infrequent gamblers the workdays on which they participated in card games were likely unusual to them due to daily factors either observable or not and thus not suitable for comparison.

Table B3 compares the worker characteristics of frequent gamblers and other workers. They have similar demographics and working histories, while the only significant difference is loss aversion: frequent gamblers were less loss averse on average, which makes sense because gambling in zero-sum games could generate loss around half of the time. However, the frequent gamblers' mean loss aversion level (3.6 lotteries rejected) was not much lower compared to the level in Abeler et al. (2011), suggesting that these gambling participating workers might still be comparable to an average person in the population in terms of loss aversion. It is worth pointing out that although within sample comparison shows little difference between gamblers and non-gamblers, I would not claim that the subjects considered in this paper were representative of male manufacturing workers, as the sample size of 147 is relatively small.

Restricted to frequent gamblers, Columns (1) and (2) of Table B5 show the variance decomposition results of the gambling participation dummy, denoted as G. Although factors including individual and factory-pay-cycle dummies, position of workday in a pay cycle, day of the week, potential opponents in games, daily morning production and so on are statistically significant in explaining whether workers participated in gambling on a certain workday, they only account for a very trivial fraction (5%). The first three rows of Table B4 show the differences in labor provision between their gambling days and other workdays. The differences were both very small in size and insignificant (or marginally significant), indicating the treatment-take-up workdays were likely similar to the non-take-up ones.

In summary, there is little identifiable selection into gambling, both across workers and across workdays within each worker.

2.3.2 Gambling Income Distributions

2.3.2.1 Within-Individual Distributions

The distributions of individual specific daily gambling income moments of frequent gamblers are shown in Figure B3. Individual means are all close to zero in the range from -10 to 10 RMB (Panel (A)), especially when compared with the variances ranging from 1000 to 2000 (Panel (B)). In additional to the small sizes, only 5 of the 132 frequent gamblers had an individual specific average gambling income significantly different from zero at 5% level (Panel (E)). Individual gambling income skewness are small in size and centered around zero from -0.4 to 0.4 (Panel (C)). If a gambler stops playing asymmetrically whenever he reaches a binding amount of loss (win) and not so for win (loss), his gambling income might be right (left) skewed; and the absence of large skewness suggests the workers did not exhibit heavily such within-game stopping rules. The excess kurtosis levels were mostly below zero (Panel (D)), meaning the tails of individual specific gambling income distributions were thinner than those of a normal distribution. This makes sense because the number of rounds and therefore the maximum wins and losses were finite, and that workers who stopped symmetrically when he reaching some binding amounts of both win and loss would have even lower kurtosis.

Figure B4 plots the relationships between individual gambling participation probabilities and gambling income moments, and shows that workers who gambled more frequently were not identifiably more successful (had higher means), more in control (had lower variance) in terms of their gambling income outcomes, nor did they exhibit different potential stopping patterns as embodied in the third and fourth moments of card game payoffs. Figure B5 shows the inter-relationships between different individual specific moments of gambling income, again exhibiting no pronounced patterns[23].

One might also imagine that gamblers who often played together might have some correlations between their individual gambling income distributions. To investigate such possibilities, I construct a pairwise proximity index between two frequent gamblers in the same factory: $proximity_{ij} = Pr(j\ participated|\ i\ participated)$. A higher proximity means the two workers were in general more often opponents in card games, although I cannot identify whether they were in the same table of each game. The relationships between pairwise proximity and pairwise average or difference in gambling income moments are plotted in Figure B6, and no correlations are detected.

[23]The only visible pattern is a negative correlation between mean and skewness, indicating a possible case where average losers in the card games were more likely to stop at a certain binding loss amount and not in a winning situation. However, as the skewness levels were very low, such patterns were very weak if existing.

2.3.2.2 Within-Game Distributions

As described in Section 2.2.3, I was able to observe only the card game (factory-workday) level outcomes and not players' choices within games: switching opponents between rounds, joining the game late or quitting early, as well as the workers' skills and strategies within a given table-round. However, these unisolatable non-luck factors might impose potential threats to the unanticipatedness and exogeneity assumptions on the gambling income variation. In this section, I simulate what the within-game gambling income distribution should look like when all these non-luck channels are shut down and gamble payoffs are randomly determined like lotteries[24]. Comparing simulation results with actual observations in the data can shed some light on the importance of non-random components (relative to luck) in the card games.

The simulation process is as follows:

If I observe L $(4 \leq L \leq 24)$ players in an actual card game, for its simulated counterpart I assume there are $\tilde{L} = 4\left(\left[\frac{L}{4}\right] + 1_{\{L \neq 0(mod\ 4)\}}\right)$ players[25], divided into $\left[\frac{L}{4}\right] + 1_{\{L \neq 0(mod\ 4)\}}$ tables of 4. Players at the same table play with each other for 10 rounds[26] without switching opponents[27]. Within each table-round, for each pair of players (i, j), $i \neq j$[28], it is a zero-sum lottery game. Specifically, player i loses (wins) 6 RMB to (from) player j with probability $\frac{1}{8}$, and 2 RMB with probability $\frac{3}{8}$, respectively[29]. The per-round payoff of player i is the sum of what he wins from all the other 3 players. The within-game payoff of each player i is the sum of the per-round payoffs from all the 10 rounds.

[24] I thank Doug Almond for suggesting this.

[25] L was sometimes not a multiple of 4, although each table had 4 players in the game. Possibilities include participation of unrecorded managerial staff, player replacement in the middle of the game and so on. I set $\tilde{L}$ as the smallest multiple of 4 that is larger than or equal to L.

[26] I interviewed several workers from these factories and asked about the usual number of rounds in which the card games were played during the lunch break, and 10 is the average number they gave. The workers also mentioned quitting in the middle and switching to other tables were uncommon.

[27] Through this I shut down cross table-round selection of opponents and strategic quitting.

[28] So there are $\frac{4 \times 3}{2} = 6$ pairs of players in each table-round.

[29] This probability distribution comes from the fact that in each table-round between each 2 players, 3 rows of cards are pairwise compared: for each row win, the winner gets 2 RMB. I assign half-half win/loss probabilities to each row comparison. Through this I shut down within table-round individual skills and strategies in card game playing.

Using the data generating process described above, I first simulate a $\tilde{L}$-dimensional within-game player payoff vector $\{GI_i^{sim.}\}_{i=1}^{\tilde{L}}$, from which I drop the last $\tilde{L} - L$ players[30] and obtain a new vector $\{GI_i^{sim.}\}_{i=1}^{L}$ which has the same dimension L as the actual within-game payoff vector $\{GI_i\}_{i=1}^{L}$. Suppose $\{GI_i^{sim.}\}_{i=1}^{L}$ and $\{GI_i\}_{i=1}^{L}$ are drawn from some L-dimensional multivariate distributions $F_L^{sim.}$ and $F_L^{act.}$ respectively, the goal is to examine whether $F_L^{act.}$ is close to $F_L^{sim.}$. If yes, the gambling income pattern at the game level is similar to that of a lottery drawn using the rules of the actual game, suggesting that the non-luck components did not dramatically render within-game-level payoff patterns. In order to include all the actual games for comparison, I simulate $\{GI_i^{sim.}\}_{i=1}^{L}$ n_L times, where n_L is the number of actual games in which there were L players (see Panel (A) of Figure B7).

After simulation, I compare the sample distribution of the simulated payoff vector, $\hat{F}_L^{sim.}$, with that of the actual payoff vectors, $\hat{F}_L^{act.}$, for each actual game size level $4 \leq L \leq 24$, using the cross-match test for the equality of two multivariate distributions proposed by Rosenbaum (2005). The results are shown in Table B6. For most card game size levels, , the p-value from the cross-match test indicates that the distributions of the actual and the simulated payoff vectors are not significantly different from each other. I also plot the normalized[31] kernel average within-game-level distribution of players' gambling income, of both the actual gambling income observations in the data and the simulated payoffs. As shown in Panel (B) of Figure B7, the two distributions are similar.

The within-individual level analysis, together with the within-game level simulation exercise, provides suggestive evidence that actual treatment assignment of gambling income was largely unpredictable and not significantly different from what a pure-luck lottery would look like. However, concerns regarding the unanticipatedness and exogeneity of gambling

[30] As simulated per table-round payoff distributions are random, it does not matter which players I drop.

[31] Each GI observation is weighted by $\frac{1}{nL}$, where L is the number of players of the game the worker participated, and n is the total number of games played in the sample. In this case the sum of weights is 1 and the plotted within-game-level distribution is comparable to the distribution of a univariate random variable.

income are not resolved because there might be within-worker-workday level non-random unobservables affecting both daily gambling income and production, which are threats to identification but uncaptured here. In fact, invariant individual specific components can be absorbed in individual fixed effects, and within-game gambling payoff distributions might mask player specific heterogeneities in each game. I will address this point in Section 2.6.1.

2.4 Competing Models of Daily Labor Supply

In this section, I model individual's preferences over daily labor supply and income in a similar form to Farber (2015), in which the neoclassical model and different versions of reference dependence are nested special cases nested. Empirically testable predictions are also presented.

Consider the preferences of a given worker on a given day $t$in a given pay cycle. Let I_t^L, I_t^U denote his labor income and unearned income on that day. Consistent with the empirical context, I_t^U (gambling income) is paid out instantaneously, and I_t^L is realized on the payday $\bar{t}$ at the end of each monthly pay cycle and should be discounted when one stands on the reference workday. T_t denotes his income target, either for the pay cycle ($T_t = T, \forall 1 \leq t \leq \bar{t}$), , or for the current day, which is what multiple studies in the literature suggest. Assume *relevance* holds: that is, I_t^U enters the latent income variable over which the target is set by the worker. In the setting where the worker is paid a piece rate, I_t^L, which equals piece rate times the number of items produced, can also be viewed as his total labor effort.

The worker's daily utility on day t is a function of his income on t and all preceding workdays $\left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t}$, his target T and his preference parameters, which is written as $U\left(I_t^L, I_t^U \left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t-1} |T; \lambda, \nu, \theta, \beta, \delta\right)$. The parameter $\lambda \geq 1$ is the coefficient of loss aversion that reflects the discontinuous drop in marginal value of income upon hitting the target, θ indexes the disutility of labor effort, $\frac{1}{\nu}$ is the Frisch elasticity of labor supply, and (β, δ)

represents the hyperbolic and exponential discount factors from Laibson (1997).

I assume that income enters the utility function linearly and separably from effort as in Farber (2015), which implies constant marginal utility from daily income and the absence of income effect without income targeting. The rationale is the following: first, as each day and each monthly pay cycle are short compared to the life-long cycle, neoclassical income effect for a consumption smoothing worker should be nil; also, as the labor provision and rewards were not aligned in time, working more on non-payday t ($t < \bar{t}$) cannot ease daily liquidity constraint even if it exists, and liquidity constraint is the main reason why consumption is not smoothed and curvature in the daily utility of income exists. Albeit unlikely, I also consider the form of daily bracketing preference with diminishing marginal return to income, which is possibly driven by liquidity constraint not a result of reference dependence *per se*, in Appendix D[32]. The analysis shows that relative risk aversion needs to be at least larger than 4 to yield the documented negative elasticities, which is unrealistically high according to Chetty (2006).

- **Model (1): Neoclassical Preference**

A neoclassical worker does not consider the target in his preference, so T irrelevant. This is the special case of Model (2) "pay cycle income reference dependence" or Model (3) "daily real income reference dependence" where $\lambda = 1$.

$$U\left(I_t^L, I_t^U \left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t-1} |T; \lambda, \nu, \theta, \beta, \delta\right) = \beta^{1\{t<\bar{t}\}} \delta^{\bar{t}-t} I_t^L + I_t^U - \frac{\theta}{1+\nu}\left(I_t^L\right)^{1+\nu}. \tag{2.1}$$

- **Model (2): Pay Cycle Income Reference Dependence**

The worker is at a loss when his accumulated income in the pay cycle has not surpassed the pay cycle target, where one additional RMB brings $\lambda \geq 1$ units of happiness.

[32] I thank Chris Hansman for suggesting this.

$$U\left(I_t^L, I_t^U \left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t-1} |T; \lambda, \nu, \theta, \beta, \delta\right) = \left[\lambda 1_{\left(\sum_{l=1}^{t}\left(I_l^L+I_l^U\right)-T<0\right)} + 1_{\left(\sum_{l=1}^{t}\left(I_l^L+I_l^U\right)-T\geq 0\right)}\right] \cdot \left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t} I_t^L + I_t^U\right) - \frac{\theta}{1+\nu}\left(I_t^L\right)^{1+\nu}. \tag{2.2}$$

- **Model (3): Daily Real Income Reference Dependence**

The worker is at a loss when his real daily income (in which the labor income component I_t^L , realized some time in the future, is discounted) is below his daily target.

$$U\left(I_t^L, I_t^U \left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t-1} |T; \lambda, \nu, \theta, \beta, \delta\right) = \left[\lambda 1_{\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t} I_t^L+I_t^U-T_t<0\right)} + 1_{\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t} I_t^L+I_t^U-T_t\geq 0\right)}\right] \cdot \left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t} I_t^L + I_t^U\right) - \frac{\theta}{1+\nu}\left(I_t^L\right)^{1+\nu}. \tag{2.3}$$

- **Model (4): Daily Mental Face-Valued Income Reference Dependence**

In the worker's daily mental account of virtual income, the face values of labor and unearned income are grouped together. The worker is at a loss when his mental daily income (in which the labor income component is not discounted) is below his daily target. This is the special case of Model (3) "daily real income reference dependence" where $\beta = \delta = 1$.

$$U\left(I_t^L, I_t^U \left\{I_\tau^L, I_\tau^U\right\}_{\tau=1}^{t-1} |T; \lambda, \nu, \theta, \beta, \delta\right) = \left[\lambda 1_{\left(I_t^L+I_t^U-T_t<0\right)} + 1_{\left(I_t^L+I_t^U-T_t\geq 0\right)}\right] \cdot \left(I_t^L + I_t^U\right) - \frac{\theta}{1+\nu}\left(I_t^L\right)^{1+\nu}. \tag{2.4}$$

After daily lunch break gambling income (I_t^U) is realized, afternoon production (I_t^L) is the only choice variable, set at I_t^{L*} to maximize the daily utility function U. If gambling income satisfies *unanticipatedness*, we have $\frac{\partial T_t}{\partial I_t^U} = 0$. The predictions of the neoclassical model and the three reference dependence models, regarding the relationship between labor supply and daily non-labor income shocks, are summarized in Table 2.1, which will all be brought to empirical tests in Section 2.5. It is worth pointing out that as long as target

T does not vary with unearned income I^U, T being observable or not is not critical for empirical identification.

For a worker with neoclassical preference (Model (1)), unanticipated unearned income I_t^U should not have an effect on their daily labor supply as it is trivial compared to his life-long income.

For a reference-dependent worker with a daily mental face-valued income target (Model (4)), income on all the other workdays is irrelevant (Prediction (2)). The response of such worker's daily labor supply to the same-day non-labor income shock, as a function of the value of the shock, is given in Prediction (5): when I_t^U is large in size, $\frac{\partial I_t^{L*}}{\partial I_t^U} = 0$; and when I_t^U is small, there is one-to-one compensation: $\frac{\partial I_t^{L*}}{\partial I_t^U} = -1$. This is essentially the same as Farber (2015): only when the unexpected earning shifters are small can they manipulate workers *around* their daily target and affect their labor supply decisions. Moreover, although $\frac{\partial I_t^L}{\partial I_t^U} = -1$ whenever $\frac{\partial I_t^L}{\partial I_t^U} < 0$ regardless of the loss aversion parameter λ, more loss averse people have a larger *range* of I_t^U in which $\frac{\partial I_t^{L*}}{\partial I_t^U} = -1$, so $E\left[\frac{\partial I_t^{L*}}{\partial I_t^U}\right] = -Pr\left(T_t - \left[\frac{\lambda}{\theta}\right]^{\frac{1}{\nu}} \leq I_t^U \leq T_t - \left[\frac{1}{\theta}\right]^{\frac{1}{\nu}}\right) \in (-1, 0)$ (Prediction (1)), and $\partial E\left[\frac{\partial I_t^{L*}}{\partial I_t^U}\right] / \partial \lambda < 0$ (Prediction (4)). Also, as income is not discounted, it does not matter how far away in the future current labor supply gets rewarded in cash (Prediction (3)).

For a reference dependent worker with a daily real income target (Model (3)), the predictions are similar to Model (4) except for Prediction (3). If the distribution of I_t^U does not vary across days[33], I_t^L is on average more responsive to I_t^U on days that are further away from the payday (larger $\bar{t} - t$), because the value of producing each extra item to compensate for a given daily unearned income loss is discounted more heavily (lower $\beta^{1_{\{t<\bar{t}\}}} \delta^{\bar{t}-t}$) and therefore more extra production is needed.

For a reference dependent worker with a pay cycle income target (Model (2)), his daily labor supply I_t^L is responsive to I_t^U on the workday where his accumulated pay cycle income passed the target and unresponsive on all the other workdays; and on this responsive work-

[33]Prediction (3) for Model (3) might not hold if I_t^U becomes less disperse as t increases.

day his labor supply is equally responsive to unearned income shocks on all the preceding workdays in the pay cycle ($I_\tau^U, \tau < t$) because they enter the kinked marginal utility from income in Equation (2.2) in the same way as the shock on the current workday (Prediction (2)).

2.5 Empirical Tests of Daily Labor Supply Models

In this section I bring all the model predictions in Table 2.1 to data and present reduced form regression results in support of the "daily mental face-valued income reference dependence" model as opposed to the other three.

The empirical analogue of daily unearned income shifter I_t^U is lunch break gambling income (denoted as GI), and that of daily labor income I_t^L is daily total production in monetary terms (denoted as TP) or afternoon production (denoted as AP). Under piece rate I_t^U also measures effort.

- **Prediction (1)**

The estimation equation is given by:

$$TP_{it} = \alpha GI_{it} + \mu_{it}^{ind} + \mu_{it}^{dow} + \mu_{it}^{f\times pc} + \sum_{j\neq i} I\left[G_{jt} = 1\right] + \mu_{it}^{dtp} + \varepsilon_{it} \tag{2.5}$$

where i and j index worker and workday respectively, and μ^{ind}, μ^{dow}, $\mu^{f\times pc}$, μ^{dtp} and $I\left[G_{jt} = 1\right],\ j \neq i$ denote individual, day-of-the-week, factory-pay-cycle, number-of-days-to-payday ($\bar{t} - t$), and card game opponents fixed effects respectively. The empirical analogue of $E\left[\frac{\partial I_t^L}{\partial I_t^U}\right]$ is coefficient α. As only afternoon production was supposed to respond to lunch break gambling income and morning production (denoted as MP) could serve as a control for worker-workday specific production shifters, I also estimate:

$$AP_{it} = \alpha GI_{it} + \gamma MP_{it} + \mu_{it}^{ind} + \mu_{it}^{dow} + \mu_{it}^{f\times pc} + \sum_{j\neq i} I\left[G_{jt} = 1\right] + \mu_{it}^{dtp} + \varepsilon_{it} \tag{2.6}$$

Main results are reported in Columns (1)-(2) and (5)-(6) of Table 2.2 where the sample only includes the gambling workdays of frequent gamblers. The estimates of coefficient α are stable at -0.37 and significantly negative using either Equation (2.5) or Equation (eq: est 2, II), indicating that the neoclassical model of labor supply (Model (1)) cannot explain the labor supply behavior of these manufacturing workers. Columns (3) and (4) show evidence that morning production was not correlated with lunch break gambling income, which is consistent with the assumption that the latter was unanticipated.

Table 2.3 presents robustness checks for the estimation of Equation (2.5). Column (2) includes infrequent gamblers; Column (3) includes the non-gambling workdays of frequent gamblers and the estimate can be viewed as the intent to treat; Column (4) excludes turnover workers whose production might be abnormal; Column (5) performs within-factory-workday comparison of gambling participants and non-participants and the estimate can be viewed as difference in difference. The estimates of α stay very stable across all the specifications, corroborating the finding that $E\left[\frac{\partial I_t^L}{\partial I_t^U}\right] < 0$ and that the neoclassical model is not supported in the empirical setting.

- **Prediction (2)**

The estimation equation is the same as (2.5), except that gambling income is not that on the current workday but on the preceding gambling days of the same pay cycle:

$$TP_{it} = \alpha_\tau GI_{i\tau} + \mu_{it}^{ind} + \mu_{it}^{dow} + \mu_{it}^{f\times pc} + \sum_{j\neq i} I\left[G_{jt} = 1\right] + \mu_{it}^{dtp} + \varepsilon_{it},\ \tau < t \tag{2.7}$$

The empirical analogue of $E\left[\frac{\partial I_t^{L*}}{\partial I_\tau^U}\right], \tau < t$ is α_τ, the estimates of which are plotted in Figure 2.2. We can see that the daily production was unresponsive to the daily gambling income even on last gambling workday ($\tau = t-1$), let alone days further in the past. As $E\left[\frac{\partial I_t^{L*}}{\partial I_\tau^U}\right] = 0, \forall\tau < t$, the "pay cycle income reference dependence" model (Model (2)) is ruled out empirically.

It is obvious that a variant of Prediction (2) for Model (2) is $E\left[\frac{\partial I_t^{L*}}{\partial accI_{t-1}^U}\right] = E\left[\frac{\partial I_t^{L*}}{\partial I_t^U}\right]$, where $accI_{t-1}^U$ is the accumulated non-labor income up to workday $t-1$ in the pay cycle. Therefore I also replace current day gambling income with the accumulated level in the pay cycle and estimate:

$$TP_{it} = \alpha_{-1}\sum_{\tau=1}^{t-1} G_{i\tau} \times GI_{i\tau} + \mu_{it}^{ind} + \mu_{it}^{dow} + \mu_{it}^{f\times pc} + \sum_{j\neq i} I\left[G_{jt}=1\right] + \mu_{it}^{dtp} + \varepsilon_{it} \tag{2.8}$$

Estimates of coefficient α_{-1}, which is the analogue of $E\left[\frac{\partial I_t^{L*}}{\partial accI_{t-1}^U}\right]$, are reported in Table 2.4. It is shown that the point estimates, albeit significantly different from zero in some specifications, are very small in size and in no way comparable to the estimates of α in Equation (2.5), which represents $E\left[\frac{\partial I_t^{L*}}{\partial I_t^U}\right]$. The results are consistent with Figure 2.2.

- **Prediction (3)**

Although delay in gambling income payment was possible and not observable, Prediction (3) is different for Model (3) and Model (4) unless all gambling income payments were delayed to the monthly payday, which was unlikely. I now estimate the following equation, allowing for different levels of $E\left[\frac{\partial I_t^{L*}}{\partial I_t^U}\right]$ on workdays with different number of days to the payday ($dtp := \bar{t} - t$) across a pay cycle. The estimated coefficients α_{dtp} are plotted in Panel (B) of Figure 2.3. Visually, α_{dtp} is independent of $\bar{t} - t$, which is confirmed statistically by the fact that a joint F test of hypothesis $\alpha_{dtp} = \alpha$ yields a p-value of 0.2919. :

$$TP_{it} = \sum_{0\leq dtp\leq 30} \alpha_{dtp} GI_t I\left[\bar{t}^{f\times pc} - t = dtp\right] + \mu_{it}^{ind} + \mu_{it}^{dow} + \mu_{it}^{f\times pc} + \sum_{j\neq i} I\left[G_{jt}=1\right] + \mu_{it}^{dtp} + \varepsilon_{it} \tag{2.9}$$

In order to rule out the "daily real income reference dependence" model (Model (3)), an additional assumption is needed that distribution of gambling income GI_{it} is independent of the position of the workday in its pay cycle ($\bar{t} - t$). I plot the standard deviations of gambling income across a pay cycle, $\sigma_{GI,\bar{t}-t}$ in Panel (A) of Figure 2.3, which validates this intra-pay-cycle invariance assumption. Taken jointly, Model (3) is also inconsistent with the data[34].

- **Predictions (4) and (5)**

The "daily mental face-valued income reference dependence" model (Model (4)), the sole survivor from the previous tests, is also tested against Predictions (4) and (5). I estimate Equation (2.5) (without μ_{it}^{ind}) for each frequent gambler i respectively, and plot the coefficients of interest, α_i, against their survey measures of loss aversion (see Section 2.2.2), which represents λ, in Figure 2.4. The negative correlation is consistent with Prediction (4).

Local polynomial regressions of daily total and afternoon production on lunch break gambling income are shown in Panels (A) and (C) of Figure 2.5 . The result is consistent with Prediction (5) that production was responsive only when the gambling payoffs were of small magnitudes.

2.6 Robustness Checks

2.6.1 Gambling Income a Valid Treatment?

This section follows the discussion in Section 2.3.2 and focuses on possible worker-workday specific confounders that might threaten the key identifying assumptions. Relevance of gambling income to move workers around their daily mental target is validated from the presence of negative wealth effect documented in Section 2.5. Here I examine the other two assumptions, namely unanticipatedness and exogeneity.

2.6.1.1 Unanticipatedness

Variance decomposition results of daily gambling income, the "treatment" variable, are presented in Columns (3) and (4) of Table B5, where I include possibly relevant anticipatable

[34]Further corroboration is presented in Section 2.9, where structural estimates of β and δ are not different from one.

observables that were known to individuals before gambling income was realized on each workday, such as individual and factory-pay-cycle dummies, distance to payday, day of the week, card game opponents, morning production, previous gambling payoffs, etc. None of these components are significant at 5% level, and they can explain only 2% of the total variance in gambling income, suggesting that gambling income was unanticipated against *observables.*

Table B7 decomposes the variances in workers' daily production. The observable anticipatable components together explained 66% of the variance in morning production which was set before lunch break gambling, and only 7% in afternoon production. In sharp contrast, the gambling income explained zero variance of earnings in the morning versus 36% in the afternoon. It is unlikely that unexplained variation, if anticipated, should explain largely different proportions of morning and afternoon production by one individual within the same day relative to anticipated observables ($\frac{1-66\%-0\%}{66\%} = 2$ for morning and $\frac{1-7\%-36\%}{7\%} = 8$ for afternoon). Therefore, it is reasonable to infer that *unobservable* determinants of daily labor supply were largely unanticipated and hence independent of possible income targets.

The sharp contrast between how the workers' morning and afternoon labor supply were correlated with gambling income which was realized during the lunch break, as shown in Panels (B) and (C) of Figure 2.3, is also evidence that card game payoffs were unanticipated: if they were, workers should have already responded in the morning.

2.6.1.2 Exogeneity

As both gambling payoffs and production were precisely measured, the common division bias concern in the literature is irrelevant here. It is clear that gambling payoffs were independent of labor demand shifters. However, the way in which gambling income was realized might have other *ex-post* effect on the workers' subsequent labor supply decisions other than shifting workers to different daily income positions relative to their targets. One plausible story is that higher mental input in card games could increase the probability of winning, and therefore winners exerted higher effort in the card games on average and the resulted "fatigue" decreased their afternoon production. If this is the case, the response I find might not be due to workers' pecuniary income targeting incentives, but instead to a story of "limited vigor".

To address this concern, I exploit a natural experiment in the pay rule in Factory A to construct a counterfactual. In April 2014, Factory A changed from piece rate to a fixed daily wage of 150 RMB. Under the new wage policy workers had no pecuniary incentive to work more/less in response to their gambling income, because the number of products they made would not make any difference to their labor earnings. If the negative correlation between gambling income and production did not disappear, it might have been the limited vigor story at play. I re-run Equation (2.5) for those observations in factory A in April and May 2014, the results for which are shown in Columns (3) and (4) of Table 2.6. A difference-in-difference estimation comparing the daily rate period with the piece rate period is reported in Columns (5) and (6). The estimates of α in both specifications were not significantly different from zero, which means that under the fixed daily wage scheme the workers' afternoon production did not respond to their gambling payoffs. The finding is consistent with the story that the gambling income effects came from the money they got from the gamble, rather than efforts they put in it.

2.6.2 Mental Income Target Daily-Bracketed?

It is suggested from Section 2.5 that the workers' mental account, on which a reference point in the face-valued income was imposed, was daily bracketed, a result that is consistent with existing findings documented in the literature. However, the result could also be consistent with the case in which the workers grouped into one mental account the face-valued income not within a workday, but between two consecutive draws of gambling payoffs. In this case, labor income in the morning immediately after the gambling day should be in the same account as the gambling income, because the next draw was realized during the next lunch break at the earliest. I test the between-two-gambling-income-draw bracketing hypothesis against the daily bracketing one by looking at labor earnings in the morning on the same day and on preceding and subsequent workdays. I re-estimate Equation (2.5), replacing the left hand side variable TP_{it} with MP_{it}, $AP_{i,t\pm\tau}$, $MP_{i,t\pm\tau}$ ($\tau = 1, 2, 3$) and plot the coefficient estimates α's with 95% confidence intervals in Panel (B) of Figure 2.6. One can see that except for the same day afternoon, labor earnings in all the nearby time windows are not correlated with the lunch break gambling income. This finding supports that the mental income targeting of the manufacturing workers was indeed daily based.

2.7 Reduced-Form Hazard Model of Production Stopping

Farber (2005)'s estimation of a hazard model yields opposite conclusions on income targeting to wage elasticity estimates inCamerer et al. (1997), even using the same dataset on taxi drivers. Although the data on manufacturing workers do not suffer from similar limitations in the setting of taxi driver, and that the real labor supply decision made by the manufacturing workers in this study was on intensity of effort (or productivity) rather than when to stop (see Section 2.2.1), I also estimate a linear hazard model of stopping for robustness in this section. I follow Farber (2005, 2008) and Crawford and Meng (2011) and view the workers' choice of afternoon labor supply as a stopping decision: whether to continue producing the next item after finishing the current one.

As reference dependence patterns considered in Models (2) and (3) have been ruled out in Section 2.5, the hazard model corresponds to "daily mental face-valued income targeting"(Model (4)), between which and the neoclassical model (Model (1)) the estimation results should be used to distinguish. Worker i on workday $t$after producing the jth piece in the afternoon stops production on that day with following linear probability:

$$\Pr\left(Stop_{itj}=1|I^L_{itj},I^U_{it},T_{it}\right)= \begin{aligned}[t] &\gamma_1\left(I^L_{itj}+I^U_{it}-T_{it}\right)+\gamma_2 1_{\left\{I^L_{itj}+I^U_{it}-T_{it}\geq 0\right\}} \\ &+\gamma_{12}1_{\left\{I^L_{itj}+I^U_{it}-T_{it}\geq 0\right\}}\left(I^L_{itj}+I^U_{it}-T_{it}\right) \\ &+\phi_1 I^L_{itj}+\phi_2\left(I^L_{itj}\right)^2+\mu^{ind}_{it}+\mu^{dow}_{it}+\mu^{f\times pc}_{it}+\eta_{itj}\end{aligned} \tag{2.10}$$

where $I^L_{itj}=MP_{it}+w_i j$ is the face-valued daily cumulative labor income (w_i denotes piece rate) and serves as the proxy for cumulative labor effort, $I^U_{it}=GI_{it}$ is the unearned gambling income during lunch break, $I^L_{itj}+I^U_{it}$ is the cumulative daily total *mental* face-valued income, T_{it} is the worker and day specific mental income target. The coefficients of interest are γ_1,γ_2 and γ_{12}, which measure the increment in stopping probability when the worker's accumulated mental income approached and hit his daily target. The neoclassical model predicts that $\gamma_1=\gamma_2=\gamma_{12}=0$, because neither the marginal benefit nor the marginal cost of labor is affected by the cumulative income or its position relative to a certain target. Model (4) predicts that $\gamma_2>0$ (and $\gamma_1,\gamma_{12}>0$ if the target T_{it} is measured with errors), as the marginal benefit of labor jumped from $\lambda>1$ to 1 when the target was surpassed. If the daily target T_{it} is proxied with errors, which is true almost certainly, estimates of γ_2 will be attenuated towards zero.

I use six different proxies for the daily mental face-valued income target[35]: Target 1 for worker i on the tth workday in a pay cycle is the average value of daily total mental income on preceding workdays in the same pay cycle ($T^1_{it}=\frac{1}{t-1}\sum_{\tau=1}^{t-1}\left(I^U_{i\tau}+I^L_{i\tau}\right)$); Target 2 is that average value on preceding same-pay-cycle gambling workdays ($T^2_{it}=\frac{1}{\sum_{\tau=1}^{t-1}I[G_{i\tau}=1]}\sum_{\tau=1,G_{i\tau}=1}^{t-1}\left(I^U_{i\tau}+I^L_{i\tau}\right)$); Target 3 is that average value on preceding same-day-of-week workdays ($\frac{1}{\sum_{\tau=1}^{t-1}I[G_{i\tau}=1]}\sum_{\tau=1,G_{i\tau}=1}^{t-1}\left(I^U_{it}+I^L_{it}\right)$); Target 4 is that on preceding same-day-of-week gambling workdays; Target 5 is the value of individual's daily total mental income on last workday; and Target 6 is that value on last gambling workday. The results are reported in Table 2.7. We see that across all specifications, estimates of coefficients $\gamma_1,\gamma_2,\gamma_{12}$ are significantly greater than zero, which is consistent

[35]Proxies for targets are not required for identifying reference-dependent against neoclassical daily labor supply, as long as the daily income shifter is unanticipated (see Section 2.5). However, to estimate hazard models for robustness, we need explicit target measures.

with Model (4). The point estimates of γ_2 are smaller for Targets 5 and 6 compared to the others, which indicates the former are noisier proxies.

I also follow Dupas and Robinson (2016) and allow stopping probabilities to vary freely across all items j and estimate the following equation:

$$\Pr\left(Stop_{itj}=1|I^L_{itj},I^U_{it},T_{it}\right)=\sum_{\substack{p=-10\\p\neq-1}}^{10}\gamma_p 1_{\{j=J_{it}+p\}}+\phi_1 I^L_{itj}+\phi_2\left(I^L_{itj}\right)^2+\mu^{ind}_{it}+\mu^{dow}_{it}+\mu^{f\times pc}_{it}+u_{itj} \tag{2.11}$$

where $J_{it}=\left[\frac{T_{it}-I^U_{it}-MP_{it}}{w_i}\right]+1$ is the minimum number of items worker i needed to produce to reach his daily mental income target T_{it}. The coefficients γ_p's represent the stopping probability when the last item finished was p items excess of what was needed to reach the target (compared to the case where $p=-1$). The results are shown in Figure **??**, which shows that the probability of stopping jumped upwards when the worker reached the daily target (from $p<0$ to $p\geq 0$).

In summary, estimation results from reduced form linear hazard models support the daily mental face-valued income reference dependence" model as opposed to the neoclassical model of labor supply.

2.8 Structural Models of Daily Labor Supply

In this section, I lay out two versions of structural models of daily labor supply based on the utility function (2.3)of Model (3) (daily real income reference dependence) in Section 2.4. Model (1) is the special case of Model (3) where $\lambda=1$, and Model (4) is the special case where $\beta=\delta=1$, hence the estimation results would help distinguish between the neoclassical, the daily real income targeting and the daily mental face-valued income targeting models of labor supply. As the models allows for real income targets, for daily targets T_{it}, I use the discounted variants of Targets 1 and 2, That is, Target 1 is the sum of individual's average discounted daily labor income and non-discounted gambling income on preceding same-pay-cycle workdays ($\tilde{T}_{it}^1 = \frac{1}{t-1}\sum_{\tau=1}^{t-1}\left(I_{i\tau}^U + \beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}I_{i\tau}^L\right)$); Target 2 is the same sum of measure, but on gambling workdays ($\tilde{T}_{it}^2 = \frac{1}{\sum_{\tau=1}^{t-1} I[G_{i\tau}=1]}\sum_{\tau=1,G_{i\tau}=1}^{t-1}\left(I_{i\tau}^U + \beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}I_{i\tau}^L\right)$).

2.8.1 Continuous Choice of Afternoon Production

In this section, I model the workers' afternoon labor supply choice as a choice made after the realization of lunch break gambling income on how many items to produce in the afternoon (or equivalently, the afternoon productivity). According to Section 2.4, the *theoretically optimal* afternoon labor supply (in monetary term) was:

$$AP_{it}^*\left(GI_{it}, T_{it}, MP_{it}; \lambda, \nu, \theta, \beta, \delta\right) = argmax_{I_{it}^L} U\left(I_{it}^L, I_{it}^U\left\{I_{i\tau}^L, I_{i\tau}^U\right\}_{\tau=1}^{t-1}|T_{it}; \lambda, \nu, \theta, \beta, \delta\right) - MP_{it} \tag{2.12}$$

where U is given in Equation (2.3) and $GI_{it} = I_{it}^U$. Assume that the workers' *real optimal* afternoon labor supply is the theoretical value plus some individual and workday specific noise:

$$\tilde{AP}_{it} = AP_{it}^* + \xi_{it}, \tag{2.13}$$

where $\xi_{it} = \mu_{it}^{\xi} + \epsilon_{it}^{\xi}$, $\epsilon_{it}^{\xi} \overset{i.i.d}{\sim} N\left(0, \sigma_{\xi}^2\right)$, and $\mu_{it}^{\xi} = c^{\xi} + \left(x_{it} - \bar{x}\right)\beta^{\xi}$. Note that μ_{it}^{ξ} is demeaned. x_{it} includes win/loss dummies, individual characteristics, day-of-the-week dummies, factory-pay-cycle dummies and distance-to-pay-day dummies ($\bar{t}-t \leq 10,\ 10 < \bar{t}-t \leq 20, \bar{t}-t \geq 20$). As the number of items produced must be an integer, I need to round the real optimal choice of labor supply to the nearest integer, which is the *real choice* of afternoon labor supply:

$$AP_{it} = w_i\left(\left[\frac{\tilde{AP}_{it}}{w_i}\right] + 1_{\left(\tilde{AP}_{it} - w_i\left[\frac{\tilde{AP}_{it}}{w_i}\right] > 0.5\right)}\right), \tag{2.14}$$

where w_i is the piece rate the worker earned. The probability that worker i on day t chose afternoon production $AP_{it} = a$ is:

$$\begin{aligned}
\Pr\left(AP_{it} = a\right) = & \Pr\left(a - \tfrac{w_i}{2} < \tilde{AP}_{it} \leq a + \tfrac{w_i}{2}\right) \\
& = Pr\left(a - \tfrac{w_i}{2} - AP^*_{it} - \mu^\xi_{it} < \epsilon^\xi_{it} \leq a + \tfrac{w_i}{2} - AP^*_{it} - \mu^\xi_{it}\right) \\
& = \Phi\left(\tfrac{a + \frac{w_i}{2} - AP^*_{it} - \mu^\xi_{it}}{\sigma_\xi}\right) - \Phi\left(\tfrac{a - \frac{w_i}{2} - AP^*_{it} - \mu^\xi_{it}}{\sigma_\xi}\right).
\end{aligned} \tag{2.15}$$

The likelihood function is therefore written as follows:

$$\begin{aligned}
& \mathscr{L}\left(AP_{it}, MP_{it}, GI_{it}, T_{it}; \lambda, \theta, \nu, \beta, \delta; \sigma_\xi, c^\xi, \beta^\xi\right) \\
= & \prod_i \prod_{t \in \{t|G_{it}=1\}} \left\{\Phi\left(\tfrac{AP_{it} + \frac{w_i}{2} - AP^*_{it} - \mu^\xi_{it}}{\sigma_\xi}\right) - \Phi\left(\tfrac{AP_{it} - \frac{w_i}{2} - AP^*_{it} - \mu^\xi_{it}}{\sigma_\xi}\right)\right\}.
\end{aligned} \tag{2.16}$$

I then use maximum likelihood estimation to estimate parameters $\left(\lambda, \theta, \nu, \beta, \delta; \sigma_\xi, c^\xi, \beta^\xi\right)$, using Target 1 and 2 as the proxies for T_{it}.

2.8.2 Binary Choice of Stopping

I also model the workers' afternoon labor supply choice as a binary choice of stopping, corresponding to the reduced form version in Section 2.7. Unlike the taxi drivers whose hourly wage from the next trip of a shift is uncertain, the piece rate means manufacturing workers knew exactly what their compensation for producing would be. Therefore, there is no uncertainty to the expected marginal utility from producing an additional piece, and if I stick to Equation (2.3) and the probability of stopping should be either zero or one without errors, making it impossible to estimate a hazard model. To address this problem, I modify the deterministic utility function by separably adding a noise term[36] for simplicity. For worker i in the afternoon of day t after completing jth piece in the afternoon, his utility is given by:

[36]This disturbance can be viewed as the uncertainty in the additional cost of effort from producing one more piece of item.

$$U\left(I_{itj}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)=\left[\lambda 1_{\left\{\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}I_{itj}^{L}+I_{it}^{U}-T_{it}<0\right\}}+1_{\left\{\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}I_{itj}^{L}+I_{it}^{U}-T_{it}\geq 0\right\}}\right]\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}I_{itj}^{L}+I_{it}^{U}\right)-\frac{\theta}{1+\nu}\left(I_{itj}^{L}\right)^{1+\nu}+\sum_{l=1}^{j}\zeta_{itl}, \tag{2.17}$$

where cumulative total income $I_{itj}^{L}=MP_{it}+w_i j$ and cumulative labor effort $I_{it}^{U}=GI_{it}$ are defined the same way as in Section 2.7. $\sum_{l=1}^{j}\zeta_{itl}$ is the cumulative disturbance in the afternoon to the utility. I further assume $\zeta_{itl}=c^{\zeta}+\left(x_{it}-\bar{x}\right)\beta^{\zeta}+\epsilon_{itl}^{\zeta}$, and $\epsilon_{itl}^{\zeta}\overset{i.i.d}{\sim}N\left(0,\sigma_{\zeta}^{2}\right)$. x_{it} includes the same set of controls as in Section 2.8.1.[37] ϵ_{itj}^{ζ}, and therefore ζ_{itj}, $\sum_{l=1}^{j}\zeta_{itl}$ and $U\left(I_{itj}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)$, are observed after producing the j-1th item and before the jth. The worker's stopping decision is given by:

$$stop_{itj}=\begin{cases}1, & if\ U\left(I_{it,j+1}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)-U\left(I_{itj}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)<0,\\ 0, & U\left(I_{it,j+1}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)-U\left(I_{itj}^{L},I_{it}^{U}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)\geq 0.\end{cases} \tag{2.18}$$

In Appendix B, I show that the likelihood function is given by:

$$\begin{aligned}&\mathscr{L}\left(j,MP_{it},GI_{it},T_{it};\lambda,\theta,\nu,\beta,\delta;\sigma_{\zeta},c^{\zeta},\beta^{\zeta}\right)\\ =\ &\prod_i\prod_{t\in\{t|G_{it}=1\}}\prod_{j=1}^{J_{it}}\Pr\left(stop_{itj}=1\right)^{stop_{itj}}\Pr\left(stop_{itj}=0\right)^{1-stop_{itj}},\end{aligned} \tag{2.19}$$

where

$$Pr\left(stop_{itj}=1\right)=\Phi\left(\frac{F_{itj}}{\sigma_{\zeta}}\right)\prod_{l=1}^{j-1}\left[1-\Phi\left(\frac{F_{itl}}{\sigma_{\zeta}}\right)\right],$$

$$\begin{aligned}F_{itj}=\ &\frac{\theta}{1+\nu}\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}\right)^{1+\nu}\left[\left(MP_{it}+w_i\left(j+1\right)\right)^{1+\nu}-\left(MP_{it}+w_i j\right)^{1+\nu}\right]\\ &-\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}w_i-\left(\lambda-1\right)A_{itj}-\left[c^{\zeta}+\left(x_{it}-\bar{x}\right)\beta^{\zeta}\right],\end{aligned}$$

and

[37] I cannot observe piece specific information analogous to the trip specific information in the New York taxi drivers datasets.

$$
\begin{aligned}
A_{itj} = \; & 1_{\left\{\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}(MP_{it}+w_i(j+1))+GI_{it}-T_{it}<0\right\}}\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}\left(MP_{it}+w_i\left(j+1\right)\right)+GI_{it}\right) \\
& -1_{\left\{\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}(MP_{it}+w_i j)+GI_{it}-T_{it}<0\right\}}\left(\beta^{1\{t<\bar{t}\}}\delta^{\bar{t}-t}\left(MP_{it}+w_i j\right)+GI_{it}\right)
\end{aligned},
$$

and $J_{it} = AP_{it}/w_i$ is the total number of pieces produced in the afternoon.

I then use maximum likelihood estimation to estimate parameters $\left(\lambda, \theta, \nu, \beta, \delta; \sigma_\zeta, c^\zeta, \beta^\zeta\right)$, using Target 1 and 2 as the proxies for T_{it}.

2.8.3 Estimation

2.8.3.1 Common Preference Parameters across Individuals

I use maximum likelihood estimation to estimate parameters $\left(\lambda, \theta, \nu, \beta, \delta; \sigma_\xi, c^\xi, \beta^\xi\right)$ for the continuous choice model in Section 2.8.1 and $\left(\lambda, \theta, \nu, \beta, \delta; \sigma_\zeta, c^\zeta, \beta^\zeta\right)$ for the hazard model in Section 2.8.2, using the sample of frequent gamblers' gambling workdays.

The results are shown in Tables **??** and **??** respectively. In columns (1) and (4), I impose restrictions $\beta = \delta = 1$ and $\beta^\xi\left(\beta^\zeta\right) = 0$, which corresponds to Model (4) with unconditional noises $\xi\left(\zeta\right)$. In Columns (2) and (5), I keep restricting $\beta = \delta = 1$ and free $\beta^\xi\left(\beta^\zeta\right)$, which corresponds to Model (4) with conditional noises. In Columns (3) and (6), I allow time preferences (β, δ) to be free parameters and impose $\beta^\xi\left(\beta^\zeta\right) = 0$ for simplicity, which corresponds to Model (3). p-values from likelihood ratio tests are reported in parentheses.

For all specifications, estimates of the coefficient of loss aversion λ lie between 1.8 and 2.0, which are stable, significantly larger than the neoclassical value $\lambda = 1$ and consistent with estimates ranging from 1.5 to 2.5 in the literature[38]. This result, consistent with all the reduced form findings in Sections 2.5 and 2.7, empirically rejects the neoclassical model of labor supply for these manufacturing workers. The estimates of inverse Frisch elasticity ν lie between 1.8 and 2.1, implying a Frisch elasticity in the range of $(0.45, 0.55)$, which is close to the suggestion in Chetty et al. (2011). Hyperbolic discount factor β and exponential discount

[38] See, for example, Kahneman and Tversky (1992), Abdellaoui et al. (2007) and Tovar (2009).

factor δ are not significantly different from 1 while significantly different from $\beta = 0.7$ and $\delta = 0.957$ as suggested by Angeletos et al. (2001), which supports Model (4) where reference dependence works for daily mental face-valued income, as opposed to Model (3) where the target is set on discounted real income.

2.8.3.2 Individual-Specific Preference Parameters

I then estimate the two structural models for each frequent gambler respectively, using Target 1 as the proxy for T_{it}, imposing $\beta = \delta = 1$ (Model (4)) and allowing β^{ξ} $\left(\beta^{\zeta}\right)$ to take non-zero values, to obtain individual specific preference parameters $(\lambda_i, \theta_i, \nu_i)$. The results are plotted in Figures 2.8 and 2.10. In both models, the estimates of loss aversion parameters λ_i are greater than 1 for all individuals (Panels (A) and (B) of Figure 2.8), and correlate positively both with each other (Panel (C) of Figure 2.8) and with the survey measures of loss aversion (Panels (A) and (B) of Figure 2.10); the estimates of inverse Frisch elasticity ν_i and effort cost θ_i from the two models are positively also correlated (Panels (D) to (I) of Figure 2.8). These results further substantiate the reliability of the structural estimates of utility function parameters, as well as Model (4) as the preferred model of daily labor supply to explain the behavior of the gambling manufacturing workers.

Pairwise correlations of $(\lambda_i, \theta_i, \nu_i)$ are plotted in Figure 2.9. Individual specific coefficient of loss aversion is not correlated with the Frisch elasticity (Panels (A) and (D)), while workers with higher loss aversion and higher Frisch elasticity tended to have higher cost of effort (that is, produce less and earn lower labor income). Again, these correlations are restricted to the sample of workers and not generalized to other contexts.

Plugging the individual specific parameters in the two structural models, I simulate the daily afternoon production values for each worker-workday observation, taking morning production (MP_{it}), gambling income (GI_{it}) and mental target (T_{it}, proxied using Target 1 in Section 2.7) as given. The simulated results are compared to real daily afternoon production values in Figure 2.11, exhibiting good fits for both models.

2.9 Wage Elasticities and Forgone Earnings

The welfare implications of reference-dependent preferences depend on whether the targeting behavior reflects true expected utility or is a mistake (Kőszegi, 2010). Income targeting, as an inefficient way to make money when faced with wage variation, diminished with experience among the New York City cab drivers, as they learned they had been forgoing earning opportunities (Farber, 2015). This was an indication that for these cab drivers targeting was a mistake incurring welfare loss, which was later corrected. As the manufacturing workers were exposed to daily unearned income variation, targeting led to reallocation of labor across workdays when gambling payoffs were different and did not necessarily imply loss in efficiency. And not surprisingly, their targeting behavior persisted. It is unclear whether income targeting would diminish with experience were these workers placed in a wage-varying setting, which is more common than non-labor income shocks in real life. However, in this section I assume that income reference dependence is persistent under daily wage rate shocks, and try to back out the *composite* wage elasticities of daily labor supply, which are of direct policy relevance, using the structural estimates of loss aversion parameter λ and Frisch elasticity of labor supply $\frac{1}{\nu}$.

According to Equation (13) in Section VI of Farber (2015), worker's daily labor supply responds to log unanticipated wage variation $reference-dependently$ with elasticity $\varepsilon^{RD} = -1$ in a range of length $\frac{\ln\lambda}{1+\nu}$, and *neoclassically* with $\varepsilon^{Frisch} = \frac{1}{\nu}$ outside this range. Suppose that log daily unanticipated wage is normally distributed and accounts for $\alpha \in (0,1)$ fraction of the total variance σ^2 in log daily wage, in Appendix C I show that the composite daily wage elasticity is:

$$\varepsilon^{comp.} = \underbrace{-\alpha\left[2\Phi\left(\frac{\ln\lambda}{2\left(1+\nu\right)\sigma\sqrt{\alpha}}\right)-1\right]}_{\text{RD elasticity } \varepsilon^{RD} \text{ (-)}} + \underbrace{\frac{1}{\nu}\left[1+\alpha-2\alpha\Phi\left(\frac{\ln\lambda}{2\left(1+\nu\right)\sigma\sqrt{\alpha}}\right)\right]}_{\text{Neoclassical elasticity } \varepsilon^{Neo.} (+)} \qquad (2.20)$$

I use the lower bound estimate $\lambda = 1.8$ and the upper bound estimate $\nu = 2.05$ from

Table 2.8[39], and plot the implied reference-dependence and composite daily wage elasticities for different levels of (α, σ^2) in Panels (A) and (B) of Figure 2.12 respectively. A higher α and a lower σ^2 yield a lower $\varepsilon^{comp.}$ for given loss aversion λ and Frisch elasticity ν. It can also be shown that the decrease in expected daily labor income when a worker switches from a neoclassical to an income targeting earner, keeping ν unchanged, is $\frac{\alpha}{2} \ln \lambda$. Plugging in $\lambda = 1.8$, the expected labor income is around $30\alpha\%$ higher as a neoclassical earner labor supplier.

If placed in the situation of the New York City cab drivers where $\alpha = \frac{1}{8}$ and $\sigma^2 = 0.05$ (Table II in Farber (2015)), the implied reference-dependent daily wage elasticity of the manufacturing workers is around -0.1; and the implied composite daily wage elasticity is around 0.3, which is a little lower than Farber's reduced form estimates ranging from 0.4 to 0.8, and they would forgo around 4% of total potential earnings by being income reference-dependent.

2.10 Conclusion

Using the unanticipated daily income shocks from lunch break gambling which were uncorrelated with other labor demand and supply shifters, this paper provides field evidence on around 130 Chinese manufacturing workers supportive of a reference dependence model of daily labor supply where the target was on the *mental* account consisting of the face-valued sum of daily labor and unearned income, as opposed to the neoclassical model and other forms of reference dependence with *real* income targets. The estimated coefficient of loss aversion, derived from two well-fitted structural models, is around 2, which is significantly different from the neoclassical value of 1 and also consistent with estimates in the literature. The finding is further validated by the fact that individuals' solicited survey measures of loss aversion are positively correlated with both the reduced form and structural estimates

[39] A lower λ and a higher ν yield smaller magnitude of negative reference dependence wage elasticity. Therefore the elasticity values recovered should be viewed as upper bounds.

from reference-dependent models. The estimates of loss aversion and mental income targeting behavior are likely lower bounds, as the workers' production responses to the unearned income shocks were institutionally constrained in terms of the limit to the total hours they were able to work and the extent to which they could shirk as a hired employee rather than a self-employer.

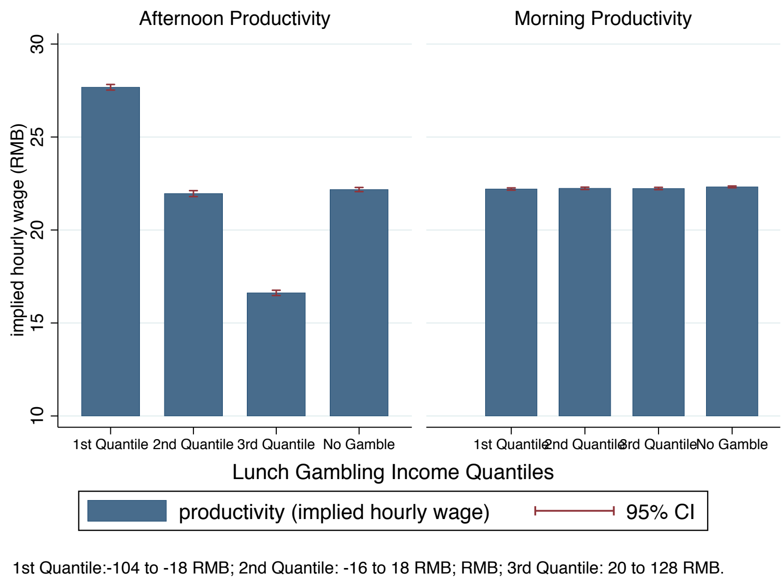
Using the estimates of the coefficient of loss aversion and Frisch elasticity of daily labor supply, I also back out the implied reference-dependent and composite daily wage elasticities as functions of the daily variance of log wage and the fraction of unanticipated components in this variance.

Compared to this paper, the majority of previous studies in the literature either exploited natural wage rate variations (such as the cab driver literature) of which the unanticipated component was difficult to unambiguously isolated, or conducted field experiments where non-labor income interventions were rare, passively accepted and likely not incorporated by individuals as routine income. The empirical setting in this paper is superior in the sense that the utilized daily earning shifters satisfied all of the three key assumptions (relevance, unanticipatedness and exogeneity) for identifying reference-dependent against neoclassical labor supply models. In addition, the non-alignment between labor provision and labor income payment in time enables me to shed some light on where reference dependence is originated: it could be generated solely from some visceral effects resulting from *mental* loss, which has nothing to do with liquidity constraints or cash needs in Dupas and Robinson (2016).

Different from Farber (2015), I conclude that reference dependence on faced-valued daily mental income played a relevant role in determining the labor supply of the Chinese manufacturing workers in the dataset who were daily card game gamblers.

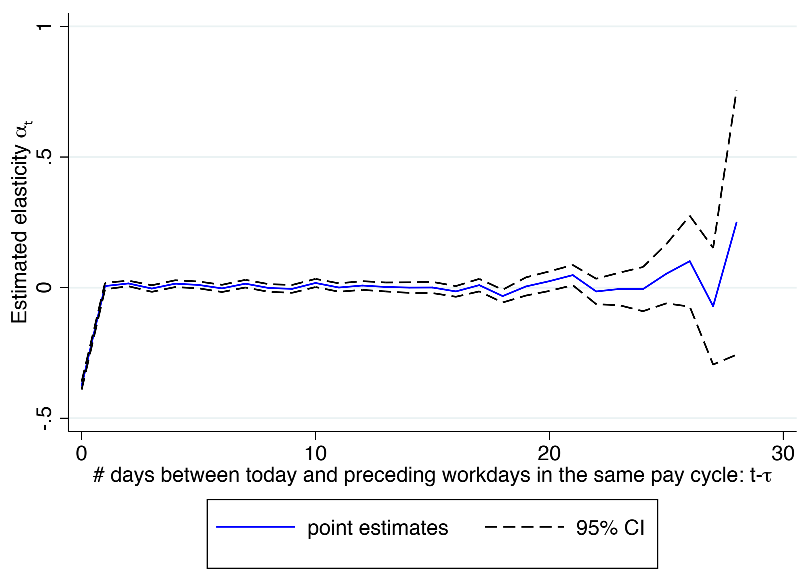
2.11 Figures

Figure 2.1: Lunch Break Income and Daily Labor Productivity



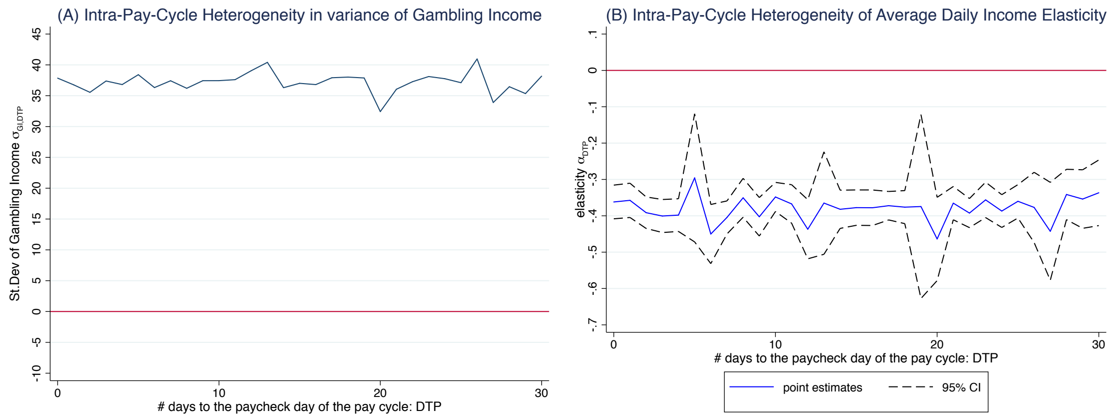
Notes: Only frequent gamblers ($Pr(G = 1) \geq 0.5$) are included.

Figure 2.2: Intra-Pay-Cycle Different-Day Non-Labor Income Elasticities



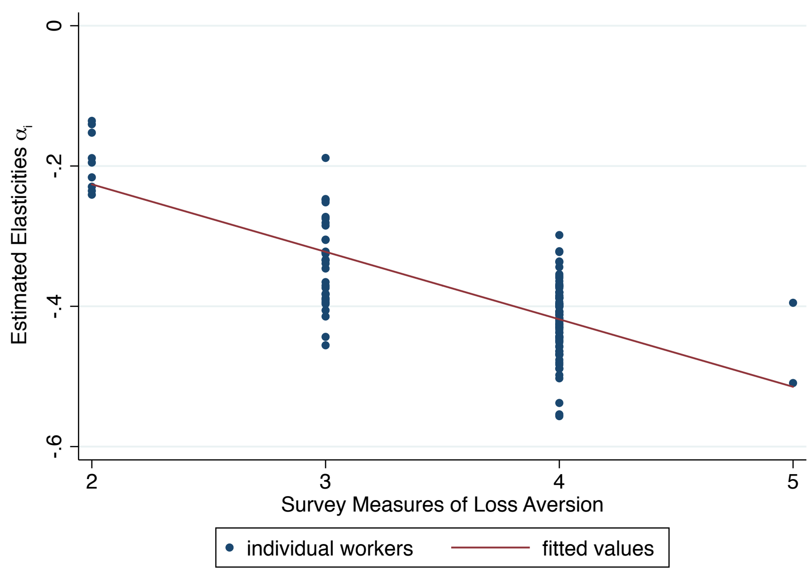
Notes: Estimated coefficients from estimation Equation (2.7). This result rules out Model (2).

Figure 2.3: Intra-Pay-Cycle Heterogeneity of Average Daily Non-Labor Income Elasticities



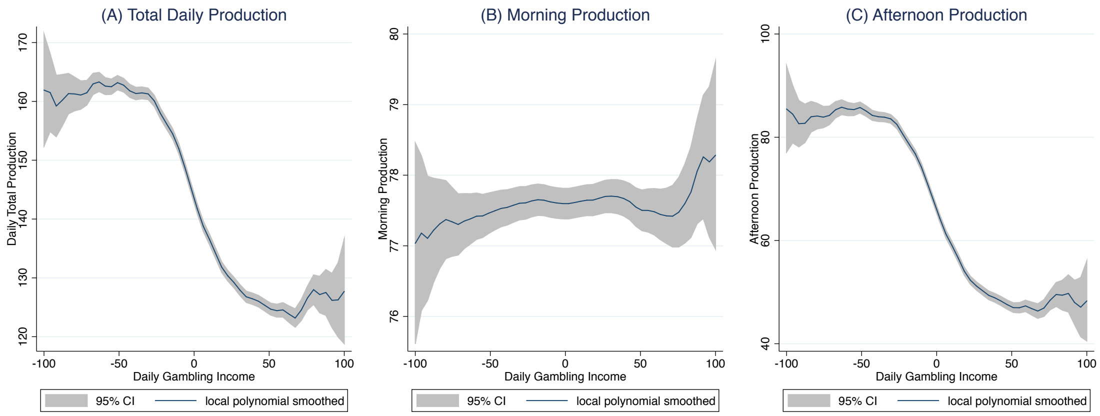
Notes: Panel (B) # days-to-payday specific estimated coefficients from estimation Equation (2.5). Coefficients for different days within a pay cycle are not significantly different from each other (p-value=0.2919). This result rules out Model (3).

Figure 2.4: Heterogeneous Local Daily Non-Labor Income Elasticities of Labor Supply



Notes: Individual specific estimated coefficients from estimation Equation (2.5).

Figure 2.5: Heterogeneous Local Daily Non-Labor Income Elasticities of Labor Supply



Notes: Local polynomial regressions.

Figure 2.6: Time Horizon: Daily Targeting

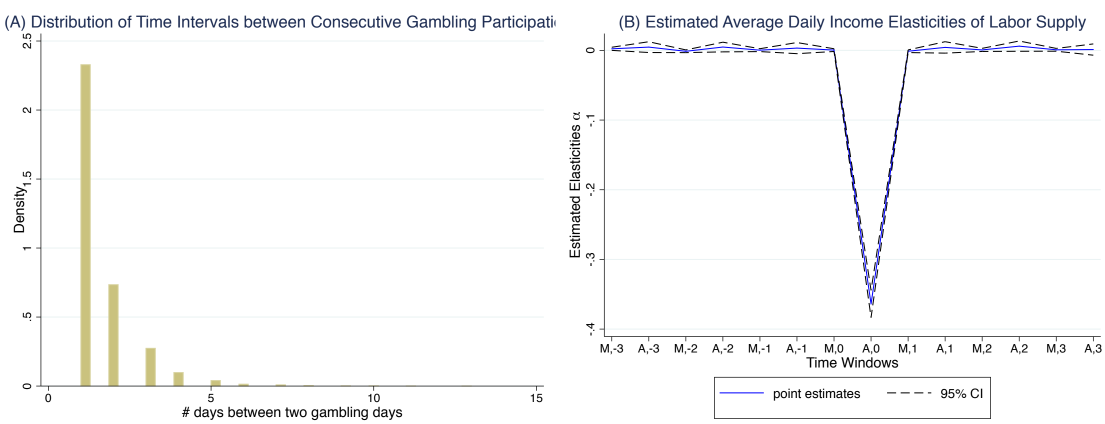
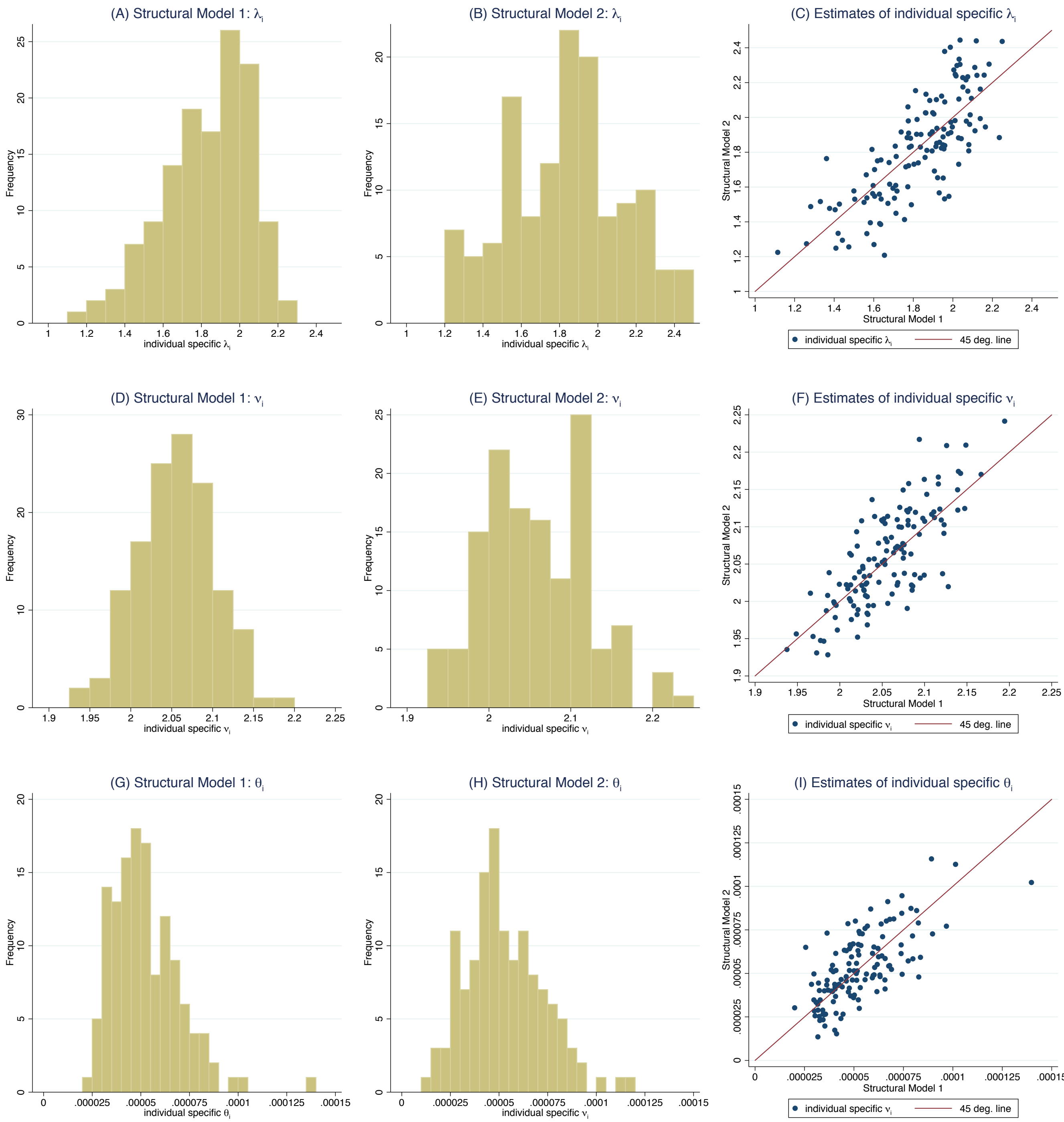
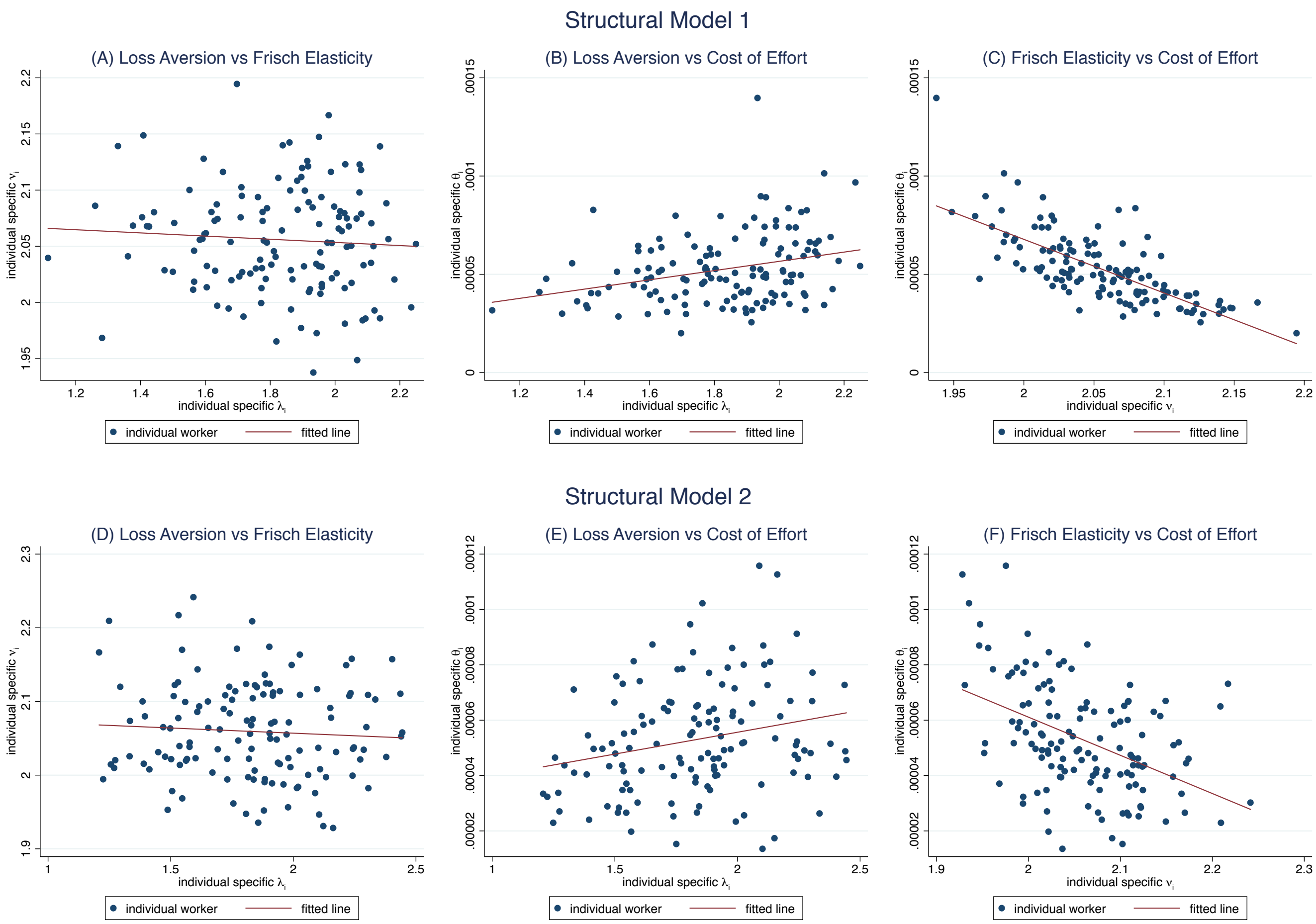


Figure 2.8: Structural Estimates of Individual Specific Preference Parameters



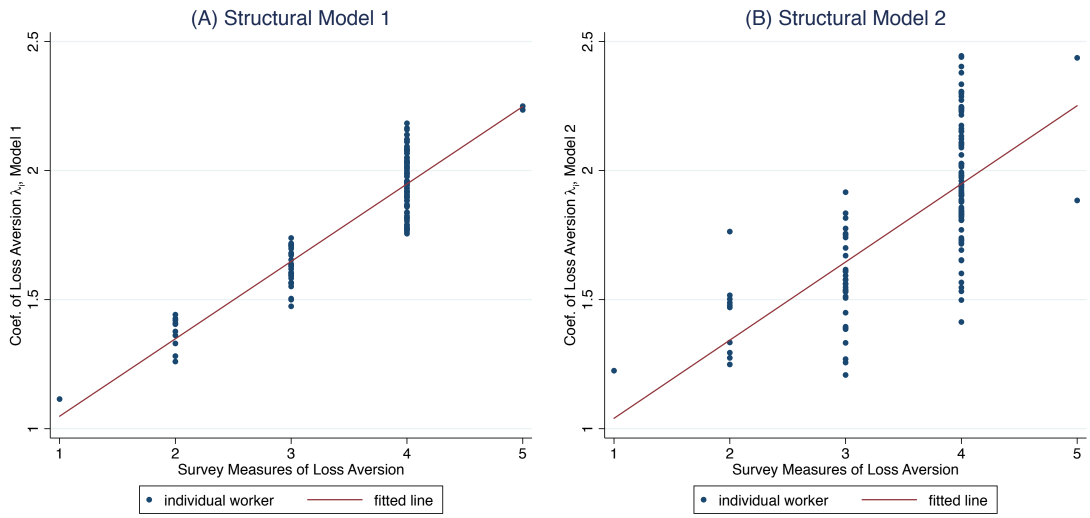
Notes: Individual specific structural estimations assume $\beta = \delta = 1$, use Target 1, and include controls other than individual charactersitics.

Figure 2.9: Structural Estimates of Individual Specific Preference Parameters (Cont.d)



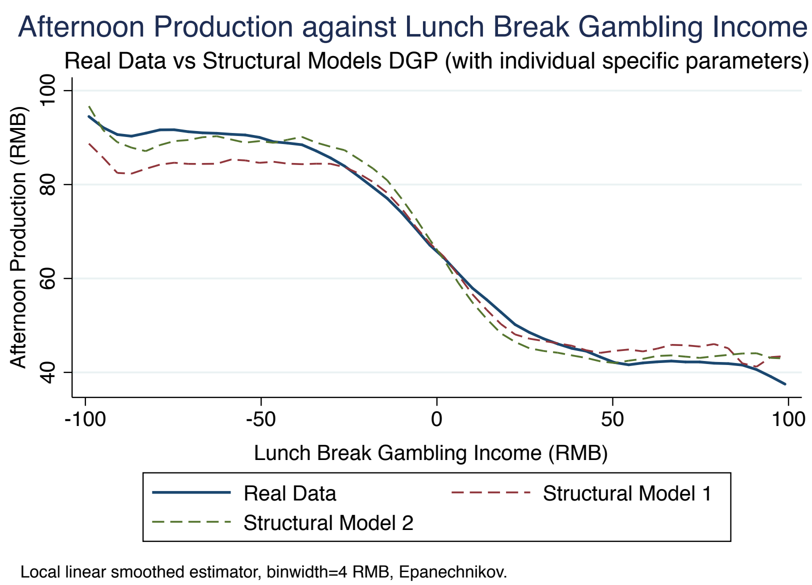
Notes: Individual specific structural estimations assume $\beta = \delta = 1$, use Target 1, and include controls other than individual charactersitics.

Figure 2.10: Estimated Coefficients vs Survey Measures of Loss Aversion



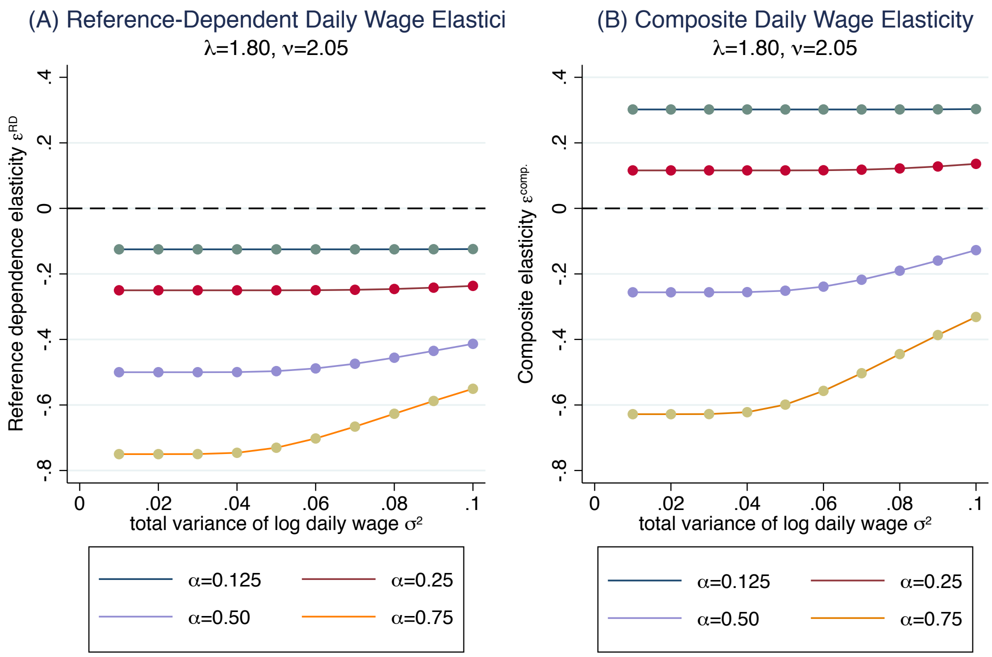
Notes: Individual specific structural estimations assume $\beta = \delta = 1$, use Target 1, and include controls other than individual charactersitics.

Figure 2.11: Daily Non-Labor Income Elasticities: Real Data versus Model Simulation



Individual specific structural parameter estimates and Target 1 are used.

Figure 2.12: Backed-Out Daily Wage Elasticities of Labor Supply



α is the fraction of anticipated log wage variance.

2.12 Tables

Table 2.1: Predictions of Competing Models of Labor Supply

	Prediction	Models			
		(1) Neoclassical	(2) Pay Cycle RD	(3) Daily Real RD	(4) Daily Mental RD
(1)	$\mathbb{E}[\frac{\partial I_t^{L*}}{\partial I_t^U}]$	$= 0$	≤ 0	< 0	< 0
(2)	$\mathbb{E}[\frac{\partial I_t^{L*}}{\partial I_\tau^U}],\ \tau < t$	$= 0$	$= \mathbb{E}[\frac{\partial I_t^L}{\partial I_t^U}]$	$= 0$	$= 0$
(3)	$\partial\mathbb{E}[\frac{\partial I_t^{L*}}{\partial I_t^U}]/\partial(\bar{t} - t)$	$= 0$	≥ 0	< 0, if I_t^U i.i.d across t	$= 0$
(4)	$\partial\mathbb{E}[\frac{\partial I_t^{L*}}{\partial I_t^U}]/\partial\lambda$	$= 0$	≤ 0	< 0	< 0
(5)	$\frac{\partial I_t^{L*}}{\partial I_t^U}$	-	-	-	$= 0,\ I_t^U < T_t - \left[\frac{\lambda}{\theta}\right]^{\frac{1}{\nu}}$, $= -1,\ T_t - \left[\frac{\lambda}{\theta}\right]^{\frac{1}{\nu}} \leq I_t^U \leq T_t - \left[\frac{1}{\theta}\right]^{\frac{1}{\nu}}$, $= 0,\ I_t^U > T_t - \left[\frac{1}{\theta}\right]^{\frac{1}{\nu}}$.

Table 2.2: Lunch Break Gambling Income and Daily Production (I)

Dep. Var	Daily Production (TP)		Morning Production (MP)		Afternoon Production (AP)	
	(1)	(2)	(3)	(4)	(5)	(6)
Gambling Income	-0.374***	-0.375***	0.020***	-0.024***	-0.375***	-0.375***
(GI)	(0.008)	(0.0077)	(0.0031)	(0.0015)	(0.0076)	(0.0077)
Morning Production					0.220***	-0.771***
(MP)					(0.0227)	(0.0271)
Afternoon Production			0.051***	-0.067***		
(AP)			(0.0065)	(0.0026)		
Individual FEs	No	Yes	No	Yes	No	Yes
Factory-Pay-Cycle FEs	No	Yes	No	Yes	No	Yes
#Days-to-Payday FEs	No	Yes	No	Yes	No	Yes
Day-of-Week FEs	No	Yes	No	Yes	No	Yes
Game Opponents FEs	No	Yes	No	Yes	No	Yes
R^2	0.3009	0.5546	0.0114	0.6856	0.3744	0.4630
# Observations	19,090	19,090	19,090	19,090	19,090	19,090

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Only gambling days of frequent gamblers are included. Observations are weighted by the inverse of total gambling participation days of each individual.

Table 2.3: Lunch Break Gambling Income and Daily Production (II)

	Dep. Var: Daily Production (TP)				
	(1)	(2)	(3)	(4)	(5)
Gambling Income	-0.375***	-0.380***	-0.375***	-0.378***	-0.374***
(GI)	(0.0077)	(0.0076)	(0.0077)	(0.0077)	(0.0081)
Individual FEs	Yes	Yes	Yes	Yes	No
Factory-Pay-Cycle FEs	Yes	Yes	Yes	Yes	No
#Days-to-Payday FEs	Yes	Yes	Yes	Yes	No
Day-of-Week FEs	Yes	Yes	Yes	Yes	No
Game Opponents FEs	Yes	Yes	Yes	Yes	No
Factory-Workday FEs	No	No	No	No	Yes
Game Participation (G)	No	No	No	No	Yes
Sample (Worker)	Freq. Gamblers	All Workers	Freq. Gambler	Freq. Gamblers Non-Turnover	Freq. Gamblers
Sample (Day)	$G = 1$	$G = 1$	$G = 1, 0$	$G = 1$	$G = 1, 0$
R^2	0.5546	0.568	0.5031	0.5641	0.4747
# Observations	19,090	19,312	30,038	16,301	30,038

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). For $G = 1, 0$ samples, observations are weighted by the inverse of total workdays of each individual; for $G = 1$ samples, observations are weighted by the inverse of total gambling participation days of each individual.

Table 2.4: Pay-Cycle-Accumulated Gambling Income and Daily Production

	Dep. Var: Daily Production (TP)				
	(1)	(2)	(3)	(4)	(5)
Accumulated Gambling Income (GI)	0.0015 (0.0033)	0.0072*** (0.0017)	0.0063*** (0.0019)	0.0044*** (0.0013)	0.0021 (0.0023)
Individual FEs	No	Yes	Yes	Yes	No
Factory-Pay-Cycle FEs	No	Yes	Yes	Yes	No
#Days-to-Payday FEs	No	Yes	Yes	Yes	No
Day-of-Week FEs	No	Yes	Yes	Yes	No
Game Opponents FEs	No	Yes	Yes	Yes	No
Factory-Workday FEs	No	No	No	No	Yes
Game Participation (G)	No	No	No	No	Yes
Sample (Worker)	Freq. Gamblers	Freq. Gamblers	All Workers	Freq. Gambler	Freq. Gamblers
Sample (Day)	$G = 1$	$G = 1$	$G = 1$	$G = 1, 0$	$G = 1, 0$
R^2	0.0000	0.2594	0.2736	0.2945	0.2723
# Observations	18,119	18,119	18,325	28,668	28,668

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Accumulated Gambling Income means individual's accumulated gambling income in the pay cycle up to the workday before the considered workday. For $G = 1, 0$ samples, observations are weighted by the inverse of total workdays of each individual; for $G = 1$ samples, observations are weighted by the inverse of total gambling participation days of each individual.

Table 2.5: Heterogeneous Daily Production Response to Gambling Income

	Dep. Var: Daily Production (TP)				
GI Range	$GI \in \mathbb{R}$ (1)	$GI \in (-50, 50)$ (2)	$GI \in (-30, 30)$ (3)	$GI \in (-10, 10)$ (4)	$GI \in (-5, 5)$ (5)
Gambling Income (GI)	-0.375*** (0.0077)	-0.499*** (0.0127)	-0.634*** (0.0202)	-0.874*** (0.0742)	-0.951*** (0.1839)
Individual FEs	Yes	Yes	Yes	Yes	Yes
Factory-Pay-Cycle FEs	Yes	Yes	Yes	Yes	Yes
#Days-to-Payday FEs	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes
Game Opponents FEs	Yes	Yes	Yes	Yes	Yes
R^2	0.5546	0.6050	0.5643	0.5497	0.5868
# Observations	19,090	15,174	9,916	3,205	1,881

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Only gambling participation days of frequent gamblers are included. Observations are weighted by the inverse of total gambling participation days of each individual where gambling income lie in the indicated ranges.

Table 2.6: Piece-Rate versus Daily-Rate Wage Schemes: Counterfactuals

	Dep. Var: Daily Production (TP)					
	Piece Rate		Daily Rate Factory A, April/May 2014		All Factories & Periods	
	(1)	(2)	(3)	(4)	(5)	(6)
Gambling Income (GI)	-0.374***	-0.375***	0.0261	0.0259	0.0261	0.0359
	(0.0080)	(0.0077)	(0.0234)	(0.0236)	(0.0340)	(0.0280)
I[Daily Rate=1]					-16.34***	-25.43***
					(1.15)	(2.07)
GI ×(1-I[Daily Rate=1])					-0.3896***	-0.3995***
					(0.0343)	(0.0283)
Individual FEs	No	Yes	No	Yes	No	Yes
Factory-Pay-Cycle FEs	No	No	No	Yes	No	Yes
#Days-to-Payday FEs	No	Yes	No	Yes	No	Yes
Day-of-Week FEs	No	Yes	No	Yes	No	Yes
R^2	0.3009	0.5546	0.0037	0.1618	0.2918	0.5307
# Observations	19,090	19,090	343	343	19,425	19,425

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Only gambling participation days of frequent gamblers are included.

Table 2.7: Linear Hazard Model: Allowing For Jumps at Proxied Daily Targets

	Dep. Var: Stop Production ($Stop_{itj}$)					
	Target 1		Target 2		Target 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative Income relative to Target ($I^L_{itj} + I^U_{it} - T$)	0.042***	0.030***	0.042***	0.031***	0.044***	0.038***
	(0.0020)	(0.0018)	(0.0023)	(0.0021)	(0.0020)	(0.0022)
Cumulative Income≥Target ($I[I^L_{itj} + I^U_{it} \geq T]$)	0.0489***	0.0511***	0.0416***	0.0440***	0.0441***	0.0471***
	(0.0032)	(0.0033)	(0.0026)	(0.0026)	(0.0034)	(0.0035)
Interaction ($(I^L_{itj} + I^U_{it} - T) \times I[I^L_{itj} + I^U_{it} \geq T]$)	0.123***	0.131***	0.118***	0.126***	0.114***	0.127***
	(0.0132)	(0.0129)	(0.0120)	(0.0118)	(0.0122)	(0.0116)
Cumulative Production (I^L_{itj})	Yes	Yes	Yes	Yes	Yes	Yes
Cumulative Production Sq. (I^{L2}_{itj})	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	No	Yes	No	Yes	No	Yes
Factory-Pay-Cycle FEs	No	Yes	No	Yes	No	Yes
#Days-to-Payday FEs	No	Yes	No	Yes	No	Yes
Day-of-Week FEs	No	Yes	No	Yes	No	Yes
R^2	0.1118	0.1248	0.1087	0.1222	0.1078	0.1241
# Observations	235,186	235,186	235,186	235,186	235,186	235,186

	Dep. Var: Stop Production ($Stop_{itj}$)					
	Target 4		Target 5		Target 6	
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative Income relative to Target ($I^L_{itj} + I^U_{it} - T$)	0.0416***	0.0350***	0.0328***	0.0226***	0.0266***	0.0238***
	(0.0019)	(0.0019)	(0.0018)	(0.0017)	(0.0015)	(0.0013)
Cumulative Income≥Target ($I[I^L_{itj} + I^U_{it} \geq T]$)	0.042***	0.045***	0.035***	0.036***	0.022***	0.024***
	(0.0028)	(0.0029)	(0.0023)	(0.0023)	(0.0025)	(0.0025)
Interaction ($(I^L_{itj} + I^U_{it} - T) \times I[I^L_{itj} + I^U_{it} \geq T]$)	0.113***	0.124***	0.075***	0.081***	0.069***	0.074***
	(0.0117)	(0.0116)	(0.0083)	(0.0083)	(0.0077)	(0.0077)
Cumulative Production (I^L_{itj})	Yes	Yes	Yes	Yes	Yes	Yes
Cumulative Production Sq. (I^{L2}_{itj})	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	No	Yes	No	Yes	No	Yes
Factory-Pay-Cycle FEs	No	Yes	No	Yes	No	Yes
#Days-to-Payday FEs	No	Yes	No	Yes	No	Yes
Day-of-Week FEs	No	Yes	No	Yes	No	Yes
R^2	0.1069	0.1231	0.1022	0.1155	0.0956	0.1125
# Observations	235,186	235,186	235,186	235,186	235,186	235,186

Notes: Standard errors clustered at worker level in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Income values are in terms of 100 RMB. Only gambling participation days of frequent gamblers are included. For consistency I drop observations if at least one target proxy is not available. Target 1 is individual's average daily mental face-valued income on preceding workdays in the same factory-pay-cycle. Target 2 is individual's average daily mental face-valued income on preceding gambling participation days in the same factory-pay-cycle. Target 3 is individual's average daily mental face-valued income on preceding same-day-of-week workdays. Target 4 is individual's average daily mental face-valued income on preceding same-day-of-week gambling participation days. Target 5 is individual's daily mental face-valued income on last workday. Target 6 is individual's daily mental face-valued income on last gambling participation day.

Table 2.8: Structural Models Estimation: Common Preference Parameters

	Structural Model I: Continuous Choice of Productivity					
	Target 1			Target 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Loss Aversion λ	1.86***	1.80***	1.93***	1.82***	1.79***	1.89***
($\lambda = 1$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Inverse Frisch Elasticity ν	1.99***	2.05***	1.93***	2.03***	1.96***	1.87***
($\nu = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Disutility of Effort ($10^5 \times \theta$)	5.3***	5.2***	5.0***	5.1***	4.7***	4.9***
($\theta = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Discount Factor (δ)	$\delta = 1$	$\delta = 1$	0.99	$\delta = 1$	$\delta = 1$	1.01
($\delta = 1$)			(0.731)			(0.824)
($\delta = 0.957$)			(0.009)***			(0.005)***
Hyperbolic Discount Factor (β)	$\beta = 1$	$\beta = 1$	0.98	$\beta = 1$	$\beta = 1$	0.97
($\beta = 1$)			(0.815)			(0.747)
($\beta = 0.7$)			(0.003)***			(0.002)***
Std Dev of Noise ϵ^{ξ} (σ_{ξ})	16.2***	13.5***	16.0***	18.2***	14.7***	18.3***
($\sigma_{\xi} = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls (β^{ζ})	$\beta^{\zeta} = 0$	-	$\beta^{\zeta} = 0$	$\beta^{\zeta} = 0$	-	$\beta^{\zeta} = 0$
($\beta^{\zeta} = 0$)		(0.000)			(0.000)	
Log-likelihood	-2,618	-2,043	-2,587	-2,591	-2,061	-2,503
# Observations	14,286	14,286	14,286	14,286	14,286	14,286

	Structural Model II: Hazard Model of Stopping					
	Target 1			Target 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Loss Aversion λ	1.93***	1.97***	1.94***	1.96***	2.03***	2.01***
($\lambda = 1$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Inverse Frisch Elasticity ν	1.98***	2.04***	1.95***	1.96***	2.02***	1.92***
($\nu = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Disutility of Effort ($10^5 \times \theta$)	4.8***	4.4***	5.1***	5.3***	5.0***	5.5***
($\theta = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Discount Factor (δ)	$\delta = 1$	$\delta = 1$	1.00	$\delta = 1$	δ=1	0.99
($\delta = 1$)			(0.202)			(0.093)*
($\delta = 0.957$)			(0.001)***			(0.002)***
Hyperbolic Discount Factor (β)	$\beta = 1$	$\beta = 1$	0.99	$\beta = 1$	$\beta = 1$	0.98
($\beta = 1$)			(0.431)			(0.348)
($\beta = 0.7$)			(0.000)***			(0.000)***
Std Dev of Noise ϵ^{ζ} (σ_{ζ})	14.6***	10.9***	14.6***	15.2***	11.1***	14.9***
($\sigma_{\zeta} = 0$)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls (β^{ζ})	$\beta^{\zeta} = 0$	-	$\beta^{\zeta} = 0$	$\beta^{\zeta} = 0$	-	$\beta^{\zeta} = 0$
($\beta^{\zeta} = 0$)		(0.000)			(0.000)	
Log-likelihood	-28,790	-22,147	-27,844	-29,172	-22,891	-28,335
# Observations	217,189	217,189	217,189	217,189	217,189	217,189

Notes: p values in parentheses from likelihood ratio tests (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Target 1 is individual's discounted average daily total income on preceding workdays in the same factory-pay-cycle. Target 2 is individual's discounted average daily total income on preceding gambling participation days in the same factory-pay-cycle. Control includes win/loss dummies, individual characteristics, day-of-the-week dummies, factory-pay-cycle dummies and distance-to-pay-day dummies ($DTP \leq 10$, $10 < DTP \leq 20$, $DTP >= 20$). Only gambling participation days of frequent gamblers are included. For consistency I keep observations for which target 1, target 2 as well as all the control variables are available.

Chapter 3

Across-Country Wage Compression in Multinationals

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[1]We thank Bentley MacLeod, Oriana Bandiera, Nick Bloom, David Card, Arindrajit Dube, Alan Krueger, Attila Lindner, Hannes Malmberg, Enrico Moretti, Suresh Naidu, and John Van Reenen for helpful suggestions; Jett Pettus for great research assistance; the Company for data access; and seminar audiences at Columbia, NYU, Uppsala, and the 2018 briq Workshop on Firms, Jobs and Inequality for feedback. All errors are our own.

3.1 Introduction

Classical economics assumes that firms adjust to variation in productivity across labor markets by setting workers' pay equal to their marginal product. However, growing empirical evidence underscores that the way wages are actually set often differs from the competitive labor market model. Some firms pay workers of similar skill levels more than others (Card et al., 2013, 2015; Barth et al., 2016; Bloom et al., forthcoming; Card et al., 2018).[2]

In this paper we explore a source of firm pay premiums that may have been overlooked. We hypothesize that the use of firm-wide wage-setting procedures in multi-establishment firms can limit wage differences relative to the headquarter. To investigate, we focus on a canonical example of high-wage firms—multinationals abroad.[3] Conventional explanations for the multinational pay premium focus on technology or production style differences across firms that affect worker productivity.[4] Building on recent work on attitudes towards pay inequality and firms' inability or unwillingness to adjust to local contexts[5], we establish an additional channel of a different nature. We show that some multinationals—those headquartered in countries with an inequality-averse culture—tend to in effect "anchor" the wages they pay abroad to headquarter wage levels. Such firms also extend *externally imposed* headquarter wage increases in part to their foreign establishments, while multinationals from less inequality-averse societies do not. Finally, we show that multinationals that anchor their wages abroad to headquarter levels reorganize production, shifting occupations away from their foreign establishments, when wage increases at the headquarter are required.

Our analysis makes use of an unusual 2005-2015 establishment$\times$year level dataset of

[2]Recognition of and interest in "firm effects" in wages have a long history in labor economics. For early work, see e.g. Slichter (1950); Rees and Schultz (1970); Dickens and Katz (1987); Krueger and Summers (1988); Van Reenen (1966); Abowd et al. (1999, 2002).

[3]That multinationals pay high wages in low- and middle-income countries is extensively documented in existing work (see Brown et al. (2004); Lipsey (2004) on the early literature, and more information below).

[4]See e.g. Aitken et al. (1996); Conyon et al. (2002); Egger and Kreickemeier (2013); Sun (2018).

[5]Recent research has shown that many societies are averse to pay inequality (Card et al., 2012; Mas, 2017; Breza et al., 2018; Cullen and Perez-Truglia, 2018b; Dube et al., 2018a; Falk et al., 2018b), and that such attitudes can influence firms' wage-setting practices (Harrison and Scorse, 2010). We also know that some firms are unwilling or unable to adjust their product prices to local contexts (DellaVigna and Gentzkow, 2017; Adams and Williams, 2019).

average wages by occupation. The dataset was constructed by a consulting company and covers 1,800 employers—the majority of which are large firms[6]—that span 16 broad sectors and as a whole operate in 170 different capital cities around the world.

We begin by showing that the average wage a given employer pays *domestic* workers within a given narrowly-defined occupation at foreign establishments is highly correlated with the average wage the employer pays workers in the same occupation in the headquarter country. This holds across the occupational skill range, including for low-skill support staff not directly engaged in production. The magnitude of the anchoring effect is such that the multinationals in our sample pay wages abroad that are in real terms an order of magnitude higher than the wages they pay workers in the same occupations in the home country.

The within-firm, across-country wage co-movement we document controls for fixed effects that rule out conventional explanations that operate through productivity differences across firm×occupations or city×years. However, the anchoring we observe in the full sample is driven by multinationals headquartered in inequality-averse countries as measured by sociologists (Hofstede, 1991, 2001; Tabellini, 2010; Gorodnichenko and Roland, 2011; Bandiera et al., 2019)[7], suggesting that the source of the multinational pay premium may go beyond differences in technology and production style.

To establish causality, we instrument for headquarter wage levels with changes in home country minimum wage laws, in four different ways. In an initial step, we document that wages in "treated" and "control" sample establishments that are located in a given foreign city evolve very similarly before the minimum wage is increased in the country where the headquarter of treated establishments is located. Our first approach is therefore to simply compare the wages of such treated and control establiments before and after the change in

[6]The size distribution of the U.S. publicly listed firms in our sample is comparable to that of all U.S. publicly listed firms with more than USD 25 billion in annual revenues. Most of the employers in our sample are private firms, but the dataset also contains multinational NGOs and public sector employers. In the Appendix we show that our results are very similar for different types of employers. For simplicity, we use "firm" and "employer" interchangeably in the remainder of the text.

[7]Hofstede (1991, 2001)'s measures of culture are widely used across the social sciences and have been validated by several other studies.

the home country's regulations. In the second approach we hold constant the foreign establishment and compare changes in wages in jobs for which the prior minimum wage was more versus less binding for the same job in the home country. In the third approach we hold constant the headquarter country×year and compare changes in wages in the foreign establishments of same-headquarter country-firms that are more versus less exposed to minimum wage changes because of differences in employment structures. Finally, we restrict our analysis to within establishment×year job comparisons *within establishments whose headquarter is located in a given country.*

In combination, the results from these four approaches make clear that externally imposed wage increases in the headquarter country directly raise wages in firms' foreign establishments. The implied wage compression across locations is in line with how many firms themselves report to set wages, at least within the headquarter country. (In a recent survey of primarily North American employers operating in multiple locations, 29 percent report paying *the same nominal wages* across locations (Culpepper and Associates Inc, 2011).[8]) We show that an (otherwise plausible) within-firm version of Feenstra and Hanson (1996)'s outsourcing of medium-skill jobs from high-wage headquarters to lower-wage establishments phenomenon cannot explain the estimated effect of minimum wage-induced increases in headquarter wages on foreign establishment wages. We also show that endogenous timing of minimum wage changes—such as policymakers raising minimum wages when demand for labor is high (see e.g. Baskaya and Rubinstein, 2015; Neumark, 2018)—can plausibly explain *at most* around half of the total estimated effect.

The multinationals that extend externally imposed wage hikes to their foreign establishments are the same ones that more generally anchor wages to headquarter levels: those headquartered in inequality-averse countries. Our results thus suggest that some firms have "wage cultures" that lead them to compress the wages they pay across locations for reasons

[8]However, surveys such as Culpepper and Associates Inc (2011) tend not to cover large multinationals such as the ones we study, and to our knowledge none explicitly ask firms how they set wages in their foreign establishments. Within the U.S., Amazon, IKEA, Walmart, and at least 58 other large American employers have self-imposed, country-wide wage floors (National Employment Law Project, 2016).

that go beyond variation in productivity.[9]

This suggests that firms with such wage cultures may respond to rising wages at the headquarter by reorganizing production (Goldschmidt and Schmieder, 2017). In the final part of the part of the paper, we show that, when a wage increase for low-wage jobs at the headquarter is externally imposed on multinationals, the firms that increase the wages they pay workers in the same jobs abroad—multinationals from inequality-averse societies—also change the occupational structure of their foreign establishments. In particular, such firms are more likely to remove and less likely to add an occupation to the composition of their workforce in foreign establishments when home country minimum wages rise. In contrast, we find that multinationals that do not transmit home country wage increases to their foreign establishments—those from less inequality-averse societies—also do not to change the occupational structure of their foreign establishments in response to externally imposed headquarter wage hikes. We view the this evidence as a first step towards understanding the *consequences* of across-country wage compression in multinationals, which may be far-reaching and multi-faceted.

This paper contributes to several strands of the literature on how firms set wages and organize production across space. First, we use a new form of data on large multi-establishment firms' operations across countries to document a novel regularity, namely that many such firms anchor their wages to headquarter levels. Our analysis builds on recent findings on invariability in firms' decisions across starkly different contexts, especially DellaVigna and Gentzkow (2017).[10] We connect this body of evidence with the literature on how firms set

[9]Paying high wages in foreign establishments may over time lead firms to attract better workers, or motivate higher effort among existing workers, or complementary investments from the firm. Any such *ex post* benefits of wage hikes that arise because firms' wage-setting procedures are not adjusted to local labor market conditions would need to occur also for low-skill workers such as drivers, cleaners, and guards, and to be very large, to compensate for the magnitude and patterns of the wage anchoring-to-the-headquarter we document. On the other hand, simple job-based wage-setting systems are cheaper in use than productivity-based systems (Lemieux et al., 2009).

[10]DellaVigna and Gentzkow (2017) show that many U.S. retailers charge nearly identical prices across large zones of the U.S. The literature on invariability in firms' decisions across contexts originates in the seminal work of Kahneman et al. (1986b). Recent empirical studies have documented constraints imposed on the wages firms pay different workers in a given worksite or country by workers' fairness preferences (Card et al., 2012; Mas, 2017; Breza et al., 2018; Cullen and Perez-Truglia, 2018b; Dube et al., 2018a). On relative

wages. Understanding what drives spatial wage differences is by many seen as the key to understanding the process of economic development itself (see e.g. Moretti, 2011; Clemens et al., forthcoming).

Second, by establishing a particular *reason* why some firms pay higher wages than others, this paper helps uncover the nature of the well-known but poorly understood phenomenon of *firm wage effects* (see paragraph and footnote 2).[11] The anchoring-to-the-headquarter wage-setting we document in firms from inequality-averse societies is consistent with existing evidence of rent-sharing[12], but to our knowledge represents the first direct evidence of firm "wage cultures".[13] Our research design builds on the seminal work of Bloom and Van Reenen (2007); Bloom et al. (2012a) showing that multinationals "transport" their practices across borders (for analogous evidence on *individuals*, see Fisman and Miguel (2007); Almond et al. (2013); Atkin (2016); Campa and Serafinelli (forthcoming)), and Harrison and Scorse (2010)'s evidence that home country attitudes towards pay levels abroad can influence firms' wage-setting there. Firm cultures may help explain other phenomena such as the surprising acyclicality of wages (Lemieux et al., 2012) and lack of delegation to establishments outside of firms' home region (see e.g. Aghion et al., 2017).

This paper also presents what to our knowledge is the first evidence of *across-country* margins of adjustment to—and components of the incidence of—minimum wages.[14] In this

pay comparisons, see also Hamermesh (1975); Akerlof and Yellen (1990); Fehr and Schmidt (1999b), and the lab-based experimental studies surveyed in—and following on from—Rabin (1998).

[11] In the literature on the multinational pay premium, research designs aimed at capturing a causal effect of who-the-owner-is have exploited foreign acquisitions and sales of establishments, although Orefice et al. (2016) document systematic trends in wages in subsequently acquired or sold establishment prior to changes in ownership. Individual worker-level evidence shows that multinationals pay a given worker more than local firms (see e.g. Lipsey and Sjoholm, 2006; Martins and Esteves, 2008; Hijzen et al., 2013; Earle et al., 2017).

[12] Budd et al. (2005); Martins and Yang (2015) document a high elasticity of average wages in *foreign affiliates* with respect to general variation in parent firm profits, consistent with our results. Card et al. (2018) review the broader literature documenting that some firms share rent with workers via higher wages.

[13] By *wage culture*, we here mean systematic components of wage-setting practices that vary across firm "types" and that are not intended to equate pay and productivity across workers, worksites, or countries. It is well-established that many firms fail in their attempt to equate workers' pay and marginal product (see e.g. Kerr, 1975; Gibbons, 1997; Lemieux et al., 2009). Hermalin (2001); Akerlof and Kranton (2005); Schein (2010); Hermalin (2013) survey the literature on corporate culture. This literature is primarily theoretical—with some important exceptions (see e.g. Guiso et al., 2015)—and to our knowledge has not made use of empirical strategies intended to capture causal estimates.

[14] The minimum wage literature is vast: see e.g. Neumark and Wascher (1992); Card and Krueger (1995);

sense our analysis relates to emerging evidence of shocks spreading across space inside firms (Boehm et al., 2017; Giroud and Mueller, forthcoming; Giroud and Rauh, forthcoming; Guo, 2018). Our findings indicate that changes in government regulations enacted in one country—in our case minimum wages—can spread to other countries via employer practices.

Finally, we contribute to the literature on how firms organize production. Most closely related to this paper, Goldschmidt and Schmieder (2017) ingeniously show that high-wage German firms outsource the particular jobs that can be (and are) paid less outside of the firm (see also Feenstra and Hanson, 1996). The broader impact of wage anchoring-to-the-headquarter and associated compression of the occupational structure in multinationals' foreign establishments is an important topic for future research (see also Lemieux et al., 2009; Harrison and Scorse, 2010; Boeri et al., 2018).[15] To understand any resulting misallocation of occupations and jobs across regions, equilibrium models of national, regional, or global production in which the wage discount associated with producing in a low-wage location for some firms depend on *the firm's origin* may be needed.[16]

Lee (1999); Aaronson and French (2007); Thompson (2009); Draca et al. (2011); Clemens and Wither (2015); MaCurdy (2015); Autor et al. (2016); Engbom and Moser (2017); Ganapati and Weaver (2017); Harasztosi and Lindner (2018); Horton (2018); Neumark (2018); Haanwinckel (2019); Cengiz et al. (forthcoming).

[15] The evidence in Lemieux et al. (2009); Harrison and Scorse (2010); Boeri et al. (2018) provide important hints. Lemieux et al. (2009) and Boeri et al. (2018) both show evidence suggesting that forms of wage-setting that equalize pay across workers significantly reduce wage inequality within countries. Boeri et al. (2018) also find that Italy's system imposing near-equality of nominal wages across regions in some jobs hampers job creation in the South, while Harrison and Scorse (2010) find mixed evidence on the impact in Indonesia of multinationals raising their wages there in response to activism.

[16] For existing work and models of how production is organized across space, see for example Dube and Kaplan (2010); Bloom et al. (2012b); Blinder and Krueger (2013); Irarrazabal et al. (2013); Rodríguez-Clare and Ramondo (2013); Tintelnot (2016); Antrás and Yeaple (2014); Antrás (2016), and in particular Grossman and Helpman (2007)'s pioneering model of wage dispersion concerns and employment structures.

3.2 Data and Summary Statistics

3.2.1 Data

The primary dataset we use comes from a company that gathers information on compensation at establishments around the world (the "Company").[17] Human Resources personnel at each establishment are instructed by the parent multinational's managers to report the full list of positions present in the establishment at the relevant point in time, and their average gross and net monthly pay. 298 position titles appear in the data; we refer to these as occupations or jobs (used interchangeably). In addition, the Company maps the 298 occupations into 16 skill levels that are defined globally. Examples of low-skill occupations include Cleaner, Guard, and Data entry clerk. Middle-skill occupations include Administrative assistant, Systems analyst, and Finance officer, while high-skill occupations include Senior legal counsel, Regional office manager, and Human Resources director.

The dataset includes establishments located in 170 cities around the world, all but four of which are capital cities. On average, we observe each multinational operating establishments in 3.7 different countries.

The Company collects data every year, and the dataset we use covers 2005-2015. Most of the multinationals in the data are interviewed every year, but not all of their establishments are included every year.[18] At establishment$\times$year level the dataset is thus an unbalanced panel.

Our primary outcome variable is the average nominal net wage of *domestic* workers employed in a given occupation at a given establishment and year, measured in U.S. dollars.[19] In Section 3.5 we focus on the occupations that are present in a given establishment.

We match our data on establishments and wages to two additional datasets. First,

[17]We define the term "establishment" to include both firms' establishments located outside of the headquarter country and the headquarter itself.

[18]If some multinationals in our sample chose not to report data for some of their establishments, such establishments are not included in the dataset we use.

[19]Most of the multinationals in the sample report their compensation data to the Company in USD. The Company converts the data of employers that report in local currency to USD.

we gather information on minimum wage changes in firms' headquarter countries from the International Labour Organisation (ILO). These data cover 118 of the 170 countries observed in our primary dataset. Second, we also link our data to information on attitudes towards fairness in headquarter countries. The measure we use comes from Hofstede (2001)'s "cultural dimensions", originally constructed from a survey of IBM employees in over 80 countries. Specifically, we use Hofstede (2001)'s Power Distance measure of inequality aversion, which captures a group's willingness to accept inequality among members. These measures have been validated by other studies (e.g. Yoo et al., 2011), and are widely in social science research (see e.g. Tabellini, 2010; Gorodnichenko and Roland, 2011; Bandiera et al., 2019).[20] We classify countries as having either high or low inequality aversion based on whether they score above or below the median of Hofstede (2001)'s measure, and then link firms to the measure based on where their headquarter is located.

3.2.2 Summary statistics

The majority of the employers in our sample are private firms. They come from a variety of sectors, including banking, consulting, health care, mining and other natural resources, technology, telecommunications, and transport. Public sector employers and NGOs are also represented.[21] Most employers in the dataset are based in North America (predominantly the United States), followed by Africa and Europe.[22]

Employers themselves choose to report data to the Company. Most are well-known to the public, and most of the private firms are publicly listed. Hjort et al. (2019) show that the size distribution of the U.S. publicly listed firms in the sample is comparable to that of

[20]For more information, see https://geerthofstede.com/culture-geert-hofstede-gert-jan-hofstede/6d-model-of-national-culture/.

[21]Sectors are defined according to the Standard Industrial Classification, with public sector employers and NGOs classified separately. In the public sector, interviewed organizations are mainly national banks and various branches of government that tend to have establishments abroad.

[22]The reason why African employers are well-represented, especially among public sector employers, is that the Company's primary focus is to collect data on establishments located in low- and middle-income countries. Most NGOs, on the other hand, are based in North America or Europe and have establishments in other continents.

all U.S. publicly listed firms with more than USD 25 billion in annual revenues, and provide an in-depth description of the employers in the sample. Table 3.1 displays their summary statistics. In Panel A we see that the mean wage they pay firm-wide is around USD 20,000, with a standard deviation of around USD 25,000. The average employer employs workers in 28 different occupations (s.d. $= \sim 18$) that belong to 11 different skill levels (s.d. $= \sim 1.5$), and has 3.7 foreign establishments (s.d. $= \sim 11$). Panel B shows that 1,100 of the 1,800 employers in the full sample are private firms; these pay somewhat higher mean wages than public employers and NGOs.

In Panel C of Table 3.1 we display employers' mean wage at each of the four quartiles of their wage distribution. The mean wage is around USD 10,000 in the lowest quartile and around USD 40,000 in the highest quartile. Panel C also shows, by headquarter wage-quartile, employers' wage levels at their foreign establishments as a percentage of their wage level for the same jobs at the headquarter. These are generally high, rising from 66 percent in the lowest skill quartile to 83 in the highest skill quartile on average.

Our full analysis sample consists of the 946 employers for which we have wage data from at least one foreign establishment in addition to the headquarter. When we restrict the sample to employers for which we observe wages for workers in occupations of the same skill-level at one or more foreign establishments and the headquarter, we end up with 93 unique employers. When we instead restrict to employers for which we observe wages for workers in identical, narrowly-defined occupations at one or more foreign establishments and the headquarter, we end up with 68 unique employers.

3.3 Anchoring to Headquarter Wages

In this section we show that many multinationals pay the workers they employ at establishments in foreign countries wages that are remarkably highly correlated with the wages they pay workers in the headquarter country. To do so we take advantage of the fact that we observe multinationals from many different countries employing workers in a given type of job in a given foreign city at the same point in time.

We run the following regression:

$$\mathrm{w}_{jfct} = \beta_1 \mathrm{HQw}_{jft} + \beta_2 \bar{\mathrm{w}}_{j(-f)ct} + \theta_{fj} + \theta_{ct} + \varepsilon_{jfct} \tag{3.1}$$

where w_{jfct} is the average wage of workers in job j at firm f's establishment in foreign city c in year t. A *job* here means a specific position such as Driver, Administrative assistant, or Human Resources director. (In alternative specifications, the results from which we also show in our analysis below, j refers instead to the average wage of workers in jobs of a given skill-level). HQw_{jft} is the average wage of workers in the same job at firm f's headquarter in year t. $\bar{\mathrm{w}}_{j(-f)ct}$ is our measure of how much *other* employers in the same sector are paying workers in the same job in the same city and point in time, specifically the average wage of workers in job j employed by all firms in our sample—*other than* firm $f$itself—that belong to the same sector as firm f in foreign city c in year t. $\bar{\mathrm{w}}_{j(-f)ct}$ thus proxies for the "market" wage of job j among (foreign and, where relevant, domestic) multinationals in a given sector. The correlation between a given multinational's wage level and other employers' wages in the same city and year is a natural benchmark to which we can compare the correlation between the firm's wage level abroad and at home.[23]

Throughout our analysis we include firm×job fixed effects θ_{fj} to account for differences across firms in the productivity of workers in job j, and city×year fixed effects θ_{ct} so that we only compare establishments located in a given city and at a given point in time. We measure all wage levels as the log of the relevant nominal, post-tax wage in USD, and cluster

[23]We could alternatively benchmark β_1 exclusively against the correlation between multinationals' wage level and that of domestic employers. For many cities in developing countries, our sample contains no or only a moderate number of employers from the country itself so that such an approach would necessitate comparison to wage data from another dataset, the sampling and data collection procedures for which would likely differ. In addition, for many of the jobs observed in our dataset, the number of domestic employers employing workers in the exact same position as the multinationals we study in the same city×year is likely limited (even if domestic employers often employ workers in related but distinct positions). Note also that our "within-sample-benchmark" approach will if anything lead us to underestimate the true extent of wage anchoring because other multinationals likely pay workers in job j wages that are closer to those of firm f than domestic employers do.

the standard errors at firm level (see Abadie et al., 2017).

We find a strong correlation between headquarter and foreign establishment wage levels. As seen in columns 1 and 3 of Table 3.2 and panels A and C of Figure 3.1, 10 percent higher occupation-specific headquarter wages is associated with 1.2-1.4 percent higher establishment wages for the same occupation.[24] This within-firm-across-country correlation in wage levels is at least twice as large as the correlation between a given establishment's wage level and our benchmark for the average wage level of workers in the same job, sector, city, and point in time.

In Section 3.4 we show evidence that the estimates in Table 3.2 reflect a "headquarter effect": wage changes at the headquarter manifest themselves in wage changes at a firm's foreign establishments, but the opposite does not hold (and between-foreign-establishments effects are very small in magnitude).[25] Insofar as the multinationals in our sample choose to operate establishments abroad in part because they can and do pay comparatively *low* wages to foreign workers, the counterfactual degree of wage anchoring-to-the-headquarter may be even greater in similar firms that choose not to do so.

We next demonstrate that wages are anchored to the headquarter level to a significantly greater extent in low-skill than higher-skill jobs. To do so we interact HQw_{jft} with indicators for the relevant occupation being respectively medium- and high-skill, as opposed to low-skill. In column 5 of Table 3.2 we see that 10 percent higher occupation-specific headquarter wages is associated with 2.6 percent higher establishment wages in low-skill occupations; 1.1 percent higher establishment wages in medium-skill occupations; and 1.5 percent higher establishment wages in high-skill occupations.

For the explanation for the headquarter-foreign establishment wage correlation we docu-

[24]Comparing columns 1 and 3 in Table 3.2 shows that this estimate is very similar when we compare jobs of a given skill-level in the headquarter versus foreign establishments and when we compare the exact same job across the two locations. This may in itself point towards firm-wide wage-setting policies that are influenced by attitudes towards fairness (a possibility we come back to below). Note also that the estimates in Table 3.2 are similar—if anything of greater magnitude—if we restrict the sample to private firms, as shown in Appendix Table C1.

[25]Note that we find no evidence that firms learn over time such that anchoring-to-the-headquarter falls with time spent operating in a given foreign city (results available from the authors upon request).

ment to operate through worker productivity, it would need to be the case that the productivity of a firm's workers at foreign establishments *changes* at the same point in time as that of its headquarter country workers. Additionally, such concurrent changes in productivity would need to occur also, and to a greater extent, for workers in low-skill jobs—support staff such as cleaners, drivers, and guards, for which the labor market is local, and for which the complementarity between labor and firm investments in technology and capital is presumably small. In the next section we analyze directly if firm-wide wage-setting policies that go beyond compensating workers for any such firm-wide, within-job changes in productivity over time help explain the anchoring-to-the-headquarter seen in Table 3.2.

In columns 2 and 4 of Table 3.2 and panels B and D of Figure 3.1, we show that the firm-wide wage correlation we observe in the full sample is driven only by multinationals headquartered in inequality-averse countries as measured by sociologists. This suggests that wage anchoring may occur because some firms have "wage cultures" that compress the wages they pay across locations for reasons that go beyond variation in productivity.[26] It is in principle possible that only firms from inequality-averse countries introduce new technologies or production styles over time that increase labor productivity in jobs of all skill levels, across the firms' worldwide operations, however. To test if such a story can explain the wage anchoring we established in this section in the absence of coexisting wage cultures, *externally imposed* wage changes in the headquarters of firms based in both more and less inequality-averse countries are needed.

[26] A firm's "culture" can manifest itself in wage-setting in many different ways. One example is that, since some societies view a given wage being attached to a given job (akin to the so-called Hay system) as natural (see e.g. Dohmen and Falk, 2011), some multinationals may choose to index the wages of foreign workers in a given position to that of headquarter workers in the same position. Another possibility is that firms from different cultures tend to use Human Resource (H.R.) management systems that differ in how easily a worker's wage can be tied to her productivity (Lemieux et al., 2009).

3.4 Changes in Foreign Establishment Wages in Response to Externally Imposed Changes in Headquarter Wages

In this section we show that, in many multinationals, headquarter wages *directly* affect the wages of domestic workers in foreign establishments. The patterns we document suggest that multinationals from inequality-averse societies have firm-wide wage-setting procedures that help explain the anchoring effects established in Section 3.3.

3.4.1 Average effect of headquarter country minimum wage changes on foreign establishment wages

We take advantage of changes in minimum wages introduced by headquarter country governments to test if headquarter wages causally affect wages paid abroad. To show that minimum wage changes—which are recorded in the ILO's databases (see Section 3.2)—provide a useful source of variation for our purposes, we begin with an event study analysis of the reduced-form relationship between minimum wage shocks in the headquarter country and foreign establishment wages. Specifically, we estimate

$$\begin{aligned}\%\Delta \mathrm{w}_{jfct} = &\sum_{k=-2}^{3} \beta_k^1 \mathbf{I}(\Delta \mathrm{MINw}_{h(f),t-k} > 0) + \theta_{fj} + \theta_{ct} \\ &+ \sum_{k=-2}^{3} \beta_k^2 \%\Delta \mathrm{MINw}_{h(f)t-k} + \varepsilon_{jfct}\end{aligned} \tag{3.2}$$

where $\%\Delta \mathrm{w}_{jfct}$ is the percent change in the average wage of workers in occupation j at firm f's establishment in foreign city c in year t.[27] The independent variable of interest, $\mathbf{I}(\Delta \mathrm{MINw}_{h(f)t-k} > 0)$, is an indicator for the minimum wage in multinational f's headquarter country increasing in year t. Other multinationals in the same city and year are therefore our

[27] We restrict the sample to multinationals that only experience one minimum wage shock during a given five-year period so that we can cleanly identify the effect of a single wage change on establishment wages. In the analysis following the event study, we include all multinationals.

control group. Specifically, the coefficient $\hat{\beta}_k^1$ represents the difference between the change in the wage paid to workers in a specific job in treated foreign establishments (for which the minimum wage in the establishment's headquarter country increases) and that paid to workers in the same job in control establishment in the same city in year k.[28] We control for the magnitude of the corresponding change in the headquarter minimum wage in percent terms, $\%\Delta\text{MINw}_{h(f)t}$, and firm×job and city×year fixed effects as in Section 3.3.

The results are shown in Figure 3.2. The coefficients $\hat{\beta}_k^1$ are plotted relative to the average occupation-level wage paid at an establishment in the year before the minimum wage shock ($k = -1$). We see clear evidence that the average wage of workers in treated establishments increases relative to that in control establishments after a minimum wage increase in the headquarter country. Workers' wages in treated establishments appear to break from the trend, increasing by 6-7 percent one year after the minimum wage shock relative to wages in $k = -1$. In control establishments, workers' wages in the same year increase by 4.5 percent. Importantly, the evolution of the average wage of workers in treated and control establishments is virtually indistinguishable before such minimum wage change events.

The patterns in Figure 3.2 indicate that changes in headquarter country minimum wage laws can be used to estimate the impact that headquarter wage changes (induced by minimum wage shocks) have on establishment wages in the year following the headquarter wage change. We now take advantage of the full analysis dataset to do so. We first show the results from estimating a reduced-form regression relating the wages paid in foreign establishments directly to changes in minimum wages in the home country (controlling for firm×job and city×year fixed effects as above):

$$\%\Delta\text{w}_{jfct} = \alpha\%\Delta\text{MINw}_{h(f)t} + \theta_{fj} + \theta_{ct} + \varepsilon_{jfct} \tag{3.3}$$

[28]Specifically, a control establishment is one that is owned by another multinational in our sample and operates in the same foreign city as a treated establishment at the same point in time, but which is not exposed to an increase in minimum wages in its own headquarter country in the same year as the relevant treated establishments. In Figure 3.2 we do not require treated and control multinationals to operate in the same sector, as many jobs—especially those that are likely subject to the minimum wage—are not specific to a given sector. For example, cleaners and guards are present in most establishments. Allowing multinationals in any sector, but with the same type of jobs, to be included in the control group therefore gives us a larger control sample. The results are robust, however, to restricting to same-sector control groups.

The results are in Table 3.3. We find that a 10 percent increase in the headquarter country's minimum wage ($\%\Delta\mathrm{MINw}_{h(f)t}$) is associated with an increase in foreign establishment wages ($\%\Delta\mathrm{w}_{jfct}$) of about 3.4 percent among the multinationals we study.

We show in the Appendix that the magnitude and statistical significance of this estimate is similar if we restrict the sample to private sector firms (see column 1 of Appendix Table **??**). We also show in the Appendix that we find a precisely estimated null effect of minimum wage changes in the country where a given foreign establishment is based on wages at the headquarter of the parent firm, and a significant but very small effect across foreign establishments that are part of the same firm but located in different countries (see Appendix Table C5). This underscores that wage anchoring is a headquarter effect.

We now turn to the primary relationship of interest; that between a firm's wage level at home and abroad. In the first stage we regress the change in the average wage firm f pays workers in a given job j at the headquarter in year t, $\%\Delta\mathrm{HQw}_{jft}$, on the change in the minimum wage in the country where the headquarter is located, $\%\Delta\mathrm{MINw}_{h(f)t}$. Column 2 of Table 3.3 shows that a 10 percent increase in the headquarter country's minimum wage is associated with an increase in headquarter wages of about 4.6 percent.

We next instrument for the change in occupation-specific headquarter wages using the first-stage estimates. Specifically, we estimate

$$\%\Delta\mathrm{w}_{jfct} = \beta_1 \%\Delta\widehat{\mathrm{HQw}}_{jft} + \theta_{fj} + \theta_{ct} + \varepsilon_{jfct} \tag{3.4}$$

where $\%\Delta\widehat{\mathrm{HQw}}_{jft}$ is the estimate in column 2.[29] $\hat{\beta}_1$ thus captures the extent to which the

[29]In order to include all headquarters and foreign establishments observed in our analysis sample in the estimation procedure, we run two-sample two-stage least squares (TS2SLS) (Angrist and Krueger, 1992; Inoue and Solon, 2010). The corresponding one-sample approach suffers from a weak instrument problem in the first stage due to power issues (especially when estimating heterogeneous effects). The power issues arise because some multinationals in our sample do not provide data to the Company on all of their establishments every year they are surveyed. For this reason, for a substantial fraction of headquarter (foreign establishment) occupation wages we (a) do not observe a corresponding foreign establishment (headquarter) occupation wage in the exact same year, but (b) we do observe such a corresponding occupation wage in another close-in-time year within the same firm. The key assumption for consistency of TS2SLS estimation is that (the probability limit of) the correlation between the endogenous variable(s) and the instruments (conditional on controls) must be similar in the first-stage sample and the second-stage sample. Readers who are concerned about this assumption in our setting can focus on the reduced form estimates in column 1 of tables 3.3 and 3.4.

average wage a given multinational pays domestic workers in a given job abroad is increased when the average wage the firm pays workers in the same job at the headquarter rises by a given amount in a year in which home country policymakers raise minimum wages.

We estimate that a 10 percent minimum wage-induced increase in headquarter wages lead to a 7.3 percent increase in foreign establishment wages, as seen in column 3 of Table 3.3.[30] We interpret the results in Table 3.3 as suggesting that externally imposed changes in multinationals' headquarter wages directly cause changes in their wages in foreign establishments. However, there are two plausible alternative interpretations. The first is that headquarter country minimum wage changes themselves—beyond firms' wage cultures—are endogenous to the wages of foreign workers. It could be, for example, that a given country's policymakers are more likely to raise minimum wages when the country's economy is doing well and aggregate demand for labor is therefore high (see e.g. Baskaya and Rubinstein, 2015; Neumark, 2018). If headquarter country labor demand is highly positively correlated with multinationals' demand for labor abroad, such a channel could contribute to our estimates Table 3.3. Another possibility comes from the influential Feenstra and Hanson (1996) model of outsourcing. Suppose that, initially, a range of higher-skill jobs are done at multinationals' headquarters, and a range of lower-skill jobs in their foreign establishments. An externally imposed increase in headquarter wages—such as a minimum wage increase—could then lead firms to shift the lowest-skill jobs previously done at the headquarter to their foreign establishments. Feenstra and Hanson (1996) point out that jobs that are "exported" (in our case within firms) in this way will tend to be high-skill relative to those previously done for the firm abroad. This could lead wages to rise in the foreign establishments (and, simultaneously, wages in the headquarter would rise due to a combination of the minimum wage increase itself and outsourcing of medium-skill jobs). We investigate the first concern in Sub-section 3.4.2 and the second in Sub-section 3.4.3.

[30] The large magnitude of the increase in foreign establishment wages in response to changes in headquarter country minimum wages relative to the wage change at the headquarter in part reflects the lower starting wage in foreign establishments. When we restrict attention to private sector firms, the results are qualitatively similar but smaller in magnitude, as shown in Panel A Appendix Table C2.

3.4.2 The timing of minimum wage changes and foreign establishment wages

To explore the endogenous-timing-of-minimum-wage-changes concern, we begin by restricting our regressions to within-establishment×year job comparisons. Insofar as fluctuations in demand for foreign labor that co-vary with changes in a headquarter country's minimum wage are similar for lower and higher-paid workers, comparing changes in wages across the two groups enables us to "difference out" the impact of labor demand itself on foreign wages. We compare changes in wages, within a given foreign establishment×year, across the two groups of workers, in a year in which the headquarter country minimum wage is raised relative to a year in which it is not, all in comparison to the same difference-in-differences in control establishments in the same foreign city and years.

Suppose the headquarter country city c is located in raises its minimum wage at some point during our data period. We then define the *prior* minimum wage as binding for job j in city c if the nominal gross wage paid to workers in this job in the year immediately preceding the minimum wage change by some establishment in our sample that is located in city c is lower than the *new* minimum wage.[31] For a job j in a foreign establishment of firm f, the headquarter country minimum wage is then considered binding if $\text{Binding}_{jh(f)} = 1$.[32] The reduced form becomes:

$$\%\Delta \text{w}_{jfct} = \alpha_2 \%\Delta \text{MINw}_{h(f)t} \times \text{Binding}_{jh(f)} + \theta_{fj} + \theta_{fct} + \varepsilon_{jfct} \tag{3.5}$$

[31] Given the unbalanced nature of our establishment×year panel, we a priori face a trade-off between constructing a measure of bindingness that is specific to a given firm/headquarter, and measuring bindingness as close in time as possible to the minimum wage change. We opt for a labor market-level measure of bindingness (akin to Card and Krueger (1995) and subsequent industry-level studies) for power reasons: the sample of firms for which we observe everything required to define a firm-specific measure of bindingness and still compare changes in foreign wages after a minimum wage change within narrowly-defined binding and unbinding occupational categories is too small to achieve meaningful estimates. As discussed above, many jobs—especially those that are likely subject to the minimum wage—are present in many different establishments. We thus view our labor market-based measure of bindingness as simply a loose measure of the relevance of the minimum wage for a given firm×job combination that may lead us to underestimate the differential effect on wages in binding and unbinding jobs within a given firm.

[32] Since we continue to include firm×job fixed effects θ_{fj}, we thus restrict the sample to establishments for which we observe both binding and unbinding jobs. The first stage becomes:
$\%\Delta \text{HQw}_{jft} = \gamma_2 \%\Delta \text{MINw}_{h(f)t} \times \text{Binding}_{jh(f)} + \theta_{fj} + \theta_{fh(f)t} + \eta_{jft}$

where the minimum wage change itself and any possibly correlated demand shocks that affect both high- and low-wage jobs are absorbed by establishment×year fixed effects θ_{fct}. The coefficient on the interaction term captures the differential response of the wage paid to workers in jobs for which the prior headquarter country minimum wage was more binding compared to the wage paid to workers in other jobs within the same foreign establishment at the same point in time.

We find significantly bigger effects of headquarter country minimum wage changes on wages paid to foreign workers in jobs for which the prior minimum wage was binding at the headquarter. The results, shown in Panel A of Table 3.4, imply for example that a 10 percent increase in the headquarter country's minimum wage results in an increase in foreign establishment (headquarter) wages that in jobs for which the prior minimum wage was binding in the headquarter country is around 1.3 (3.3) percentage points greater than in jobs within the same foreign establishment for which the prior minimum wage was not binding in the home country.[33] The second stage estimates in column 3 imply that a 10 percent minimum wage-induced increase in headquarter wages in jobs for which the prior minimum wage was binding results in an increase in foreign establishment wages that is 3.8 percentage points greater than any simultaneous changes in the wages of other jobs in the same foreign establishments.

The estimates in Panel A of Table 3.4 of the component of the co-movement of wages in foreign establishments with headquarter country minimum wage changes that is specific to low-wage jobs, in combination with the results in Panel A, arguably do in fact suggest that headquarter minimum wage increases are more likely to occur when multinationals' demand for foreign labor is high. This can be seen for example by comparing the reduced form estimate of a 1.3 percent differential effect (for a minimum wage increase of 10 percent) in low-wage jobs with the corresponding reduced form estimate of the *average* association

[33]The number of headquarters observed in our sample is naturally much smaller than the number of foreign establishments. The differential effect of minimum wage changes on binding jobs at headquarters—and therefore also the second stage estimate in column 3—is therefore noisily estimated and not significant.

of foreign wages across all jobs and a same-sized minimum wage increase in Table 3.3 (3.4 percent).

However, in line with the existing minimum wage literature (see, among many others, Card and Krueger, 1995; Cengiz et al., forthcoming), the results in Panel B also begin to make clear that, in addition, minimum wage changes *themselves* directly affect the wages the firms in our sample pay in their headquarter countries , and—as shown in this paper for to our knowledge the first time—that such externally imposed changes in headquarter wages directly affect foreign establishment wages.

We now take a different approach and restrict our analysis to within-multinationals-headquartered-in-a-given-country×year comparisons.[34] Doing so is useful for three related reasons. First, unlike in Panel A and Table 3.3, we can now include headquarter country×year fixed effects that fully control for any macro level domestic demand shocks in the headquarter country that that are correlated with changes to the minimum wage. Second, the within establishment×year job comparison approach in Panel A will miss any impact of minimum wage changes on wages in low-wage jobs at home and abroad that is matched by causal spillover effects on wages higher up in the wage distribution within the same establishment that occur through a regular ("culture-free") process of wage-formation (Teulings, 2003; Haanwinckel, 2019) (see also Engbom and Moser (2017)). (While the estimates in Panel A —which *will* include "real" effects higher up in the wage distribution—may on the other hand also capture more than the causal effect of minimum wage changes as discussed above). Third, the approach in Panel A will also miss such within-establishment spillover effects that occur for a different reason, namely because of firm-level wage-setting policies of the form we study in this paper.

Following Lee (1999) and Autor et al. (2016) (see also Neumark (2018)), we compare the wage response of firms for which the prior minimum wage was more versus less binding,

[34] To be able to estimate the impact of minimum wage changes on headquarter wages in the first stage, we thus restrict the sample of establishments×years to those for which our dataset includes wage information on the headquarter for the same year.

measuring *firm*-level bindingness as the ratio between the ex ante minimum wage and the firm's median wage at the headquarter (the so-called Kaitz index).[35] The reduced form becomes:

$$\%\Delta \mathrm{w}_{jfct} = \alpha_1 \%\Delta \mathrm{MINw}_{h(f)t} \times \mathrm{Kaitz}_{ft} + \theta_{fj} + \theta_{ct} + \theta_{h(f)t} + \varepsilon_{jfct} \tag{3.6}$$

where the change in the minimum wage change itself and any correlated macro level demand shocks affecting the headquarter country are absorbed by headquarter country×year fixed effects $\theta_{h(f)t}$. The coefficient on the interaction term captures the differential response by firms for which the prior headquarter country minimum wage was more binding because of the firm's employment structure.

We find significantly bigger effects of headquarter country minimum wage changes on wages paid to foreign workers in firms for which the prior minimum wage was more binding in the headquarter country. The results from estimating (3.6) and the corresponding first and second stage regressions are shown in Panel B of Table 3.4. The estimates imply for example that a 10 percent increase in the headquarter country's minimum wage results in an increase in foreign establishment (headquarter) wages that at a firm at the 90th percentile of "bindingness" is around 4 (7) percentage points greater than at another firm headquartered in the same country that is at the 10th percentile of bindingness.[36]

We now restrict our analysis to within foreign establishment×year job comparisons *within establishments belonging to multinationals based in a given headquarter country*, combining the approaches from panels A and B of Table 3.4. To do so, we restrict the sample of establishment×years to the intersection of those that could be used in panels A and B, and interact our measure of *job-level* bindingness $\mathrm{Binding}_{jh(f)}$ with our measure of *firm-level* bindingness Kaitz_{ft}.[37] Recall that Kaitz_{ft} captures the extent to which a given headquarter

[35] $\mathrm{Kaitz}_{ft} := \mathrm{MINw}_{h(f)t}/\mathrm{MED}\left(\mathrm{HQw}\right)_{fh(f)t}$. The first stage becomes: $\%\Delta \mathrm{HQw}_{jft} = \gamma_1 \%\Delta \mathrm{MINw}_{h(f)t} \times \mathrm{Kaitz}_{ft} + \theta_{fj} + \theta_{h(f)t} + \eta_{jft}$

[36] The 90th percentile of "bindingness" is Kaitz_{ft}=0.266 while the 10th is Kaitz_{ft}=0.022. (0.266-0.022)× 1.702=0.42, (0.266-0.022)× 3.029=0.74.

[37] Notice that this approach is essentially a within-firm version of the difference-in-differences-in-differences strategy Neumark (2018) proposes as a way to overcome the challenge of endogenously timed minimum wage changes in the minimum wage literature. Thompson (2009); Clemens and Wither (2015) use a conceptually

is exposed to minimum wage changes because of its employment structure.

We find significantly bigger effects of headquarter country minimum wage changes on wages paid to foreign workers employed by multinationals from a given country in jobs for which the prior minimum wage was more binding in the headquarter country. These results from our final approach to separating the causal effect of minimum-wage induced changes in multinationals' headquarter wages on the wages they pay abroad from that of any correlated fluctuations in labor demand are shown Table 3.5.

Two important findings stand out. The first is a bound on how much of (what we interpret as) partial transmission of externally imposed wage increases at the headquarter to foreign establishments can plausibly be explained by endogenous timing of minimum wage changes. Columns 1, 3, and 4 of panels A and B of Table 3.5 show that, while the association between headquarter country minimum wage changes and wages paid at the headquarter and foreign establishments is somewhat smaller (although still sizeable and statistically significant) in the "intersection" sample we use in Table 3.5 relative to those found in Table 3.3, the differential effects in more binding jobs and firms are of similar magnitude to those found in Table 3.4.[38] With this in mind, notice that the estimated coefficients on %Δ Min Wage itself and %Δ Min Wage $\times$ Occ'n. Binding are of very similar size to each other in column 2, and that the latter is essentially unchanged when we replace %Δ Min Wage itself with firm$\times$establishment$\times$year fixed effects in column 3. This suggests that endogenously timed mininum wage changes can plausibly explain *at most* around half of the total, average effect of minimum wage-induced increases in headquarter wages on foreign establishment wages we estimated in Table 3.3.[39]

The second important finding from Table 3.5 is that, *within firms whose headquarters are*

similar approach implemented on more aggregate data.

[38]The differential effect in more binding jobs in column 3 of Table 3.5 is the same as in Panel A of Table 3.4. This is because by including firm$\times$establishment$\times$year fixed effects only variation within establishment$\times$years that have both binding and unbinding jobs are identified off of.

[39]More rigorously, endogenous timing of minimum wage changes that might proxy for high demand for labor that is specific to low-paying *firms* within the same headquarter country can explain at most around half of the total estimated effect. We cannot directly test whether there is endogenous timing that is specific to low-wage *occupations*.

heavily exposed to minimum wage changes because of their employment structure, we observe an only modestly differentially greater increase in wages in jobs in foreign establishments for which the prior minimum was binding in the headquarter country than in those for which it was not, after an increase in minimum wages induce firms to increase wages at home. Even more starkly, in such firms we see no evidence of a differentially greater increase in wages in binding relative to unbinding jobs at the headquarter. While hard to reconcile with the kinds of stories that could explain the results in Panel A of Table 3.4 in the absence of transmission of externally imposed wage increases to foreign establishments—demand for labor that is specific to low-wage jobs driving minimum wage changes—these findings have a natural interpretation insofar as firms have "wage cultures". In particular, firms may then choose to raise the wages of all or most workers in a given establishment when they are induced to raise the wages of a high proportion of those workers by changes to minimum wage laws.[40]

In combination, the evidence presented in this sub-section makes clear that endogenous timing of minimum wage changes cannot fully explain the transmission of externally imposed headquarter wage increases to multinationals' foreign establishments we estimated in Table 3.3.

3.4.3 Outsourcing in response to minimum wage changes and foreign establishment wages

Recall that another alternative interpretation of the results in Table 3.3 is "within-firm-outsourcing": an externally imposed increase in multinationals' headquarter wages raising foreign establishment wages due to an induced change in the composition of jobs present at home and abroad when some medium-skill jobs are "exported", akin to Feenstra and Hanson (1996).

[40]Not surprisingly, jobs for which the prior minimum wage was binding for the wages of the corresponding jobs at the headquarter make up a much higher proportion of all jobs in the foreign establishments in our sample where such jobs are present than they do among the corresponding headquarters.

However, the set of narrowly-defined occupations that are present in the foreign establishments of firms that appear to transmit externally imposed headquarter wage increases abroad *shrinks*, and the set present at such firms' headquarters *expands*, when wage increases (at the headquarter) are required. We show this in Section 3.5, where we analyze how the occupational structure of multinationals' various establishments responds to changes in the minimum wage in the headquarter country. This pattern of occupational compression at foreign establishments and expansion at the headquarter is the opposite of what a within-firm-outsourcing-explaining-wage-changes story would predict. It could be that such a mechanism is at play *within* occupations; that is, that the medium-skill tasks associated with a given occupation are reallocated from workers in the relevant occupation at the headquarter to workers in the same occupation at foreign establishments. However, for such a phenomenon to be the primary explanation underlying the estimates in Table 3.3, extensive within-firm-outsourcing of tasks within narrowly-defined occupations would need to occur in parallel with essentially the opposite happening across occupations.

Another way to investigate the within-firm-outsourcing-explaining-wage-anchoring concern is to compare the correlation between headquarter and foreign establishment wage levels and changes in complex, multi-task jobs, and simpler, "single-task" jobs. Recall that column 5 of Table 3.2 shows estimates of equation (3.1) separately for low, medium, and high-skill positions. We observe a considerably greater extent of anchoring-to-the-headquarter in low-skills jobs. In addition, we saw in Panel A of Table 3.4 that wages rise significantly *more* in low-wage than high-wage jobs in foreign establishments when the minimum wage is increased in the headquarter country. It is arguably unlikely that firms reallocate tasks from, for example, drivers, cleaners, and guards in the headquarter country to those in foreign establishments when wages rise in the headquarter country.

Finally, in Panel A of Appendix Table C3 we re-estimate the regression in Table 3.3, restricting the set of occupations that were already present in the relevant foreign establishment in the immediately preceding year surveyed. The results are similar to those in Table

3.3, although of slightly smaller magnitudes. We conclude that the evidence suggests that a within-firm-outsourcing phenomenon is not the primary explanation for the transmission of externally imposed headquarter wage increases to multinationals' foreign establishments we estimated in Table 3.3.

In combination, the results in sub-sections 3.4.1 – 3.4.3 suggest that the estimated impact of increases in multinationals' headquarter wages on foreign establishment wages is at least in part—and most likely primarily—a direct effect due to wage anchoring.

3.4.4 Understanding the effect on foreign establishment wages

In this section we have so far documented that externally imposed increases in the wages multinationals pay workers in a given job at the headquarter lead to increases in the wages they pay workers in the same job abroad. Such a global response to externally imposed increases in headquarter wages points towards the existence of firm-wide wage-setting procedures. We saw in Section 3.3 that the headquarter-foreign establishment wage correlation we observe in the full sample is driven by multinationals headquartered in inequality-averse countries as measured by sociologists. A natural question is therefore whether such multinationals also drive the impact of externally imposed headquarter wage *changes* on foreign wages.

This is indeed what we find in Table 3.6, where we repeat the regressions from columns 1 and 2 of Table 3.3, now interacting $\%\Delta\text{MINw}_{h(f)t}$ with the same measure of societal inequality-aversion we used in Section 3.3. Remarkably, while the estimated coefficient on $\%\Delta$ Min Wage $\times$ Low Ineq. Aversion is small and statistically insignificant when the outcome variable is the associated percent change in *headquarter* wages, the coefficient on $\%\Delta$ Min Wage $\times$ Low Ineq. Aversion is negative and almost equal in (absolute value) magnitude to the (positive) coefficient on $\%\Delta$ Min Wage itself.[41] Both the wage correlation in levels (general anchoring to headquarter) and changes (partial transmission of externally imposed wage

[41]The same holds when we restrict attention to private sector firms or to pre-existing jobs, as shown in Panel B Appendix Tables C2 and C3.

increases to foreign establishments) we uncover in this paper are thus driven by employers from inequality-averse societies. This is in itself difficult to reconcile with the endogenous minimum wage changes and within-firm outsourcing concerns discussed in sub-sections 3.4.2 and 3.4.3, instead pointing towards the existence of wage cultures.

3.5 Changes in Firm-wide Wages and Firms' Organizational Structure

The consequences of across-country wage compression in multinationals may be far-reaching and multi-dimensional. We leave a deeper investigation for future research, but now take a first step towards investigating the consequences for firms' job location decisions.

Existing evidence suggests that wage compression can lead high-wage firms to outsource low-wage jobs to avoid paying a premium for low-skill workers (Goldschmidt and Schmieder, 2017). If so, firms that anchor their wages to headquarter levels may face an incentive to limit the number of occupations in their establishments outside of the home region, especially in the face of externally imposed headquarter wage increases that spill over to establishments. We test this conjecture by estimating the impact that a minimum wage change in the headquarter country has on the presence of occupations in a firm's foreign establishments and headquarter.

In each year, we observe whether an establishment employs workers in a given occupation. From this we define two outcomes, "Occupation Removed" and "Occupation Added", and estimate the following:

$$\begin{aligned}\text{Removed}_{jfct} = {} & \beta_1 \%\Delta\text{MINw}_{h(f)t} + \beta_2 \%\Delta\text{MINw}_{h(f)t} \times \text{Low Ineq. Aversion} \\ & + \theta_{fj} + \theta_{ct} + \varepsilon_{jfct}\end{aligned} \tag{3.7}$$

and

$$\text{Added}_{jfct} = \beta_1 \%\Delta\text{MINw}_{h(f)t} + \beta_2 \%\Delta\text{MINw}_{h(f)t} \times \text{Low Ineq. Aversion} \qquad (3.8)$$
$$+ \theta_{fj} + \theta_{ct} + \varepsilon_{jfct}$$

Removed_{jfct} takes the value one if firm f employed workers in occupation j in establishment c in year t (i.e. we see the occupation code in a firm's establishment in year t), but that same occupation does not exist in establishment c year $t+1$, and vice versa for Added_{jfct}. We test whether the relative wages of a firm's headquarter and establishment play a role in job location decisions by interacting $\%\Delta\text{MINw}_{h(f)t}$ with the "Low Inequality Aversion" dummy. Finally, we include firm×job and establishment country×year fixed effects as above.

The results from estimating equations (3.7) and (3.8) are presented in Table 3.7. In columns 1-4, we consider occupations in foreign establishments, and in columns 5-6 we look at the impact on occupations at headquarters. Column 1 suggests that wage increases in the headquarter raise the probability that occupations are removed from a foreign establishment. However, Column 2 shows that this result is driven entirely by multinationals headquartered in countries with strong fairness norms. In response to a 10 percent minimum wage increase in the headquarter country, the probability that an occupation is removed from the foreign establishments of multinationals headquartered in countries that are highly averse to inequality increases by 1.8 percentage points. However, there is a precisely estimated zero impact in foreign establishments of multinationals headquartered in countries with low levels of inequality aversion.

A similar pattern is seen when looking at multinationals adding occupations to their foreign establishments. In response to a 10 percent minimum wage increase, the probability that an occupation is added to the foreign establishments of multinationals headquartered in countries that are highly averse to inequality declines by 1 percentage points, but does not change for those headquartered in less inequality averse countries.

In columns 5-6 of Table 3.7 we show results from estimating equations (3.7) and (3.8) for headquarters. Multinationals headquartered in inequality averse countries are less likely to remove occupations from the headquarter (Column 5), while multinationals from less inequality averse countries are more likely to remove occupations from the headquarter, in response to externally imposed wage increases at the headquarter. Column 6 indicates that firms headquartered in inequality averse countries are more likely to add occupations to the headquarter, while there is no such impact for firms headquartered in less inequality averse countries.

Recall that the multinationals that appear to have "wage cultures" and partially transmit wage increases that are externally imposed on the headquarter to their foreign establishments are those headquartered in inequality averse countries. In contrast, multinationals headquartered in less inequality averse countries do not change the wages they pay abroad when their headquarter wage levels rise. The results in this section suggest that, in parallel with these contrasting forms of wage-setting, wage increases at the headquarter affect the occupational structure of the two types of firms differently. When wages are increased at the headquarter, and the increase partially transmitted to foreign establishments, "inequality-averse firms" also compress the occupational structure of their foreign establishments. They do so by adding fewer and removing more occupations than they otherwise would have. Simultaneously, such firms expand the occupational structure of their headquarter. In contrast, "less inequality-averse firms" do not change the occupational structure of their foreign establishments, but compress the occupation structure of their headquarters, when wages rise (only) at the headquarter.[42]

3.6 Conclusion

The evidence in this paper makes clear that many large multinationals have firm-wide wage-setting procedures that are not adjusted to the local labor market conditions in each location they operate in. We show that, in particular, multinationals headquartered in inequality-

[42]The latter may occur through outsourcing, as in Goldschmidt and Schmieder (2017), or job destruction, when wages rise.

averse countries anchor the wage they pay *domestic* workers in a given occupation at foreign establishments to the wage they pay workers in the same occupation in the home country. They do so across the occupational skill range—including for low-skill support staff—and transmit wage increases *externally imposed* on the headquarter (via changes in the headquarter country's minimum wage) in part to their foreign establishments. Multinationals headquartered in less inequality-averse countries do not anchor their wages abroad to headquarter levels. Our results thus point towards the existence of "wage cultures" lead some firms to pay workers of similar skill levels more than others.

An important topic for future research is how costly across-country wage compression in multinationals ultimately is to firms and workers. We take a first step towards understanding the consequences of wage anchoring by showing that the multinationals that anchor wages abroad to the headquarter re-organize the occupational structure of their foreign establishments in relation to that at the headquarter when wage increases are externally imposed on the headquarter. In particular, such firms compress the occupational structure at their foreign establishments and expand that at the headquarter. Multinationals that that do not anchor wages abroad to the headquarter also do not change the occupational structure of their foreign establishments when wage increases are externally imposed on the headquarter.

However, the broader consequences of across-country wage compression may be far-reaching and multi-faceted, complicating a cost/benefit calculation. On the one hand, simple job-based wage-setting systems are cheaper in use for firms than productivity-based systems (Lemieux et al., 2009). It could also be that paying high wages over time allows firms to attract better workers, or motivate higher effort among existing workers, or complementary investments from the firm—essentially an *ex post* adjustment version of the productivity-based conventional understanding of the multinational pay premium discussed in the introduction. However, to explain the evidence in this paper, any such *ex post* adjustments would need to occur also for low-skill workers for which the labor market is local, and for which the complementarity between labor and firm investments is presumably small.[43] On the other hand, Boeri et al. (2018)'s evidence on Italy's spatial wage-equalization scheme suggests that wage-setting procedures that are not adjusted to local labor market conditions can be very costly to firms and workers.[44]

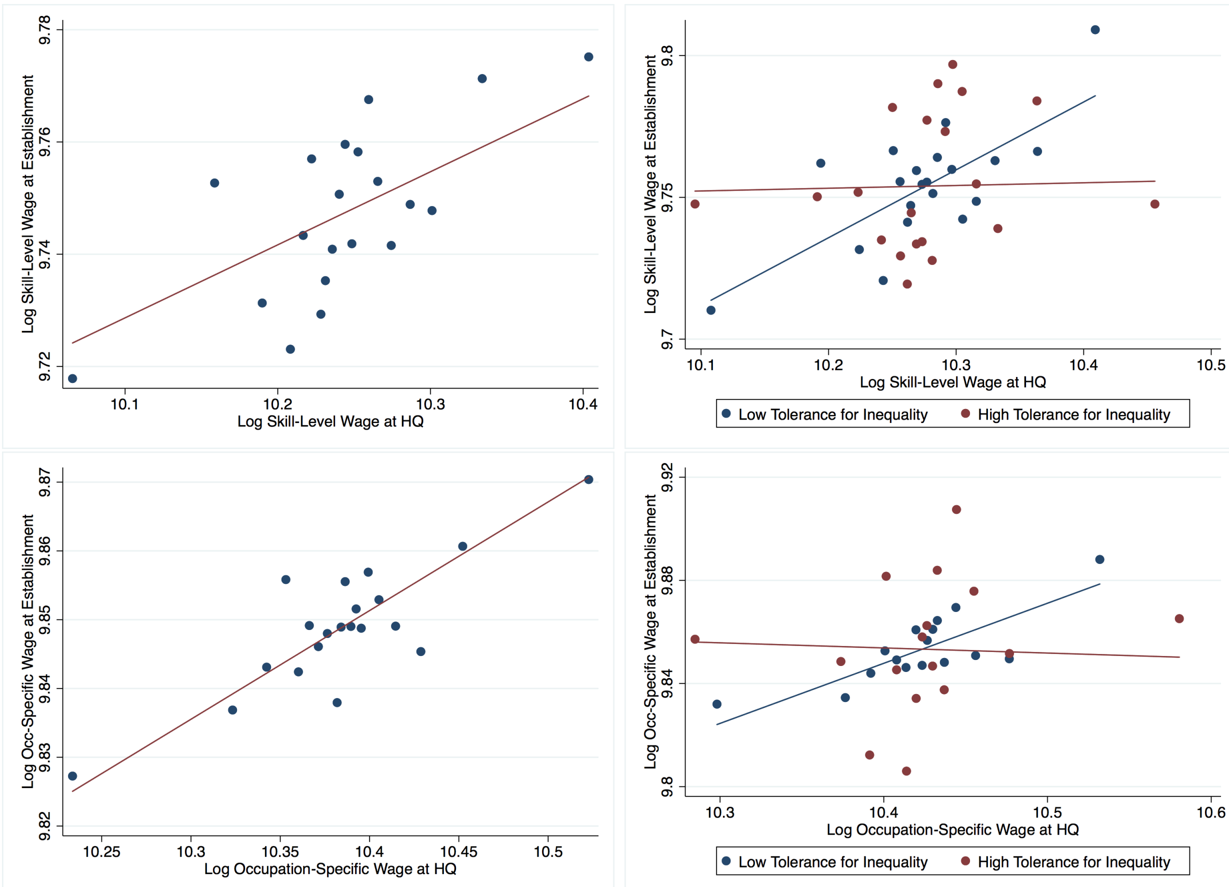
We conclude that ex post adjustments to wage changes in foreign establishments over time will need to be surprisingly large in order for firm-wide wage-setting procedures not to prove costly for multinationals whose wages abroad are in real terms an order of magnitude higher than the wages they pay workers in the same occupations in the home country. To understand the full welfare impact of across-country wage compression—including any any resulting misallocation of occupations and jobs across regions—equilibrium models of national, regional, or global production in which the wage discount associated with producing in a low-wage location for some firms depend on *the firm's origin* may be needed.

[43] Workers in low-skill jobs being significantly more productive in high-wage firms is arguably inconsistent with the evidence in Goldschmidt and Schmieder (2017).

[44] The related across-country evidence in Harrison and Scorse (2010) is more mixed.

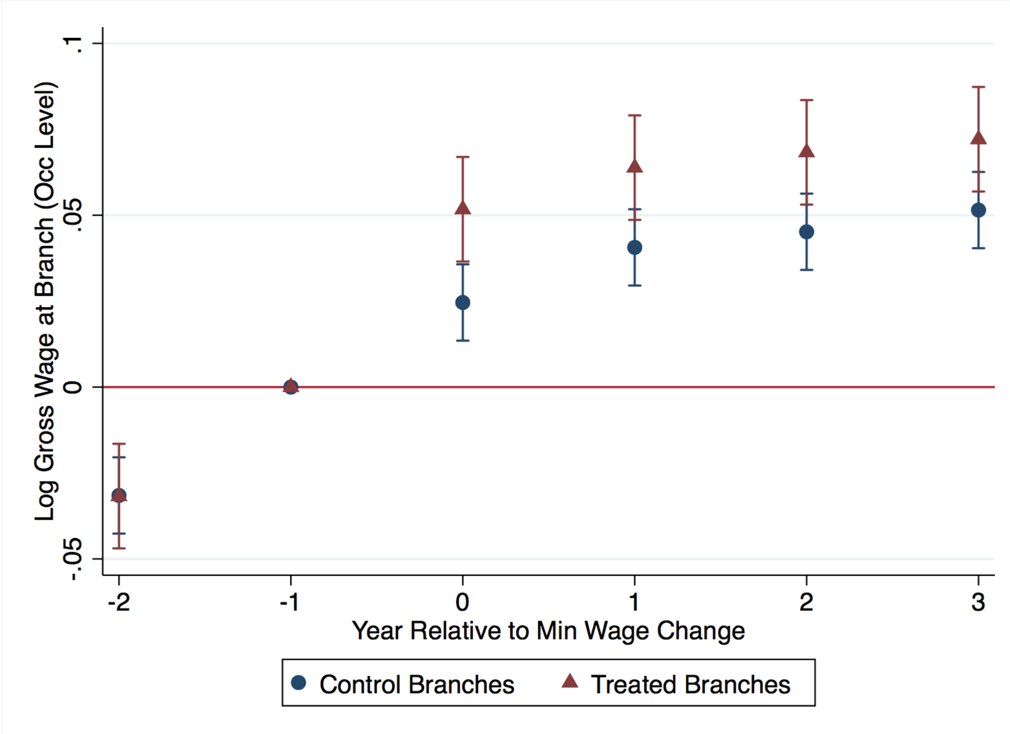
3.7 Figures

Figure 3.1: Correlation between HQ and Establishment Wages



Note: These binned scatterplots show the relationship between headquarter and establishment wages by skill level (Panels A and B) and occupation level (Panels C and D). The y-variable in Panels A and B is the skill-level log wage at an establishment. The y-variable in Panels C and D is the occupation-specific log wage at an establishment. In Panels B and D, we split the sample into countries that have a high or low tolerance to inequality, as defined in Section 3. To construct each plot, establishment wages are first residualized with respect to the following controls: the average skill-level or occupation-level wage for other employers' establishments operating in the same country, establishment country-year fixed effects, and firm-skill level (or firm-occupation level) fixed effects. The x-variable, log wage at the headquarter, is then divided into twenty equal-sized groups. Wages at headquarter are also measured at either the skill level (A and B) or the occupation level (C and D). Within each of these groups, we plot the mean of the y-variable residuals against the mean of the x-variable. We then add back the unconditional mean of the y-variable (establishment wages), to help with the interpretation of the line of best fit. The lines of best fit for each scatter plot are as follows. Panel A: $\hat{\beta} = 0.130$ (s.e.=0.030). Panel B: $\hat{\beta}_L = 0.239$ (s.e.=0.044) and $\hat{\beta}_H = 0.010$ (s.e.=0.047). Panel C: $\hat{\beta} = 0.126$ (s.e.=0.027). Panel D: $\hat{\beta}_L = 0.205$ (s.e.=0.036) and $\hat{\beta}_H = 0.017$ (s.e.=0.056).

Figure 3.2: Impact of Minimum Wage Shock on Occupation-Specific Establishment Wages



Note: This event study plots the coefficients from a regression in which occupation-specific establishment wages are regressed on year dummies. A minimum wage shock in the HQ occurs in $t = 0$. All coefficients are plotted relative to the average wage in the establishment in $t = -1$ (the year before the shock). A treated establishment is an establishment in country c whose HQ experienced a minimum wage shock. Control establishments are other firms' establishments in country c in the same sector s for which the HQ did not experience a minimum wage shock.

3.8 Tables

Table 3.1: Summary Statistics of Multinationals

Panel A: Full Sample	Mean (1)	SD (2)	Min (3)	Max (4)
Net Wage (USD)	20,242.8	24,897.2	571.6	173,029.2
Number of Occupations	28.2	18.3	2	126
Number of Skill Levels	10.7	1.4	1.0	16.0
Number of Establishments	3.6	11.2	1.0	152.0
Observations	8,334	8,334	8,334	8,334

Panel B: By Sector	Public Sector Orgs. (1)	Private Sec (2)	NGOs (3)
Net Wage (USD)	19,845.07	26,242.22	20,857.35
# Employers	148	1098	560

Panel C: Distribution of Wages	HQ-Quart1 (1)	HQ-Quart2 (2)	HQ-Quart3 (3)	HQ-Quart4 (4)
Headquarter				
Wage	10,100.53	15,647.67	23,367.12	39,720.93
Establishments				
Wage as % of HQ Wage	66.5	74.1	80.3	83.1

All variables are measured at the employer-year level. There are 5,894 employer-year observations and 1,806 total employers. "Net Wage" is the average wage of all employees in a firm in a given year, and is measured in 2005 USD. "Number of Occupations" is the number occupations a firm has (across all foreign establishments) in a given year. "Skill levels" is average number of skill levels that exist in a firm. "Number of Establishments" is the number of establishments that a firm has operating in countries outside of the headquarter country. In Panel B, we separate employers into public sector organizations, private sector employers, and NGOs. All numbers in Panel B are means. In Panel C, we show the average wages at a firm's headquarter within a quartile. We then show the average wage in the firm's establishments as a percentage of headquarter wages for each quartile.

Table 3.2: Relationship between HQ and Establishment Wages

Log Wage at Establishment by:	Skill Level		Occupation		
	(1)	(2)	(3)	(4)	(5)
Log Wage at HQ (Skill)	0.115**	0.256***			0.264***
	(0.054)	(0.046)			(0.062)
Log Wage at HQ (Skill) × Low Ineq. Aversion		-0.260***			
		(0.086)			
Log Wage at HQ (Occ'n)			0.144***	0.223***	
			(0.051)	(0.051)	
Log Wage at HQ (Occ'n) × Low Ineq. Aversion				-0.185*	
				(0.099)	
Log Wage at HQ (Occ'n) × Med Skill Occ.					-0.149***
					(0.041)
Log Wage at HQ (Occ'n) × High Skill Occ.					-0.111**
					(0.003)
Log Benchmark Wage	0.061***	0.062***	0.005*	0.005*	0.005**
	(0.011)	(0.011)	(0.003)	(0.003)	(0.003)
Observations	9,201	9,201	9,659	9,659	9,659
R-squared	0.954	0.954	0.960	0.962	0.956

Note: This table shows the correlation between a firm's wage levels at its headquarter and its establishments. The outcome variable in columns 1 and 2 is the skill-level-specific log wage at an establishment. The outcome in columns 3 and 4 is the occupation-specific log wage at an establishment. In columns 2 and 4, we interact the main independent variable, headquarter wage, with a binary variable indicating whether a country is classified as having low inequality aversion according to the Hofstede measures of culture. If the variable "Low Ineq. Aversion" equals one, it indicates that the country is more accepting of inequality than the average country in the sample.

Table 3.3: Impact of Home Min. Wage Change on Firm Wages at Home and Abroad

	%Δ Estab. Wage	%Δ HQ Wage	%Δ Estab. Wage
	(1)	(2)	(3)
%Δ Min Wage	0.339***	0.463***	
	(0.086)	(0.146)	
%Δ HQ Wage (IVed)			0.731**
			(0.296)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	218,352	5,900	218,352
R-squared	0.248	0.443	0.248

Note: This table shows the impact that a 100% minimum wage increase in a firm's home country has on gross wages in its foreign establishments (column 1) and its headquarter (column 2). We perform two-sample 2SLS estimation in column 3, where the full headquarter sample (first stage, column 2) and the full foreign establishment sample (second stage, column 1) are used. Outliers with wage changes larger than 4 or smaller than -0.8 are excluded. Standard errors are reported in parentheses and clustered at the firm level. TS2SLS standard errors are computed following Pacini and Windmeijer (2016). (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table 3.4: Impact of Home Min. Wage Change for Binding Occupations and Firms

Panel A: Within Establishment -Year Across Occupations	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage × Occ'n. Binding	0.130***	0.334	
(Binary: below New Min Wage)	(0.054)	(0.231)	
%Δ HQ Wage (IVed)			0.388
			(0.312)
Occupation-Firm FE	Y	Y	Y
Firm-HQ-Year FE	Y	Y	Y
Firm-Estab.-Year FE	Y	-	Y
Observations	218,305	5,900	218,305
R-squared	0.684	0.719	0.684

Panel B: Within HQ Country-Year Across Firms	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage ×Firm Bindingness	1.702***	3.029***	
(Kaitz: Min Wage-Median Wage Ratio)	(0.411)	(0.242)	
%Δ HQ Wage (IVed)			0.562**
			(0.143)
Occupation-Firm FE	Y	Y	Y
HQ country-Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	17,522	2,520	17,522
R-squared	0.590	0.667	0.590

Note: Panel A shows the impact that a 100% minimum wage increase in a firm's home country has on gross wages in its foreign establishments (column 1) and its headquarter (column 2) depending on whether an occupation is binding in the headquarter country (had a wage that was below the new minimum wage in the headquarter). In Panel B, only the firms of which the headquarter and at least one foreign establishment are observed are included, as the Kaitz index is only available for these firms. We perform two-sample 2SLS estimation in column 3, where the full headquarter sample (first stage, column 2) and the full foreign establishment sample (reduced form, column 1) are used. Outliers with wage changes larger than 4 or smaller than -0.8 are excluded. Standard errors are reported in parentheses and clustered at the firm level. TS2SLS standard errors are computed following Pacini and Windmeijer (2016). (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table 3.5: Impact of Home Min. Wage Change for Binding Occupations and Firms (II)

Panel A: Reduced Form	%Δ Establishment Wage				
	(1)	(2)	(3)	(4)	(5)
%Δ Min Wage	0.126***	0.120**			
	(0.049)	(0.050)			
%Δ Min Wage × Occ'n. Binding		0.123**	0.129***		
		(0.060)	(0.055)		
%Δ Min Wage × Firm Bindingness				1.113***	1.112***
				(0.141)	(0.139)
%Δ Min Wage × Firm Bindingness					0.181***
× Occ'n. Binding					(0.044)
Occupation-Firm FE	Y	Y	Y	Y	Y
Estab. country-Year FE	Y	Y	Y	Y	Y
HQ country-Year FE	N	N	N	Y	Y
Firm-Estab.-Year FE	N	N	Y	N	N
Observations	11,224	11,224	11,224	8,290	8,290
R-squared	0.548	0.548	0.613	0.586	0.586
Panel B: First Stage	%Δ HQ Wage				
	(1)	(2)	(3)	(4)	(5)
%Δ Min Wage	0.286***	0.246**			
	(0.105)	(0.110)			
%Δ Min Wage × Occ'n. Binding		0.294	0.334		
		(0.231)	(0.234)		
%Δ Min Wage × Firm Bindingness				3.009***	3.021***
				(0.247)	(0.212)
%Δ Min Wage × Firm Bindingness					-0.073
× Occ'n. Binding					(0.448)
Occupation-Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
HQ country-Year FE	N	N	N	Y	Y
Firm-HQ-Year FE	N	N	Y	N	N
Observations	2,575	2,575	2,575	1,318	1,318
R-squared	0.365	0.370	0.780	0.693	0.693
Panel C: IV	%Δ Establishment Wage				
	(1)	(2)	(3)	(4)	(5)
%Δ HQ Wage (IVed)	0.442*	0.467**	0.388	0.370***	0.367
	(0.236)	(0.220)	(0.317)	(0.056)	(0.284)

Note: This table compares the differential impact of minimum wage increase in a home country on the gross wages paid to (i) home-minimum-wage-binding and unbinding occupations and (ii) firms which differ in firm-level minimum-wage-bindingness in foreign establishments (Panel A) as well as headquarters (Panel B). An occupation is minimum-wage-binding if it had a wage in the headquarter that was below the new minimum wage of the homer country in the immediately preceding year. Firm-level minimum-wage-bindingness is measured by the ratio between the home country minimum wage and the headquarter median wage (so-called Kaitz index). Only establishment-year cells in which at least one home-minimum-wage-binding occupation exists are included. In columns (4) and (5), only the firms of which the headquarter and at least one foreign establishment are observed are included, as the Kaitz index is only available for these firms. Associated IV (TS2SLS) estimates of the relationship between foreign establishment wage changes and headquarter wage changes are reported in the corresponding columns in panel C (reduced form results in panel A and first stage results in panel B). Outliers with wage changes larger than 4 or smaller than -0.8 are excluded. Standard errors are reported in parentheses and clustered at the firm level. TS2SLS standard errors are computed following Pacini and Windmeijer (2016). (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table 3.6: Heterogeneous Wage Effects of Home Minimum Wage Change

	%Δ Estab. Wage	%Δ HQ Wage	%Δ Estab. Wage
	(1)	(2)	(3)
%Δ Min Wage	0.585***	0.622***	
× High Ineq. Aversion	(0.103)	(0.237)	
%Δ Min Wage	-0.012	0.459***	
× Low Ineq. Aversion	(0.075)	(0.153)	
%Δ HQ wage			0.940**
× High Ineq. Aversion (IVed)			(0.395)
%Δ HQ wage			-0.027
× Low Ineq. Aversion (IVed)			(0.164)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	218,352	5,900	218,352
R-squared	0.252	0.450	0.252

Note: This table looks at heterogeneity in the impact that a 100% minimum wage increase in a firm's HQ country has on gross wages in its foreign establishments (column 1) and headquarter (column 2), based on whether the home country has high or low inequality aversion. Inequality aversion is defined according to the Hofstede measures of culture. We use the "Power Distance" index, which measures the extent to which people in a group or society accept that power and opportunity is distributed unequally. In the table, "Low Ineq. Averse" ("High Ineq. Averse) is a dummy variable indicating that the country has an above-average (below-average) tolerance for inequality (a high Power Distance Index score). We perform two-sample 2SLS estimation in column 3, where the full headquarter sample (first stage, column 2) and the full foreign establishment sample (reduced form, column 1) are used. Outliers with wage changes larger than 4 or smaller than -0.8 are excluded. Standard errors are reported in parentheses and clustered at the firm level. TS2SLS standard errors are computed following Pacini and Windmeijer (2016). (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table 3.7: Impact of Home Minimum Wage Change on Occupations

	Establishment				Headquarter	
	Occupation Leaves		Occupation Added		Occ. Leaves	Occ. Added
	(1)	(2)	(3)	(4)	(5)	(6)
%Δ Min. Wage	0.102	0.189*	-0.048**	-0.101***	-0.697***	0.096***
	(0.066)	(0.099)	(0.029)	(0.038)	(0.233)	(0.033)
%Δ Min. Wage		-0.188*		0.100**	1.239***	-0.100***
× Low Ineq. Aversion		(0.113)		(0.041)	(0.318)	(0.031)
Occupation-Firm FE	Y	Y	Y	Y	Y	Y
Estab. Country-Year FE	Y	Y	Y	Y	N	N
HQ Country FE	N	N	N	N	Y	Y
Year FE	N	N	N	N	Y	Y
Observations	169,743	169,743	1,090,051	1,090,051	102,329	825,707
R-squared	0.492	0.492	0.110	0.110	0.582	0.027

Note: This table shows the impact of a 100% minimum wage increase in a firm's HQ country on the existence of occupations in the firm's foreign establishments (columns 1-4) and headquarter (columns 5-6). The outcome variables in columns 1, 2 and 5 is a dummy variable indicating that an occupation that previously existed in a firm's establishment or HQ no longer existed in the year after the minimum wage increase. The outcome variable in columns 3, 4, and 6 is a dummy variable indicating that an occupation that did not exist in a firm's establishment or HQ before the minimum wage increase, appeared in the establishment or HQ in the year following the minimum wage increase. "Low Ineq. Averse" is a dummy variable indicating that the country has an above-average tolerance for inequality (a high Power Distance Index score). Standard errors are reported in parentheses and are clustered at the firm-HQ country level. (*=p<0.10, **=p<0.05 ,***=p<0.01)

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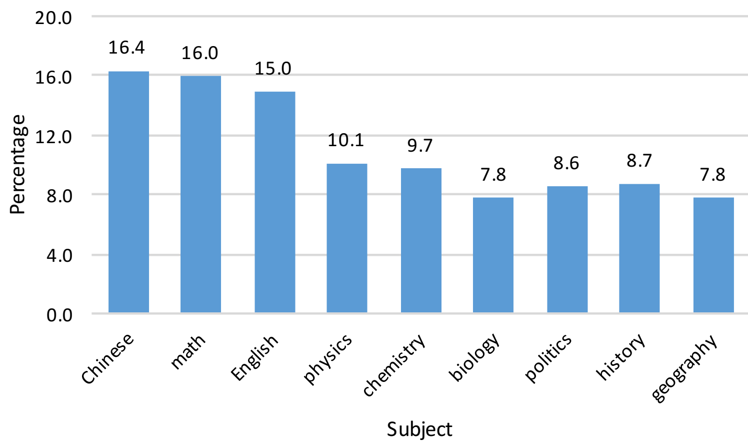
Appendix A

Appendix of Chapter I

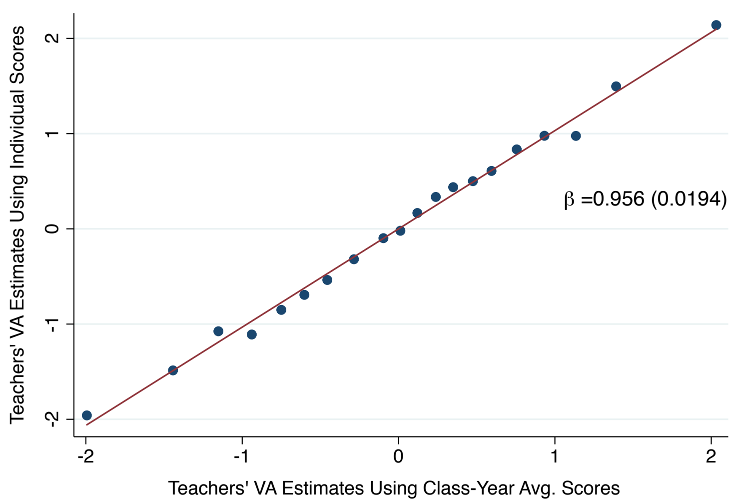
A1 Figure Appendix

Figure A1: Distribution of Subjects Taught by Applicants



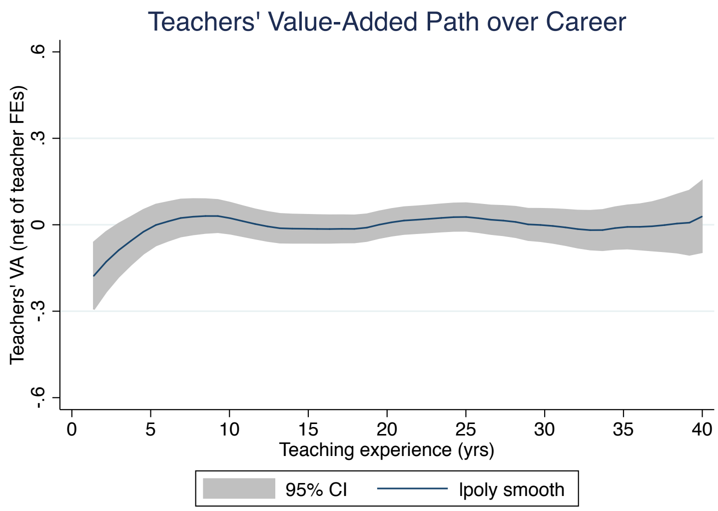
Notes: The unit of analysis is the applicant-year. N=59,121.

Figure A2: Value-Added Measures: Class-year Average Scores vs Students' Individual Scores



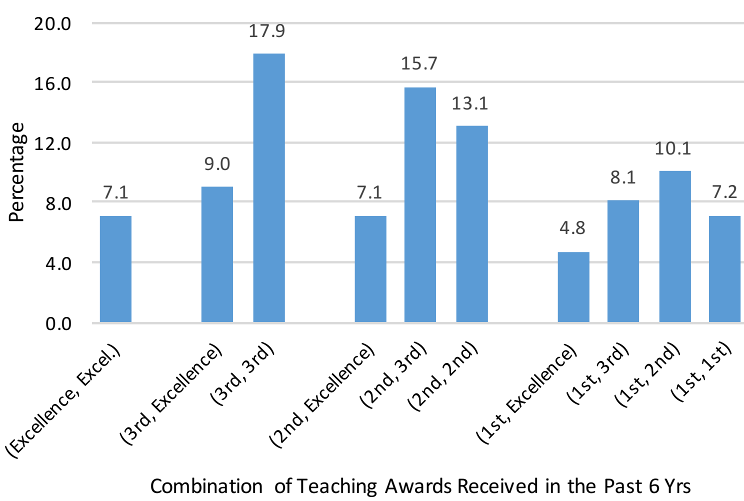
Notes: This graph plots the correlations between the VA estimates described in Section 1.2.4.2 and those using the Chetty et al. (2014a) method for teachers in city A in years 2009-2017.

Figure A3: Value-Added and Teaching Experience



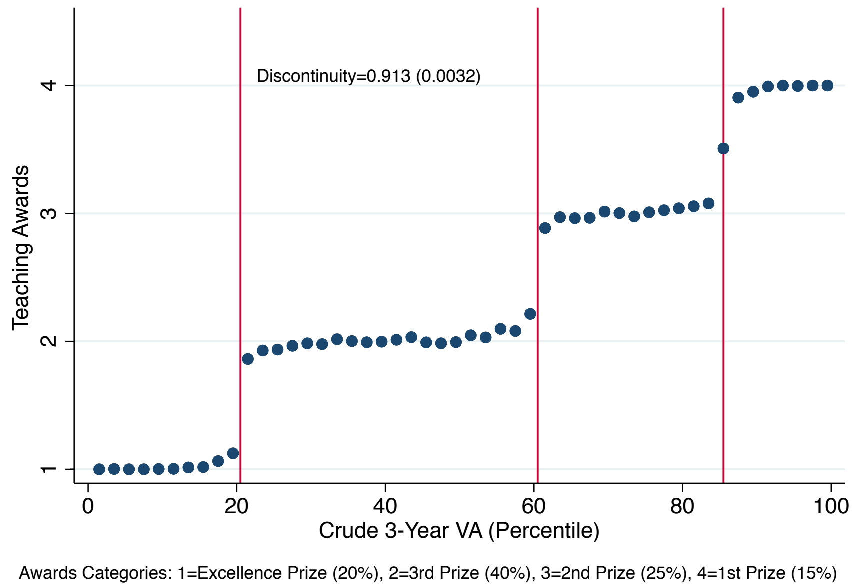
Notes: The graph shows the estimated non-parametric experience function $\hat{h}(\cdot)$ from estimating equation $\text{VA}_{it} = \text{VA}_{ih} + h(experience_{it}) + \epsilon_{it}$, where h is a local polynomial function using Epanichnikov kernel with bandwidth 3.25. N=210,424.

Figure A4: Distribution of Teaching Award Combinations



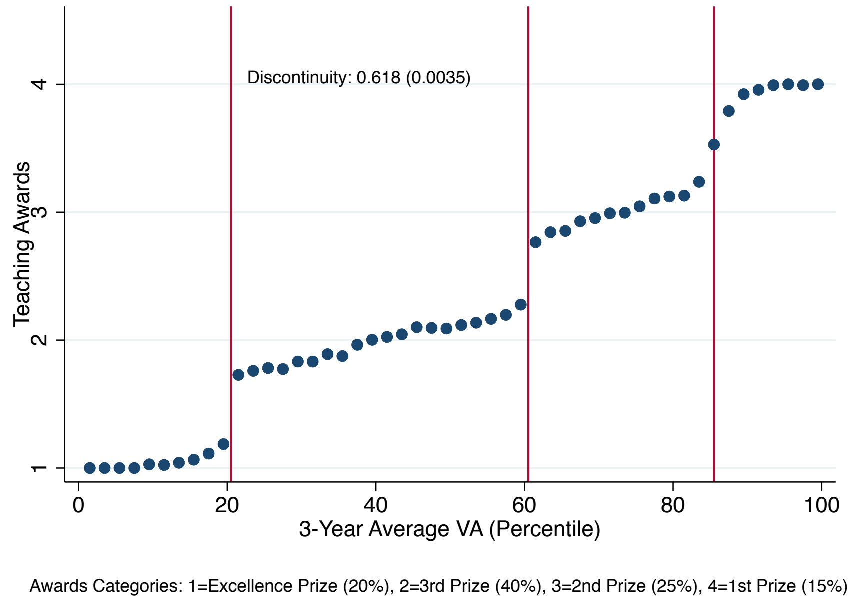
Notes: This graph plots the distribution of the combinations of the 2 highest teaching awards received by the applicants in the past 6 years. The unit of analysis is the applicant-year. N=59,121. The College Entrance Exam teaching awards are awarded in 4 levels (from low to high): Excellence Prize (20%), 3rd Prize (40%), 2nd Prize (25%) and 1st Prize (15%). As the teachers are asked to list the teaching awards in the past 6 years in their application, and it is common practice that a teacher follows the same class for its 3-year duration of high school from their entry to graduation, and the awards are based on the evaluation of the test performance of the graduating cohort, in most cases the applicants list 2 teaching awards lin their application forms. Only 9.7% of applicants list more than 2 awards. In those cases, I use the combination of their 2 highest awards.

Figure A5: Rule of Teaching Awards Assignment



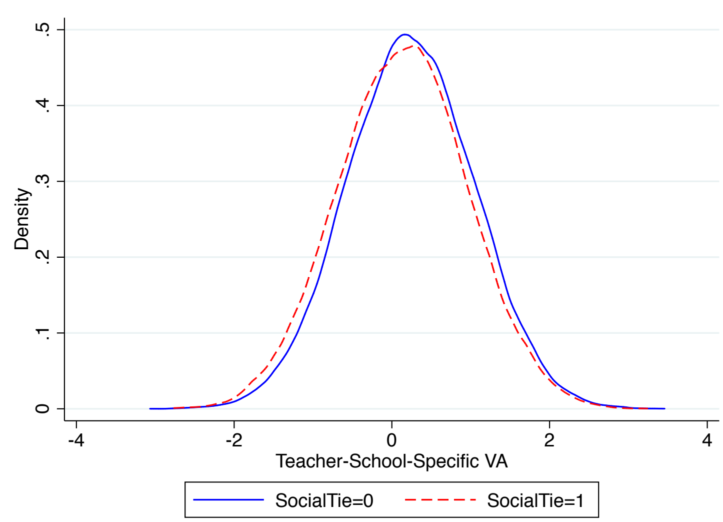
Notes: This graph checks whether the teaching awards are assigned according to the stated rule, which says "the College Entrance Exam teaching awards for teachers are based on the ranking of the difference between their students' average standardized (SD=1 within subject-city-year) College Entrance Exams (CEE) scores and their average standardized High School Entrance Exams (HEE) scores." Awards (from low to high) include Excellence Prize (20%), 3rd Prize (40%), 2nd Prize (25%) and 1st Prize (15%). The x-axis variable, crude 3-year VA is the (percentilized) difference between the average standardized CEE scores and HEE scores of the classes a teacher teaches. On y-axis is a discrete-valued variable representing different levels of teaching awards: Excellence=1, 3rd Prize=2, 2nd Prize=3 and 1st Prize=4. For visual clarity the crude VA percentiles are in bins of width 2 and on the y-axis the bin-average teaching prize values are plotted. The cutoffs are $x = 20$ (from the Excellence to the 3rd Prize), $x = 60$ (from the 3rd to the 2nd) and $x = 85$ (from the 2nd to the 1st). If the stated teaching award assignment rule is followed strictly, we shall expect the effect of surpassing a certain crude VA cutoff on the probability of receiving a higher-level teaching award to be one. A regression discontinuity estimation stacking all 3 cutoffs and including quadratic running variable controls yields an estimate of this effect at 0.913 (SE=0.0032).

Figure A6: The Relationship between Cohort-Average Value-Added and Teaching Awards



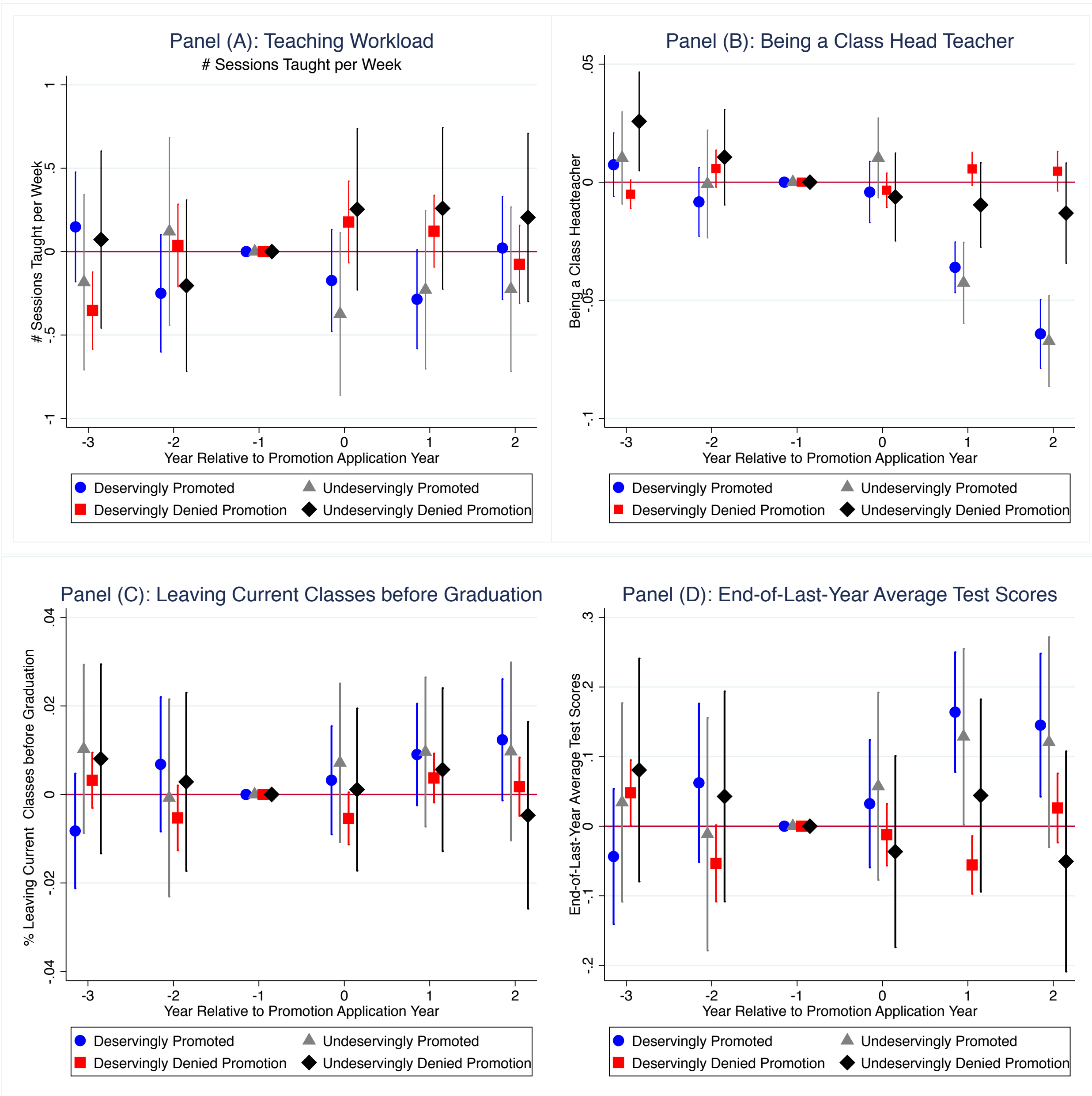
Notes: This graph repeats the exercise in Figure A5 where the x-axis variable is the average of the teachers' VA (estimated by the author) in the 3 years when they teach the CEE-taking cohort. An analogous regression discontinuity estimation to the one described in Figure A5 shows that the effect of surpassing the percentile cutoffs in the 3-year average VA on the probability of receiving a higher level teaching award is estimated to be 0.618 (SE=0.0035).

Figure A7: Distributions of Applicants' Value-Added by Social Ties Status



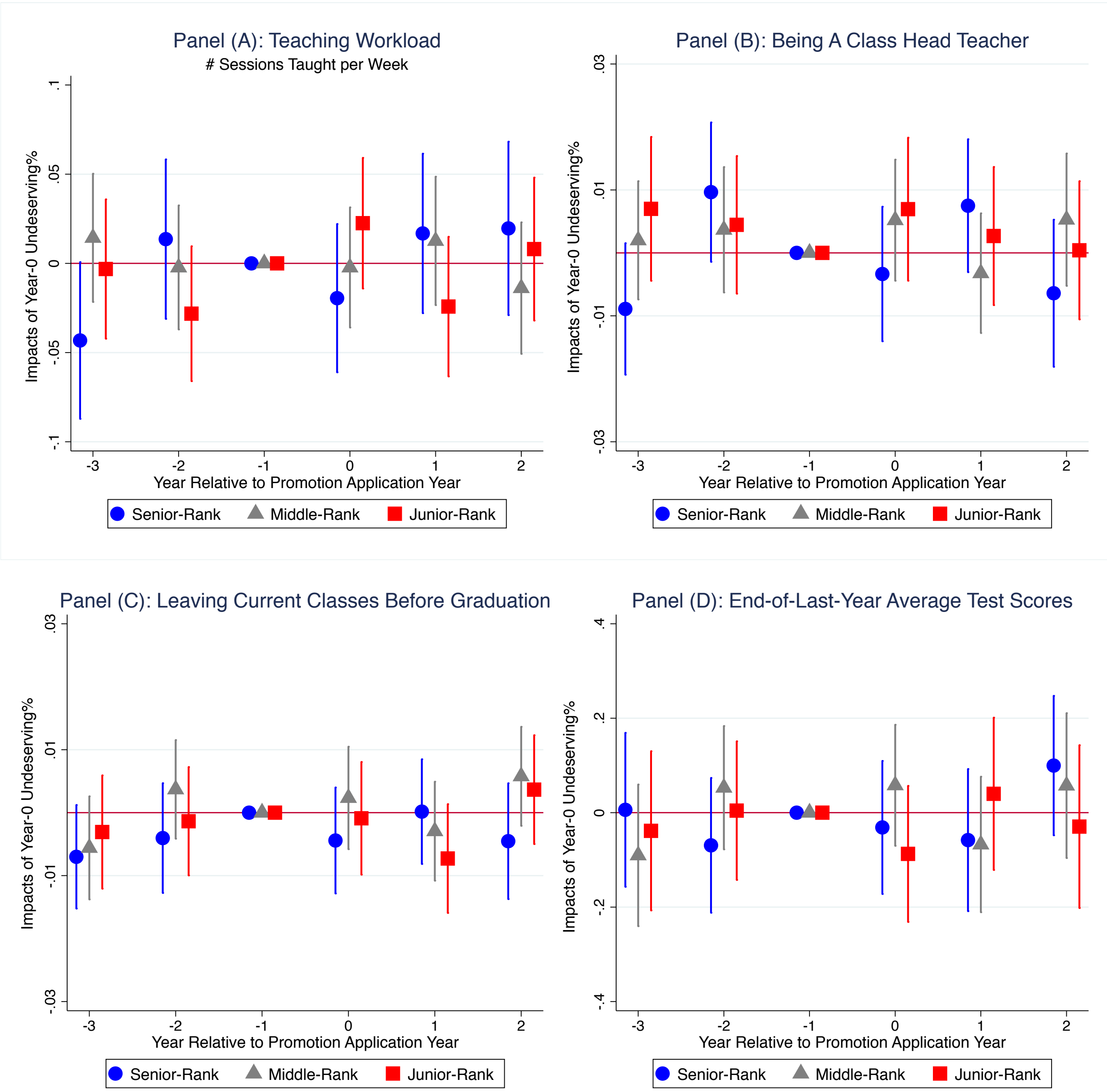
Notes: This graph plots the teacher-school-specific VA of applicants who are either socially tied or untied to their principals. The unit of analysis is the applicant-year. N=59,121. SocialTie=1 if an applicant is either the principal's hometown or college fellow. Kolmogorov-Smirnov test for the equality of distribution functions yields D-val=0.0492 (p-val=0.000).

Figure A8: Event Studies of Individual Promotion Results: Applicants' Job Characteristics



Notes: This graph plots event studies of the applicants' job characteristics variables ($\mathbf{Z}$ ={teaching workload, being a class head teacher, irregular change of classes, beginning-of-year score of students}) before and after the application year of applicant type $m \in \mathbb{M}$ ={Undeservingly Promoted, Deservingly Promoted, Deservingly Denied, Undeservingly Denied}. The estimated coefficients on the relative year dummies ($\{\hat{\varphi}_{m\tau}\}_{\tau=-2}^{3}$) from the regressions of Equation (1.18), as well as the 95% confidence intervals, are plotted. The estimated coefficients along with their associated standard errors clustered at the applicant level are reported in Table A17. Only the applicant-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. School-specific time trends, applicant-(year 0)-principal fixed effects, applicant-(current)-principal fixed effects are controlled for.

Figure A9: Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Job Characteristics: Professional Ranks



Notes: This graph shows the impacts of current perceived promotion unfairness (Undeserving%) on the current, future and lagged job characteristics of the non-applicant teachers of different professional ranks in a school. The estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20), as well as the 95% confidence intervals are plotted. These coefficients along with their associated standard errors clustered at the teacher level are reported in Table A18. Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$), are included. Lagged outcome variables, school-specific time trends, teacher-(year 0) principal fixed effects and teacher-current-principal fixed effects are controlled for. $\theta_{-1} = 0$ by construction.

A2 Table Appendix

Table A1: Effect of Social Ties with Principal on Promotion Rates (Expanded) (I)

	Outcome Variable: Promoted Pooling Hometown and College Ties					
	(1)	(2)	(3)	(4)	(5)	(6)
Tie	0.2084***	0.2112***	0.2241**	0.2225***	0.2062***	0.2063***
	(0.0088)	(0.0087)	(0.0043)	(0.0044)	(0.0059)	(0.0058)
VA in past 6 yrs		0.0166***		0.0149***		0.0038
(Standardized)		(0.0034)		(0.0027)		(0.0037)
Share of same-subject applicants	-0.110*	-0.109*	-0.118***	-0.118***	-0.089*	-0.097*
	(0.052)	(0.052)	(0.0047)	(0.0048)	(0.0054)	(0.0054)
	Demographics					
Age/10	0.0142*	0.0139	0.0146**	0.0145**	0.0134*	0.0129*
	(0.0079)	(0.0080)	(0.0058)	(0.0058)	(0.0075)	(0.0074)
Male	0.0076	0.0076	0.0077*	0.0076*		
	(0.0053)	(0.0054)	(0.0044)	(0.0044)		
Ethnic Minority	0.0981	0.0977	0.0103**	0.0106**		
	(0.0064)	(0.0065)	(0.0056)	(0.0055)		
CPC Member	0.0144***	0.0142***	0.0148***	0.0146***		
	(0.0057)	(0.0057)	(0.0044)	(0.0043)		
College FE	Y	Y	Y	Y	Y	Y
City of Birth FE	Y	Y	Y	Y	Y	Y
Subject FE	Y	Y	Y	Y	Y	Y
	Experience					
Experience (in yrs/10)	0.0168***	0.0164***	0.0173***	0.0168***	0.0156***	0.0158***
	(0.0079)	(0.0079)	(0.0063)	(0.0064)	(0.0070)	(0.0070)
# Years in Middle Rank/10	0.0160***	0.0157***	0.0175***	0.0180***	0.0155***	0.0158***
	(0.0072)	(0.0071)	(0.0054)	(0.0054)	(0.0062)	(0.0061)
# Years in Current School/10	0.0132*	0.0137*	0.0152**	0.0153**	0.0164**	0.0166**
	(0.0082)	(0.0081)	(0.0055)	(0.0056)	(0.0076)	(0.0077)
	Workload					
# Years as Class Head Teacher	0.0033	0.0028	0.0026*	0.0026**	0.0029*	0.0027*
(in Past 6 Yrs)	(0.0017)	(0.0017)	(0.0013)	(0.0012)	(0.0018)	(0.0018)
Ave. # Classes Taught per Week	0.0024	0.0025	0.0022	0.0023	0.0016	0.0017
(in Past 6 Yrs)	(0.0021)	(0.0021)	(0.0017)	(0.0016)	(0.0022)	(0.0022)
	Research					
# National Publications	0.0434***	0.0427***	0.0442***	0.0439***	0.0315***	0.0314***
(in Past 6 Yrs)	(0.0057)	(0.0057)	(0.0046)	(0.0046)	(0.0078)	(0.0078)
# Provincial Publications	0.0217***	0.0216***	0.0240***	0.0236***	0.0142**	0.0144**
(in Past 6 Yrs)	(0.0052)	(0.0052)	(0.0047)	(0.0049)	(0.0069)	(0.0070)
	Teaching					
Teaching awards combination FE	Y	Y	Y	Y	Y	Y
Additional Teaching Award Being	0.0009	-0.0032	0.0029	0.0041	-0.0033	-0.0024
Excellence Prize	(0.0119)	(0.0124)	(0.0126)	(0.0126)	(0.0167)	(0.0166)
Additional Teaching Award Being 3rd Prize	0.0084	0.0042	0.0068	0.0059	0.0132	0.0140
	(0.0108)	(0.0109)	(0.00807)	(0.00813)	(0.0175)	(0.0177)
Additional Teaching Award Being 2nd Prize	0.0199*	0.0202*	0.0224**	0.0229**	0.0184	0.0193
	(0.0136)	(0.0137)	(0.0089)	(0.0089)	(0.0180)	(0.0178)
Additional Teaching Award Being 1st Prize	0.0397**	0.0392**	0.0371***	0.0364***	0.0419**	0.0408*
	(0.0169)	(0.0169)	(0.0130)	(0.0131)	(0.0197)	(0.0198)
	Other					
# Other Awards (Provincial or Above)	0.0155***	0.0157***	0.0179***	0.0179***	0.0140*	0.0141*
	(0.00548)	(0.0052)	(0.0479)	(0.0478)	(0.0674)	(0.0678)
# Other Awards (City or Below)	0.0092*	0.0096**	0.0092***	0.0089***	0.0101*	0.0092*
	(0.0041)	(0.0042)	(0.0036)	(0.0036)	(0.058)	(0.058)
School-year FE	Y	Y	Y	Y	Y	Y
Individual FE	N	N	N	N	Y	Y
Model	Logit	Logit	Linear	Linear	Linear	Linear
Mean Dep. Var	0.217	0.217	0.217	0.217	0.193	0.193
# Obs	57,613	57,613	57,613	57,613	42,283	42,283
(pseudo) R^2	0.721	0.723	0.705	0.710	0.832	0.834

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table is the expanded version of Panel (A) of Table 1.7. This table presents the estimated average effect of an applicant's social ties to the principal on her promotion probability (estimation results of Equation (1.4)). Teaching award combination fixed effects, city-of-birth fixed effects, college fixed effects, school-year fixed effects and share of same-subject applicants are included in all specifications. Columns (2)(4)(6) control for the teacher-school-specific value-added of the applicants (normalized to have unit standard deviation). Columns (5) and (6) include applicant fixed effects. Coefficients are in terms of average marginal effects in the logit models (columns (1) & (2)).

Table A2: Effect of Social Ties with Principal on Promotion Rates (Expanded) (II)

	Outcome Variable: Promoted					
	Seperating Hometown and College Ties					
	(1)	(2)	(3)	(4)	(5)	(6)
HomeTie	0.1757***	0.1770***	0.1940***	0.1918***	0.1758***	0.1760***
	(0.0082)	(0.0081)	(0.0053)	(0.0052)	(0.0072)	(0.0071)
CollegeTie	0.135***	0.1376***	0.1390***	0.1383***	0.1316***	0.1309***
	(0.0082)	(0.0083)	(0.0058)	(0.0057)	(0.0087)	(0.0087)
VA in past 6 yrs		0.0183***		0.0164***		0.0024
(Standardized)		(0.0032)		(0.0026)		(0.0038)
Share of same-subject applicants	-0.102*	-0.103*	-0.120***	-0.116***	-0.099*	-0.102*
	(0.053)	(0.052)	(0.0046)	(0.0047)	(0.0053)	(0.0053)
	Demographics					
Age/10	0.0143	0.0141	0.0148**	0.0151**	0.0127*	0.0137*
	(0.0083)	(0.0082)	(0.0058)	(0.0058)	(0.0074)	(0.0075)
Male	0.0074	0.0073	0.0082*	0.0079*		
	(0.0053)	(0.0054)	(0.0044)	(0.0044)		
Ethnic Minority	0.0108*	0.0110*	0.0111**	0.0112**		
	(0.0066)	(0.0067)	(0.0053)	(0.0052)		
CPC Member	0.0154***	0.0146***	0.0151***	0.0152***		
	(0.0054)	(0.0054)	(0.0045)	(0.0045)		
College FE	Y	Y	Y	Y	Y	Y
City of Birth FE	Y	Y	Y	Y	Y	Y
Subject FE	Y	Y	Y	Y	Y	Y
	Experience					
Experience (in yrs/10)	0.0178***	0.0181***	0.0153***	0.0153***	0.0174***	0.0174***
	(0.0078)	(0.0077)	(0.0063)	(0.0063)	(0.0071)	(0.0072)
# Years in Middle Rank/10	0.0153***	0.0156***	0.0178***	0.0181***	0.0201***	0.0199***
	(0.0067)	(0.0067)	(0.0055)	(0.0055)	(0.0063)	(0.0064)
# Years in Current School/10	0.0142*	0.0145*	0.0152**	0.0153**	0.0162**	0.0161**
	(0.0082)	(0.0081)	(0.0065)	(0.0066)	(0.0078)	(0.0078)
	Workload					
# Years as Class Head Teacher	0.0024	0.0025	0.0025*	0.0026**	0.0032*	0.0032*
(in Past 6 Yrs)	(0.0017)	(0.0016)	(0.0013)	(0.0013)	(0.0019)	(0.0019)
Ave. # Classes Taught per Week	0.0014	0.0012	0.0010	0.0015	0.0013	0.0012
(in Past 6 Yrs)	(0.0022)	(0.0023)	(0.0016)	(0.0016)	(0.0021)	(0.0020)
	Research					
# National Publications	0.0441***	0.0437***	0.0428***	0.0430***	0.0293***	0.0294***
(in Past 6 Yrs)	(0.0056)	(0.0055)	(0.0045)	(0.0044)	(0.0077)	(0.0078)
# Provincial Publications	0.0214***	0.0213***	0.0229***	0.0231***	0.0161**	0.0157**
(in Past 6 Yrs)	(0.0049)	(0.0049)	(0.0041)	(0.0040)	(0.0067)	(0.0066)
	Teaching					
Teaching awards combination FE	Y	Y	Y	Y	Y	Y
Additional Teaching Award Being	-0.0062	0.0054	0.0057	0.0023	0.0080	-0.0017
Excellence Prize	(0.0129)	(0.0131)	(0.0118)	(0.0118)	(0.0164)	(0.0162)
Additional Teaching Award Being 3rd Prize	0.0083	0.010	0.0036	0.0021	0.0072	0.0076
	(0.0110)	(0.0112)	(0.00832)	(0.00840)	(0.0181)	(0.0182)
Additional Teaching Award Being 2nd Prize	0.0279**	0.0280**	0.0231**	0.0232**	0.0171	0.0190
	(0.0130)	(0.0130)	(0.0092)	(0.0092)	(0.0177)	(0.0180)
Additional Teaching Award Being 1st Prize	0.0389**	0.0401**	0.0362***	0.0354***	0.0321	0.0351*
	(0.0172)	(0.0172)	(0.0125)	(0.0130)	(0.0202)	(0.0201)
	Other					
# Other Awards (Provincial or Above)	0.0158***	0.0162***	0.0174***	0.0180***	0.0131*	0.0143*
	(0.00544)	(0.00550)	(0.0481)	(0.0478)	(0.0691)	(0.0685)
# Other Awards (City or Below)	0.0083*	0.0087**	0.010***	0.0097***	0.0081	0.0078
	(0.0042)	(0.0043)	(0.0038)	(0.0038)	(0.056)	(0.057)
School-year FE	Y	Y	Y	Y	Y	Y
Individual FE	N	N	N	N	Y	Y
Model	Logit	Logit	Linear	Linear	Linear	Linear
Mean Dep. Var	0.217	0.217	0.217	0.217	0.213	0.213
# Obs	57,613	57,613	57,613	57,613	42,283	42,283
(pseudo) R^2	0.734	0.738	0.713	0.716	0.844	0.848

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the applicant level are reported in parentheses. This table is the expanded version of Panel (B) of Table 1.7. See more detailed notes under Table A1. The estimated coefficients on the teaching award combination dummies in the column (1) specification are shown in Figure 1.2.

Table A3: Application Profiles of Socially Tied and Untied Applicants

	Tie=0		Tie=1		Difference	
	Mean	SD	Mean	SD	Diff	p-value
Age	41.5	(3.92)	40.8	(3.63)	0.29	0.071
Male	0.388	(0.236)	0.378	(0.237)	0.01	0.024
Ethnic Minority	0.244	(0.184)	0.245	(0.191)	-0.001	0.120
CPC Member	0.360	(0.237)	0.349	(0.234)	0.011	0.027
Experience (in yrs)	17.9	(3.88)	17.1	(3.69)	0.8	0.009
# Years in Middle Rank	11.3	(4.12)	10.2	(4.33)	1.1	0.007
# Years in Current School	13.6	(3.44)	12.3	(3.49)	1.3	0.005
# Years as Class Head Teacher (Past 6 Yrs)	4.91	(2.14)	4.92	(2.13)	-0.01	0.231
Ave. # Classes Taught per Week (Past 6 Yrs)	12.16	(1.289)	12.32	(1.302)	0.16	0.004
# National Publications	0.371	(0.512)	0.348	(0.507)	0.023	0.004
# Provincial Publications	2.28	(0.881)	2.10	(0.902)	0.18	0.003
# 1st Prize Teaching Awards	0.278	(0.618)	0.258	(0.626)	0.020	0.001
# 2nd Prize Teaching Awards	0.598	(0.719)	0.576	(0.725)	0.022	0.001
# 3rd Prize Teaching Awards	0.691	(0.759)	0.725	(0.743)	-0.034	0.001
# Excellence Prize Teaching Awards	0.352	(0.608)	0.360	(0.613)	-0.008	0.001
# Other Awards (Provincial or Above)	1.258	(0.515)	1.241	(0.521)	0.017	0.001
# Other Awards (City or Below)	2.672	(0.926)	2.674	(0.937)	-0.002	0.001
Ave. VA in past 6 Yrs	0.234	(0.801)	0.143	(0.822)	0.091	0.001
Obs.	40,734		18,367			

Notes: The unit of analysis is the teacher-year. Only the years for which the application profiles dataset is available are included.

Table A4: Event Studies of Principal Entries: Promotion Probabilities of Differently-Tied Applicants

	Outcome Variable: Promoted ($\text{Promoted}_{j,t+s}$)							
Year relative to Promotion Year	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Panel (A): Principal Hometown Change								
Untied to Tied	-0.021	0.017	-0.028	0.142***	0.150***	0.170***	0.188***	6,804
	(0.042)	(0.037)	(0.034)	(0.032)	(0.034)	(0.040)	(0.038)	
Tied to Untied	0.158***	0.193***	0.176***	0.014	-0.034	0.019	-0.046	7,114
	(0.042)	(0.037)	(0.038)	(0.035)	(0.040)	(0.037)	(0.041)	
Never Tied	0.033	-0.017	0	0.029	0.015	-0.030	0.011	24,513
	(0.025)	(0.027)	-	(0.022)	(0.023)	(0.027)	(0.027)	
Panel (B): Principal College Change								
Untied to Tied	0.022	-0.014	-0.021	0.114***	0.141***	0.133***	0.156***	6,792
	(0.048)	(0.045)	(0.037)	(0.040)	(0.041)	(0.044)	(0.046)	
Tied to Untied	0.142***	0.127***	0.133***	-0.038	-0.020	-0.038	-0.041	6,625
	(0.050)	(0.044)	(0.045)	(0.042)	(0.041)	(0.044)	(0.048)	
Never Tied	-0.028	0.015	0	-0.011	0.014	0.019	-0.007	29,034
	(0.024)	(0.026)	-	(0.022)	(0.021)	(0.025)	(0.028)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the applicant level are reported in parentheses. This table reports the values plotted in Figure 1.3. These are estimated coefficient estimates on the relative year dummies ($\{\hat{\mu}_{q\tau}\}_{\tau=-3}^{3}$) from the regressions of Equation (1.6). Controls of applicant characteristics $\{\mathbf{X}_g\}_{g\in\mathbb{G}}$ ($\mathbb{G}$ ={demographics, experience, workload, research, teaching, other}, see Table 1.2 for a detailed description of these variables), the share of same-subject applicants, and school-year fixed effects are included.

Table A5: Event Studies of Individual Promotion Results: Applicants' Value-Added and Job-Quitting Probability

Year relative to Promotion Year	$t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Panel (A): Outcome Variable: Value-Added ($\text{VA}_{i,t+s}$)						
Deservingly Promoted	0.097***	0.126***	-0.163***	-0.146***	-0.108***	51,529
	(0.033)	(0.029)	(0.029)	(0.033)	(0.035)	
Undeservingly Promoted	0.028	0.050	-0.043	0.072	0.054	14,382
	(0.058)	(0.053)	(0.051)	(0.058)	(0.062)	
Deservingly Denied	0.037*	-0.047**	0.254***	0.201***	0.164***	218,372
	(0.023)	(0.021)	(0.023)	(0.024)	(0.026)	
Undeservingly Denied	-0.052	0.093*	0.036	-0.029	0.024	13,548
	(0.054)	(0.0505)	(0.050)	(0.054)	(0.0560)	
Panel (B): Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)						
Deservingly Promoted			0.0324***	0.0312***	0.0349***	51,529
			(0.0024)	(0.0025)	(0.0025)	
Undeservingly Promoted			0.0271 ***	0.0274***	0.0325***	14,382
			(0.0046)	(0.0047)	(0.0049)	
Deservingly Denied			0.0476***	0.0433***	0.0438***	218,372
			(0.0011)	(0.0012)	(0.0012)	
Undeservingly Denied			0.0636***	0.0547***	0.0492***	218,372
			(0.0047)	(0.0047)	(0.0048)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.4 and 1.5. These are estimated coefficients on the relative year dummies ($\{\hat{\varphi}_{m\tau}\}_{\tau=-3}^{2}$) from the regressions of Equation (1.18). Only the teacher-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Applicants' job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0)-principal fixed effects, applicant-(current)-principal fixed effects are controlled for.

Table A6: Impacts of Perceived Promotion Unfairness on Applicants' Value-Added and Job-Quitting Probability

Year relative to Promotion Year	$t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Panel (A): Outcome Variable: Value-Added ($\text{VA}_{i,t+s}$)						
	-0.035	0.014	-0.022	-0.003	0.005	297,831
	(0.053)	(0.048)	(0.044)	(0.049)	(0.053)	
Panel (B): Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)						
			0.0024	0.0017	0.0007	297,831
			(0.0029)	(0.0034)	(0.0038)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.8 and 1.9. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_{\tau}\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20) Only the applicant-year observations where the applicant works in the same school as the application year ($h(j, t+s) = h(j,t)$), and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A7: Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Value-Added and Job-Quitting Probability

Year relative to Promotion Year	$t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Panel (A): Outcome Variable: Value-Added ($\text{VA}_{i,t+s}$)						
	0.046	-0.074	-0.938***	-0.451***	-0.245***	435,998
	(0.044)	(0.040)	(0.036)	(0.040)	(0.044)	
Panel (B): Outcome Variable: Value-Added ($\text{VA}_{i,t+s}$) Subsample: Teaching the Same Classes as in Year $t-1$						
	-0.018	-0.089	-1.005***	-0.490***		241,118
	(0.061)	(0.055)	(0.051)	(0.056)		
Panel (C): Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)						
			0.0456***	0.0293***	0.0189**	435,998
			(0.0024)	(0.0028)	(0.0032)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.8 and 1.9. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20) Only the teacher-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0) principal fixed effects, applicant-current-principal fixed effects are controlled for.

Table A8: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants: Principal's Presence

Year relative to Promotion Year	$t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Panel (A): Outcome Variable: Teachers' VA ($\text{VA}_{i,t+s}$)						
Same Principal as in Year t	-0.083*	-0.038	-1.031***	-0.512***	-0.289***	364,930
	(0.048)	(0.044)	(0.040)	(0.044)	(0.048)	
Different Principal from Year t	0.040	-0.081		-0.385***	-0.164	71,068
	(0.108)	(0.099)		(0.099)	(0.108)	
Panel (B): Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)						
Same Principal as in Year t			0.0521***	0.0334***	0.0221***	364,930
			(0.0027)	(0.0030)	(0.0035)	
Different Principal from Year t				0.0082	0.0023	71,068
				(0.0069)	(0.0078)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.10 and 1.11. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i, t+s) = h(i,t)$), are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0) principal fixed effects, applicant-current-principal fixed effects are controlled for.

Table A9: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants: Professional Ranks

Year relative to Promotion Year	$t-3$	$t-2$	t	$t+1$	$t+2$	Obs.
	(1)	(2)	(3)	(4)	(5)	
Panel (A): Outcome Variable: Teachers' VA ($\text{VA}_{i,t+s}$)						
Senior-Ranked	0.112	-0.092	-1.820***	-0.847***	-0.436***	139,743
	(0.083)	(0.077)	(0.070)	(0.078)	(0.085)	
Junior-Ranked	-0.118	0.032	-1.180***	-0.578***	-0.313***	122,974
	(0.087)	(0.079)	(0.068)	(0.080)	(0.078)	
Middle-Ranked	0.087	-0.112	-0.593***	-0.306***	-0.169**	173,281
	(0.080)	(0.077)	(0.065)	(0.078)	(0.082)	
Panel (B): Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)						
Senior-Ranked			0.0257***	0.0162***	0.0104**	139,743
			(0.0043)	(0.0049)	(0.0056)	
Junior-Ranked			0.0483***	0.0311***	0.0202***	122,974
			(0.0038)	(0.0044)	(0.0050)	
Middle-Ranked			0.0598***	0.0348***	0.0256***	173,281
			(0.0046)	(0.0052)	(0.0059)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.12 and 1.13. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$), are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A10: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants: Social Ties w/ Principal

Year relative to Promotion Year	$t-3$	$t-2$	t	$t+1$	$t+2$	Obs.
	(1)	(2)	(3)	(4)	(5)	
Panel (A): Outcome Variable: Teachers' VA ($VA_{i,t+s}$)						
Hometown/College Tied w/ Principal	0.026	0.021	-0.818***	-0.386***	-0.217***	135,159
	(0.079)	(0.074)	(0.068)	(0.073)	(0.083)	
Not Hometown/College Tied w/ Principal	-0.021	0.008	-1.028***	-0.481***	-0.245***	300,839
	(0.054)	(0.050)	(0.046)	(0.050)	(0.056)	
Panel (B): Outcome Variable: Teachers' VA ($VA_{i,t+s}$) Subsample: Middle-Ranked Non-Applicant Teachers						
Hometown/College Tied w/ Principal	0.048	-0.061	-0.528***	-0.276**	-0.147	53,717
	(0.125)	(0.117)	(0.108)	(0.116)	(0.132)	
Not Hometown/College Tied w/ Principal	0.105	-0.135*	-0.622***	-0.319***	-0.179**	119,564
	(0.086)	(0.079)	(0.073)	(0.079)	(0.088)	
Panel (C): Outcome Variable: Outcome Variable: Job Quitting before Retirement ($Leave_{i,t+s}$)						
Hometown/College Tied w/ Principal			0.0376***	0.0268***	0.0172***	135,159
			(0.0044)	(0.0050)	(0.0057)	
Not Hometown/College Tied w/ Principal			0.0492***	0.0304***	0.0196***	300,839
			(0.0029)	(0.0033)	(0.0038)	
Panel (D): Outcome Variable: Job Quitting before Retirement ($Leave_{i,t+s}$) Subsample: Middle-Ranked Non-Applicant Teachers						
Hometown/College Tied w/ Principal			0.0284***	0.0168*	0.0142	53,717
			(0.0077)	(0.0088)	(0.0100)	
Not Hometown/College Tied w/ Principal			0.0740***	0.0429***	0.0308***	119,564
			(0.0052)	(0.0059)	(0.0067)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figures 1.14 and 1.15. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$), are included. Lagged VA ($VA_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A11: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Social Interactions with Victims

Year relative to Promotion Year	Outcome Variable: Teachers' VA ($\text{VA}_{i,t+s}$) $t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Panel (A): Teaching the Same Cohort as Victims						
Same Cohort	0.008	0.017	-1.129***	-0.557***	-0.282***	248,519
	(0.058)	(0.054)	(0.051)	(0.054)	(0.062)	
Different Cohort	-0.004	-0.065	-0.739***	-0.326***	-0.174***	187,479
	(0.066)	(0.061)	(0.056)	(0.061)	(0.069)	
Panel (B): Teaching the Same Subject as Victims						
Same Subject	0.064	0.015	-1.335***	-0.705***	-0.377***	95,920
	(0.091)	(0.084)	(0.076)	(0.084)	(0.095)	
Different Subject	0.041	-0.099**	-0.824***	-0.382***	-0.208***	340,078
	(0.051)	(0.047)	(0.043)	(0.046)	(0.053)	
Panel (C): Socially Tied w/ Victims						
Hometown/College Tied	0.007	0.055	-1.167***	-0.632***	-0.285***	265,959
	(0.058)	(0.053)	(0.048)	(0.052)	(0.060)	
Not Hometwon/College Tied	-0.005	-0.144**	-0.699***	-0.315***	-0.166***	170,039
	(0.071)	(0.066)	(0.060)	(0.066)	(0.075)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figure 1.16. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$), are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, applicant-(year 0) principal fixed effects, applicant-current-principal fixed effects are controlled for. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A12: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' VA: Past Promotion Experience

Year relative to Promotion Year	Outcome Variable: Teachers' VA ($\text{VA}_{i,t+s}$) $t-3$ (1)	$t-2$ (2)	t (3)	$t+1$ (4)	$t+2$ (5)	Obs.
Self-Perceived Undeservingly	-0.013	0.042	-1.060***	-0.520***	-0.341**	24,595
Promoted in the Past	(0.153)	(0.139)	(0.129)	(0.141)	(0.152)	
Self-Perceived Deservingly	0.131	-0.052	-2.140***	-1.020***	-0.487***	85,020
Promoted in the Past	(0.102)	(0.098)	(0.087)	(0.093)	(0.102)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figure 1.17. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher has been promoted to the senior rank before the reference application year (year 0) and works in the same school as in the reference application year, are included. Lagged VA ($\text{VA}_{i,t-1}$), job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A13: Heterogeneous Spillover Impacts of Perceived Promotion Unfairness on Non-Applicants' Quitting Probability: Teacher Quality

Outcome Variable: Job Quitting before Retirement ($\text{Leave}_{i,t+s}$)				
Year relative to Promotion Year$t$	$t+1$	$t+2$	Obs.	
	(1)	(2)	(3)	
Above-Median Teaching Quality (VA)	0.0931***	0.0612***	0.0436***	61,606
	(0.0064)	(0.0071)	(0.0076)	
Below-Median Teaching Quality (VA)	0.0266***	0.0084	0.0077	61,368
	(0.0064)	(0.0071)	(0.0076)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at applicant level are reported in parentheses. This table reports the values plotted in Figure 1.18. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regression of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0), works in the same school as in the reference application year ($h(i,t+s) = h(i,t)$) and is currently middle-ranked, are included. Job characteristics with individual-specific coefficients, school-specific time trends, teacher-(year 0)-principal fixed effects, teacher-(current)-principal fixed effects are controlled for.

Table A14: Impacts of Perceived Promotion Unfairness on Teacher Turnover

Year relative to Promotion Year	$t-3$	$t-2$	t	$t+1$	$t+2$
	(1)	(2)	(3)	(4)	(5)
Panel (A): Outcome Variable: Number of Teachers					
Retired	0.310*	0.143	0.193	-0.141	0.074
	(0.164)	(0.184)	(0.151)	(0.197)	(0.184)
Quitters	-0.210	0.404	2.210***	1.560***	0.890***
	(0.272)	(0.263)	(0.227)	(0.227)	(0.265)
New Hires	-0.066	-0.040	2.040***	1.680***	0.790***
	(0.287)	(0.271)	(0.269)	(0.279)	(0.289)
Panel (B): Outcome Variable: Average Individual-School-Specifc VA					
Retired	0.105	-0.104	-0.063	0.099	0.072
	(0.148)	(0.148)	(0.132)	(0.139)	(0.142)
Quitters	-0.102	0.187	0.560***	0.420***	0.334**
	(0.148)	(0.146)	(0.143)	(0.147)	(0.151)
New Hires	-0.176	0.123	-0.284**	-0.220	-0.171
	(0.134)	(0.124)	(0.139)	(0.137)	(0.139)
Panel (C): Outcome Variable: School-Level Change in Total Teacher VA					
	-1.05	0.58	-3.71***	-2.22***	-1.59**
	0.66	0.63	(0.66)	(0.66)	(0.67)

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the school level are reported in parentheses. The unit of analysis is the school-year. N=3,642. This table reports the values plotted in Figure 1.19. These are estimated coefficients on current Undeserving% (interacted with relative year dummies) from the regressions of Equation (1.21). Lagged outcome variable, school-specific time trends and principal-school fixed effects are controlled for.

Table A15: Impacts of Perceived Promotion Unfairness on Students' Test Scores

Year Relative to Reform Start Year	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Panel (A): Outcome Variable: Crude Class-Subject Avg. VA of All In-School Cohorts ($A_{c,k,t+s} - A_{c,k,t+s-1}$)								
	0.076*	-0.048	0.029	-0.576***	-0.373***	-0.205***		657,512
	(0.041)	(0.043)	(0.042)	(0.037)	(0.035)	(0.045)		
Panel (B): Outcome Variable: Class-Subject Avg. CEE Scores of Graduating Cohort ($A^{\text{CEE}}_{c,k,t+s}$, $g\,(c, t+s) = 3$)								
	0.057	-0.051	0.066	-0.635***	-0.951***	-1.147***		218,147
	(0.071)	(0.075)	(0.072)	(0.063)	(0.061)	(0.078)		
Panel (C): Outcome Variable: Class-Subject Avg. HEE Scores of Newly Enrolled Cohort ($A^{\text{HEE}}_{c,k,t+s}$, $g\,(c, t+s) = 1$)								
	-0.077	0.078	-0.054	-0.059	-0.420***	-0.471***	-0.566***	221,628
	(0.064)	(0.066)	(0.060)	(0.060)	(0.069)	(0.082)	(0.097)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the school level are reported in parentheses. The unit of analysis is the class-subject-year. This table reports the values plotted in Figures 1.20 and 1.21. These are coefficients from the regressions of Equation (1.22) on current Undeserving% (interacted with relative year dummies). School specific time trends and principal-school fixed effects are controlled for.

Table A16: Event Studies of Transparency Reform on School Performance

Year Relative to Reform Start Year						
$t-3$	$t-2$	t	$t+1$	$t+2$	$t+3$	Obs.
(1)	(2)	(3)	(4)	(5)	(6)	
Panel (A): Perceived Promotion Unfairness (Undeserving$\%_{it}$)						
-0.034	0.065	-0.210***	-0.247***	-0.213***	-0.236***	124,082
(0.048)	(0.057)	(0.060)	(0.065)	(0.074)	(0.091)	
Panel (B): College Entrance Exams Scores (A_{ckt}^{CEE}, $g(c,t)=3$)						
-0.027	0.032	0.101*	0.146***	0.190***	0.177***	63,178
(0.053)	(0.053)	(0.052)	(0.053)	(0.058)	(0.063)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors are reported in parentheses.

Panel (A) reports the values plotted in Figure 1.22. These are coefficients from the regression of Equation (1.26) on *Post*. Standard errors are clustered at the teacher level. The unit of analysis is the teacher-year.

Panel (B) reports the values plotted in Figure 1.23. These are coefficients from the regression of Equation (1.28) on *Post*. Standard errors are clustered at the school level. The unit of analysis is the class-subject-year.

School specific time trends and principal-school fixed effects are controlled for.

Table A17: Event Studies of Promotion Results: Applicants' Job Characteristics

	Year relative to Promotion Year					
	$t-3$	$t-2$	t	$t+1$	$t+2$	Obs.
	(1)	(2)	(3)	(4)	(5)	
Panel (A): Outcome Variable: Teaching Workload # Sessions Taught Per Week						
Deservingly Promoted	0.148	-0.250	-0.173	-0.286	0.022	51,529
	(0.168)	(0.180)	(0.156)	(0.152)	(0.157)	
Undeservingly Promoted	-0.184	0.121	-0.374	-0.229	-0.225	14,382
	(0.268)	(0.287)	(0.249)	(0.242)	(0.251)	
Deservingly Denied	-0.353***	0.037	0.178	0.122	-0.076	218,372
	(0.118)	(0.126)	(0.125)	(0.110)	(0.119)	
Undeservingly Denied	0.072	-0.204	0.254	0.259	0.205	13,548
	(0.271)	(0.262)	(0.247)	(0.247)	(0.257)	
Panel (B): Outcome Variable: Being a Class Head Teacher						
Deservingly Promoted	0.0074	-0.0083	-0.0042	-0.0360***	-0.0642***	51,529
	(0.0069)	(0.0074)	(0.0066)	(0.0055)	(0.0074)	
Undeservingly Promoted	0.0103	-0.0008	0.0103	-0.0426***	-0.0673***	14,382
	(0.0100)	(0.0116)	(0.0086)	(0.0088)	(0.0098)	
Deservingly Denied	-0.0051*	0.0057	-0.0034	0.0056**	0.0047	218,372
	(0.0031)	(0.0040)	(0.0029)	(0.0026)	(0.0034)	
Undeservingly Denied	0.0258**	0.0106	-0.0063	-0.0096	-0.0131	13,548
	(0.0107)	(0.0103)	(0.0095)	(0.0091)	(0.0108)	
Panel (C): Outcome Variable: Leaving Current Classes Before Graduation						
Deservingly Promoted	-0.0082	0.0068	0.0032	0.0090	0.0123*	51,529
	(0.0066)	(0.0077)	(0.0062)	(0.0059)	(0.0070)	
Undeservingly Promoted	0.0103	-0.0008	0.0072	0.0096	0.0097	14,382
	(0.0097)	(0.0114)	(0.0092)	(0.0086)	(0.0103)	
Deservingly Denied	0.0032	-0.0053	-0.0054*	0.0037	0.0017	218,372
	(0.0032)	(0.0037)	(0.0030)	(0.0028)	(0.0034)	
Undeservingly Denied	0.0080	0.0028	0.0011	0.0056	-0.0047	13,548
	(0.0109)	(0.0103)	(0.0094)	(0.0094)	(0.0108)	
Panel (D): Outcome Variable: End-of-Last-Year Average Test Scores						
Deservingly Promoted	-0.044	0.062	0.032	0.164	0.145	51,529
	(0.050)	(0.058)	(0.047)	(0.044)	(0.052)	
Undeservingly Promoted	0.034	-0.012	0.057	0.129	0.121	14,382
	(0.073)	(0.085)	(0.069)	(0.064)	(0.077)	
Deservingly Denied	0.048	-0.053	-0.012	-0.056	0.026	218,372
	(0.024)	(0.028)	(0.023)	(0.021)	(0.025)	
Undeservingly Denied	0.081	0.043	-0.037	0.044	-0.051	13,548
	(0.082)	(0.077)	(0.070)	(0.070)	(0.081)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at the applicant level are reported in parentheses. This table reports the values plotted in Figure A8. These are estimated coefficients on the relative year dummies ($\{\hat{\varphi}_{m\tau}\}_{\tau=-2}^{3}$) from the regressions of Equation (1.18). Only the applicant-year observations where the applicant works in the same school as the application year, and for a denied applicant the years in which she has not been subsequently promoted subsequently, are included. School-specific time trends, applicant-(year 0)-principal fixed effects, applicant-(current)-principal fixed effects are controlled for.

Table A18: Impacts of Perceived Promotion Unfairness on Teachers' Job Characteristics

	Year relative to Promotion Year					
	$t-3$	$t-2$	t	$t+1$	$t+2$	Obs.
	(1)	(2)	(3)	(4)	(5)	
Panel (A): Outcome Variable: Teaching Workload # Sessions Taught Per Week						
Senior-Ranked	-0.0108*	0.0034	-0.0049	0.0042	0.0049	139,743
	(0.0056)	(0.0057)	(0.0053)	(0.0057)	(0.0062)	
Junior-Ranked	-0.0008	-0.0071	0.0056	-0.0061	0.0020	122,974
	(0.0050)	(0.0048)	(0.0047)	(0.0050)	(0.0051)	
Middle-Ranked	0.0036	-0.0006	-0.0006	0.0031	-0.0035	173,281
	(0.0046)	(0.0044)	(0.0043)	(0.0046)	(0.0047)	
Panel (B): Outcome Variable: Being a Class Head Teacher						
Senior-Ranked	-0.0089*	0.0096*	-0.0034	0.0075	-0.0064	139,743
	(0.0053)	(0.0056)	(0.0055)	(0.0054)	(0.0060)	
Junior-Ranked	0.0070	0.0045	0.0069	0.0027	0.0004	122,974
	(0.0058)	(0.0056)	(0.0058)	(0.0056)	(0.0056)	
Middle-Ranked	0.0020	0.0037	0.0052	-0.0032	0.0053	173,281
	(0.0048)	(0.0051)	(0.0049)	(0.0048)	(0.0054)	
Panel (C): Outcome Variable: Leaving Current Classes Before Graduation						
Senior-Ranked	-0.0070*	-0.0040	-0.0044	0.0002	-0.0045	139,743
	(0.0042)	(0.0044)	(0.0043)	(0.0042)	(0.0047)	
Junior-Ranked	-0.0031	-0.0014	-0.0009	-0.0073*	0.0037	122,974
	(0.0046)	(0.0044)	(0.0046)	(0.0044)	(0.0044)	
Middle-Ranked	-0.0056	0.0037	0.0023	-0.0029	0.0058	173,281
	(0.0042)	(0.0040)	(0.0042)	(0.0040)	(0.0040)	
Panel (D): Outcome Variable: End-of-Last-Year Average Test Scores						
Senior-Ranked	0.006	-0.069	-0.031	-0.058	0.100	139,743
	(0.083)	(0.073)	(0.072)	(0.077)	(0.075)	
Junior-Ranked	-0.038	0.004	-0.087	0.040	-0.030	122,974
	(0.086)	(0.075)	(0.073)	(0.082)	(0.088)	
Middle-Ranked	-0.090	0.053	0.058	-0.067	0.057	173,281
	(0.077)	(0.067)	(0.065)	(0.073	(0.078)	

Notes: * p=0.1, ** p=0.05, *** p=0.01. Standard errors clustered at teacher level are reported in parentheses. This table reports the values plotted in Figure A9. These are coefficients on current Undeserving% (interacted with relative year dummies) ($\left\{\hat{\theta}_\tau\right\}_{\tau=-3}^{2}$) from the regressions of Equation (1.20). Only the teacher-year observations where the teacher is a non-applicant in the reference application year (year 0) and works in the same school as in the reference application year ($h(i,t+s)=h(i,t)$), are included. Lagged outcome variables, school-specific time trends, applicant-(year 0) principal fixed effects and applicant-current-principal fixed effects are controlled for.

A3 Model Appendix

This appendix presents the principal-agent model guiding the empirical tests of the impacts of the transparency reform in Section 1.6.

Motivated by the empirical findings discussed in Sections 1.3 and 1.5, the principal-agent model features i) the principal's preference for favoring her socially tied agents, ii) the agents' preference for fairness in the principal's promotion decisions on co-workers, and iii) the introduction of an intervention that allows agents to observe the quality of co-workers like the principal.[1] I derive several testable predictions regarding the implications of the intervention and illustrate how they correspond to the empirical tests performed in Section 1.6.

A3.1 Model Set-Up

Consider the following set-up:

3 agents (A_1, A_2, A_3) work for principal P. A_1 and A_2 apply for one promotion slot, and P decides whom to select. A_3 is a co-worker. A_1 is favored with P and A_2 is not. μ_1 and μ_2 denote the applicants' quality. As the relevant inputs in evaluation are the relative values of μ_1 and μ_2, I assume $\mu_1 = 0$ and let μ_2 follow some distribution F_μ. For simplicity assume symmetry: $F_\mu(0) = \frac{1}{2}$, so that A_2 is 50% likely to be better than A_1.

[1] The model considers a very similar problem to the Prendergast and Topel (1996) and MacLeod (2003) that analyzes the implications of favoritism (or discrimination) under subjective evaluation and its resulting optimal labor contracts. Compared to their frameworks, my formation portrays the empirical setting better and provides more directly testable predictions for several reasons. First, consistent with the empirical setting in which i) the salaries of the teachers are determined and funded by the local government rather than the principal, and ii) principals can observe workers' performance relatively well and they are the highest decision maker within a school, I treat labor contract terms as exogenously given and do not consider bureaucracy problems, and the efficiency cost of discrimination is induced by the workers' social preferences (supported by the results presented in Sub-section 1.5.2.3) and not by the cost of contract implementation arising from subjective evaluation (MacLeod, 2003), or the de-emphasis of incentive pay for workers due to arbitrariness in rewards and less productive job assignment due to inefficient aggregation of information on workers' performance by the firm and the supervisor (middle-level manager who shows favoritism and reports subordinates' performance to the firm) (Prendergast and Topel, 1996). In addition, although favoritism in preferences is an exogenous parameter, discriminating (or unfair) behavior by the principal is endogenous in my model (same as Prendergast and Topel (1996) and different from MacLeod (2003)), allowing me to perform corresponding comparative statics analysis using the policy intervention.

P derives happiness from promoting his favored agent A_1, and his utility function is given by

$$U^P(e, S_1) = e + \theta S_1, \tag{A1}$$

where e is the effort A_3's effort exerted at work,[2] $S_1 = 1$ if the he promotes A_1, and parameter $\theta \geq 0, \theta \sim F_\theta$ represents P's extent of favoritism.

I adapt the framework of DellaVigna et al. (2016) and Breza et al. (2017), in which workers' social preferences affect effort provision. Specifically, A_3 derives happiness from seeing co-worker i with higher μ_i receive promotion (that is, fair promotion),[3] and her utility function is given by

$$V(e, S_1) = u(e) - c(e) + M(S_1)\, e, \tag{A2}$$

where $u(\cdot)$ represents the intrinsic utility from teaching, $u' > 0$, $u'' < 0$.[4] $c(\cdot)$ is the cost of effort, $c' > 0$, $c'' > 0$. $\lim_{e \to 0^+} u'(e) > \lim_{e \to 0^+} c'(e)$. $M(S_1)$ is a morale effect term that depends on the A_3's posterior perceived probability that the promotion is fair:

$$M(S_1) := S_1 \times \Pr_3[\mu_2 < 0 | S_1 = 1] + (1 - S_1) \times (1 - \Pr_3[\mu_2 < 0 | S_1 = 0]). \tag{A3}$$

The first term refers to the case where A_2 is of lower quality than A_1 and P promotes A_1, while the second term refers to the case where A_2 is of higher quality and receives promotion.

[2]I do not include the efforts of A_1 and A_2 in the model, as the findings in Sub-section 1.5.1.3 suggest that the principal's promotion decision has little impact on the average performance of the applicants, and it is the co-workers' performance that is affected. In the setting of Chinese public high schools, a principal is not monetarily rewarded for the teachers' effort (or the test scores produced by his school), but it might still enter his utility function for two reasons. First, the performance of the school she manages might affect her own promotion prospects to better schools or higher-level positions in the public education system. In addition, principals might intrinsically value good test scores and long-term well-being of the students, or social pressure might make them care about their subordinates viewing them positively. All these hypotheses are formulated using by the (lack of) effort reduction induced by a morale effect.

[3]This is the mechanism for which the discussions in Sub-section 1.5.2.3 finds most support. I do not consider job quitting in this model, as the turnover rates in the Chinese high schools are relatively low (5%) and unlikely to drive the overall impacts on schools compared to the effort effect.

[4]As wage is fixed in the empirical setting and independent of effort, I introduce the intrinsic utility of effort to induce the conflict needed for agents to exert positive level of effort.

Now I introduce the transparency reform which affects the observability of μ_2 to A_3.

Pre-reform: P can observe μ_2 perfectly *ex-ante*, while A_3 cannot observe μ_2 and her prior belief is $\mu_2 \sim F_\mu$.

Post-reform: Both P and A_3 can observe μ_2 perfectly *ex-ante*.

A3.2 Equilibria

The equilibrium solution to the model consists of i) P's promotion strategy given μ_2: $S_1(\mu_2)$; ii) A_3's posterior belief about μ_2 given S_1: $F'_\mu(\cdot|S_1)$; iii) A_3's effort chosen given S_1 and $F'_\mu(\cdot|S_1)$: $e^*(S_1)$.

Pre-reform equilibrium:

$$S_1^{Pre}(\mu_2|\theta) = \begin{cases} 1, & \text{if } \theta \geq \theta^*; \\ 0, & \text{if } \theta < \theta^*. \end{cases} \tag{A4}$$

$$\Pr_3^{Pre}[\mu_2 < 0|S_1] = \begin{cases} \frac{1}{2}, & \text{if } S_1 = 1; \\ 0, & \text{if } S_1 = 0. \end{cases} \tag{A5}$$

$$e^{*Pre}(S_1) = \begin{cases} e^{mid}, & \text{if } S_1 = 1; \\ e^{max}, & \text{if } S_1 = 0. \end{cases} \tag{A6}$$

Post-reform equilibrium:

$$S_1^{Post}(\mu_2|\theta) = \begin{cases} 1, & \text{if } \theta \geq \theta^{**}; \\ 1, & \text{if } \mu_2 \geq 0,\ 0 < \theta < \theta^{**}; \\ 0, & \text{if } \mu_2 < 0,\ 0 < \theta < \theta^{**}. \end{cases} \tag{A7}$$

$$\Pr_3^{Post}[\mu_2 < 0|S_1] = \mathbb{I}[\mu_2 < 0]. \tag{A8}$$

$$e^{*Post}\left(S_1\right)=\begin{cases} e^{min}, & \text{if } \left[S_1=1,\mu_2\geq 0\right] \text{ or } \left[S_1=0,\mu_2<0\right]; \\ e^{max}, & \text{if } \left[S_1=1,\mu_2<0\right] \text{ or } \left[S_1=0,\mu_2\geq 0\right]. \end{cases} \tag{A9}$$

where $\left(e^{max}, e^{mid,}e^{min}\right)$ is the solution to

$$\begin{cases} u'(e^{max})-c'(e^{max})+1 &= 0; \\ u'(e^{mid})-c'(e^{mid})+\frac{1}{2} &= 0; \\ u'(e^{min})-c'(e^{min}) &= 0. \end{cases}$$

and

$$\begin{cases} \theta^{*}=e^{max}-e^{mid}; \\ \theta^{**}=e^{max}-e^{min}. \end{cases}$$

It is obvious that $e^{min}<e^{mid}<e^{max}$ and therefore $0<\theta^{*}<\theta^{**}$.

A3.3 Model Predictions

Define event E_{fair} as the set of all fair promotion outcomes: $E_{fair}:=\{\left[S_1=1,\mu_2<0\right],\left[S_1=0,\mu_2\geq 0\right]\}$, and E_{Unfair} the set of unfair promotion outcomes: $E_{Unfair}:=\{\left[S_1=1,\mu_2\geq 0\right]\}$. I state the following propositions regarding the impacts of the reform:

Proposition 1. *(Effort Response to Unfairness) The expected levels of equilibirum effort by co-worker A_3 conditional on events E_{Fair} and E_{Unfair} pre- and post-reform is given satisfy:*

$$\begin{cases} \mathbb{E}^{Pre}\left[e^{*}|E_{Fair}\right]-\mathbb{E}^{Pre}\left[e^{*}|E_{Unfair}\right]=-\frac{1}{2}\theta^{*}; \\ \mathbb{E}^{Post}\left[e^{*}|E_{Fair}\right]-\mathbb{E}^{Post}\left[e^{*}|E_{Unfair}\right]=-\theta^{**}. \end{cases}$$

Therefore $\mathbb{E}^{Post}\left[e^{*}|E_{Fair}\right]-\mathbb{E}^{Post}\left[e^{*}|E_{Unfair}\right]<\mathbb{E}^{Pre}\left[e^{*}|E_{Fair}\right]-\mathbb{E}^{Pre}\left[e^{*}|E_{Unfair}\right]<0$. That is, the impact of principal P's unfairness on co-worker A_3's expected performance is larger after the reform.

The intuition is that in the pre-reform setting, as A_3 cannot observe μ_2 perfectly even *ex-post*, when P acts unfairly, A_3 still believes there is a positive probability that the promotion outcome is fair; therefore A_3 punishes P less harshly compared to the post-reform scenario where A_3 knows with complete accuracy that P is unfair if he actually is.

Proposition 2. *(Probability of Unfairness) The probabilities with which the unfair event E_{Unfair} takes place pre- and post- reform are given by:*

$$\begin{cases} \Pr^{Pre}\left[E_{Unfair}\right] = \frac{1}{2}\Pr\left[\theta \geq \theta^*\right]; \\ \Pr^{Post}\left[E_{Unfair}\right] = \frac{1}{2}\Pr\left[\theta \geq \theta^{**}\right]. \end{cases}$$

Therefore $\Pr^{Post}\left[E_{Unfair}\right] < \Pr^{Pre}\left[E_{Unfair}\right]$. That is, the probability with which principal P delivers an unfair promotion outcome is lower after the reform.

The intuition is that as the cost of unfair promotion is higher after the reform, the required level of favoritism for principal P to act unfairly is higher ($\theta^{**} > \theta^*$) in order to compensate for the cost from reduced effort levels.

Proposition 3. *(Expected Level of Effort) The expected levels of equilibrium effot by co-worker A_3 pre- and post-reform satisfy:*

$$\mathbb{E}^{Post}\left[e^*\right] - \mathbb{E}^{Pre}\left[e^*\right] = \frac{1}{2}\left[\underbrace{\theta^*\left(\Pr\left[\theta \geq \theta^*\right] - \Pr\left[\theta \geq \theta^{**}\right]\right)}_{+(P \text{ less likely to be unfair})} + \underbrace{\Pr\left[\theta \geq \theta^{**}\right]\left(\theta^* - \theta^{**}\right)}_{-(A_3 \text{ punishes unfairness more heavily})}\right].$$

Therefore it is ambiguous whether the reform entails an efficiency gain or loss.

The overall impact of the reform on efficiency depends on the relative strengths between its two counter-acting effects: making principal P less likely to treat applicants A_1 and A_2 unfairly, while at the same time making co-worker A_3 punish P more heavily if he indeed chooses to do so. If principal P's preferences for his friend A_1 is more likely to be at the margin of making him change his behavior pre- and post-reform (higher $\Pr\left[\theta^* < \theta \leq \theta^{**}\right]$), it is more likely that the reform improves efficiency.

A3.4 Tests of Model Predictions

The empirical analogue to $\mathbb{I}\left[E_{Unfair}\right]$ in the model is Undeserving% , i.e., the extent to which one observe unfairly promoted applicants; and $\mathbb{E}\left[\text{Undeserving\%}\right]$ corresponds to $\Pr\left[E_{Unfair}\right]$. $\mathbb{E}\left[e^*|E_{Fair}\right] - \mathbb{E}\left[e^*|E_{Unfair}\right]$ corresponds to the effect Undeserving% has on the expected productivity of teachers.

Proposition 1 can be tested using the following difference-in-difference-in-differences model:

$$Y = \theta^{Post}\text{Post} \times \text{Undeserving\%} + \theta^{Pre}\left(1 - \text{Post}\right) \times \text{Undeserving\%} + \pi\text{Post} + \text{Controls} + \varepsilon.$$

where Post $=$ 1 if the reform is effective. The empirical prediction of Proposition 1 is $\theta^{Post} < \theta^{Pre} < 0$.

Propositions 2 and 3 can be tested using the following difference-in-differences model:

$$Y = \delta\text{Post} + \text{Controls} + \varepsilon,$$

Proposition 2 predicts that $\delta < 0$, while proposition 3 does not give an unambiguous prediction regarding the sign of δ.

Appendix B

Appendix of Chapter II

B1 Figure Appendix

Figure B1: Distribution of Individual Daily Production

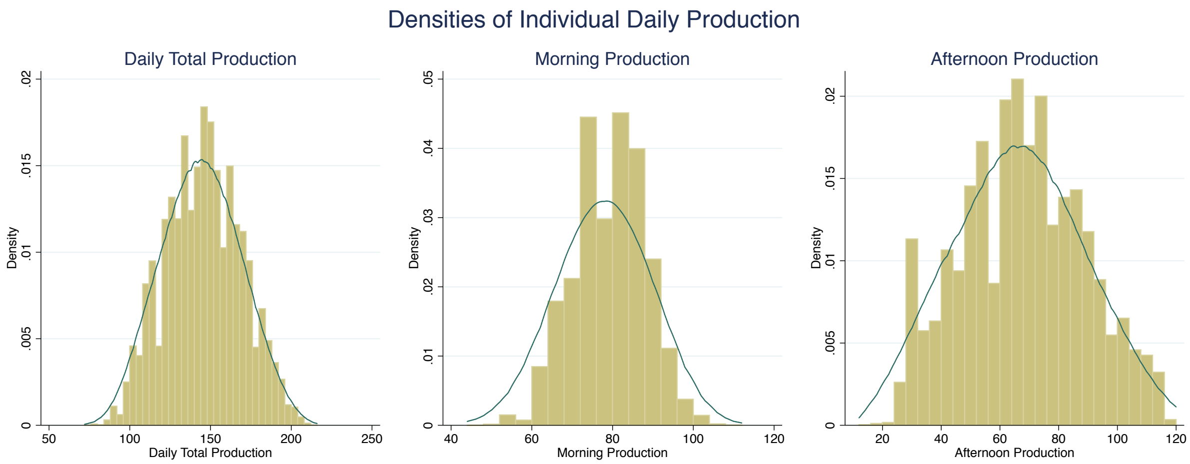


Figure B2: Distribution of Individual Frequencies of Gambling Participation

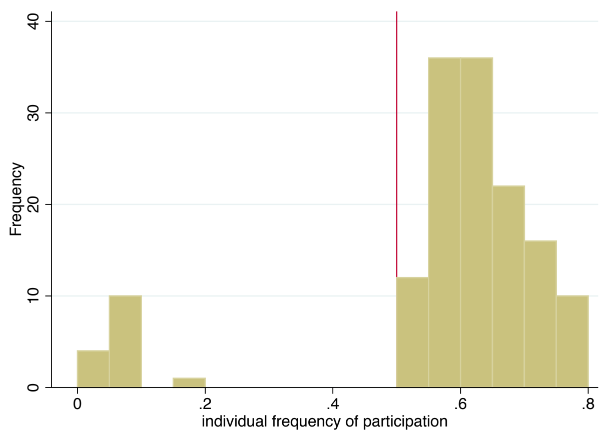
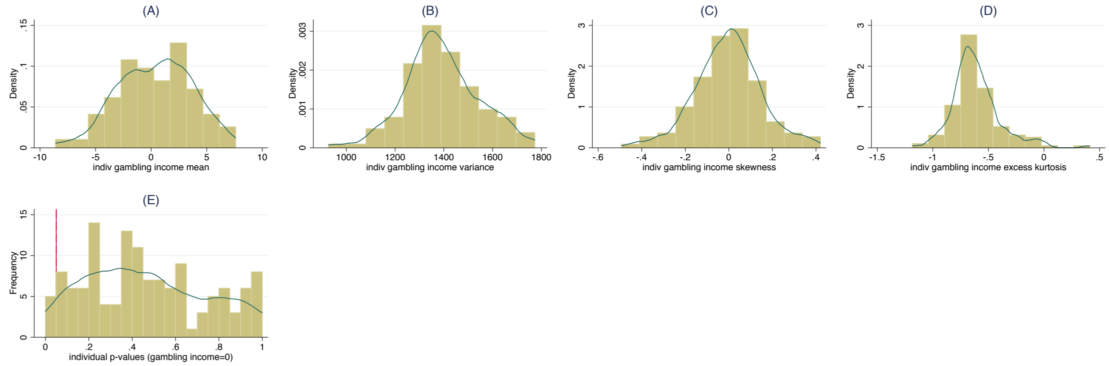
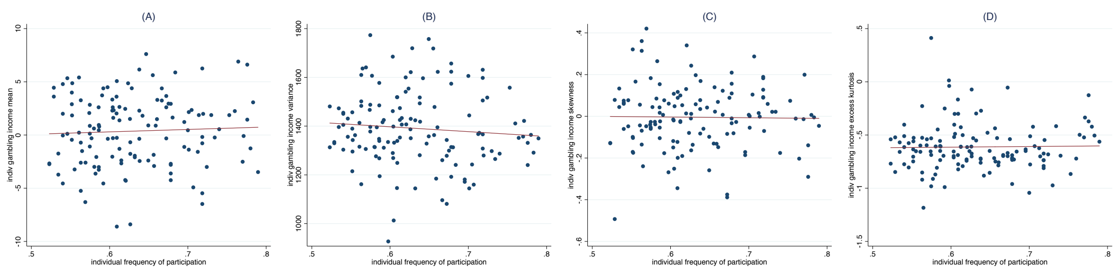


Figure B3: Distributions of Individual Gambling Income Moments



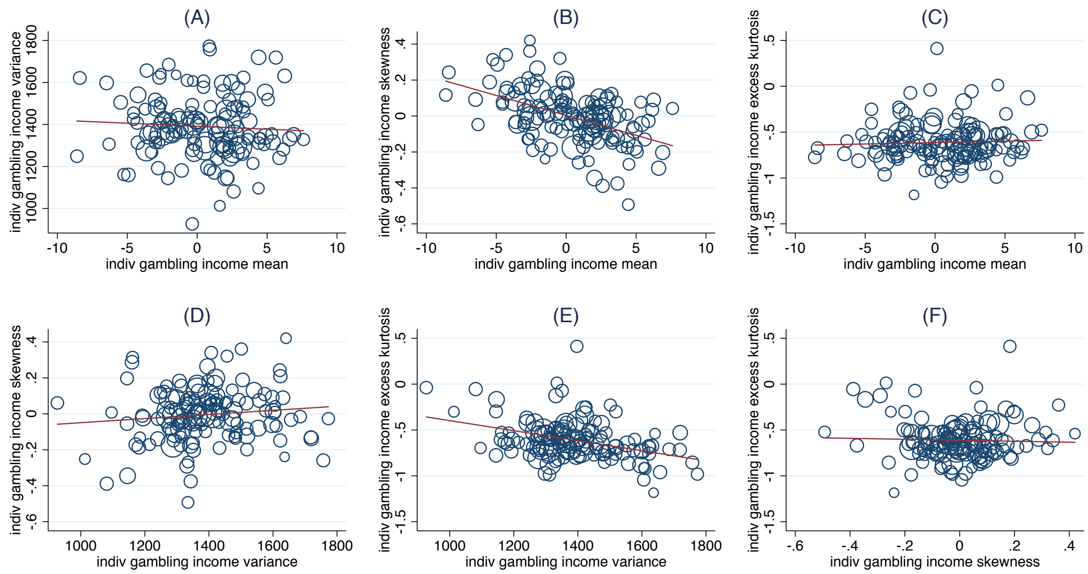
Notes: Only frequent gamblers ($Pr(G = 1) \geq 0.5$) are included.

Figure B4: Individual Gambling Income Moments versus Frequencies of Participation



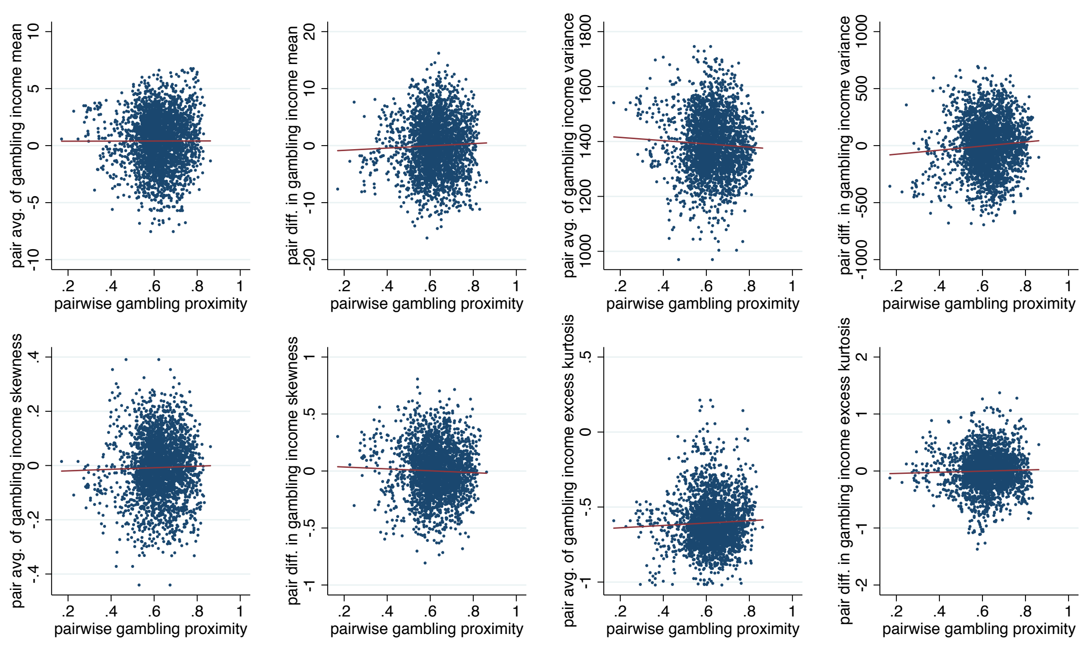
Notes: Only frequent gamblers ($Pr(G = 1) \geq 0.5$) are included.

Figure B5: Inter-Relationships between Individual Gambling Participation and Gambling Income Moments



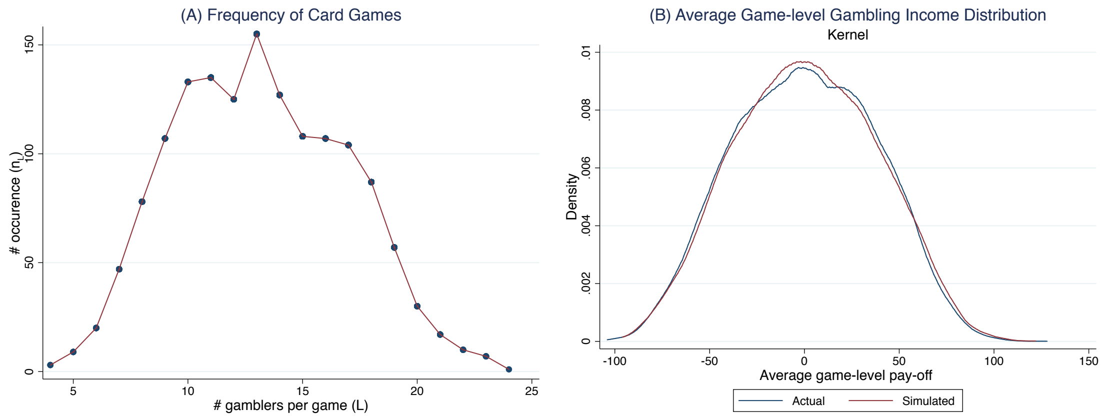
Notes: Only frequent gamblers ($Pr(G = 1) \geq 0.5$) are included. Size of bubbles is proportional to # of individual gambling participation days.

Figure B6: Inter-Worker Gambling Proximity and Gambling Income Distribution



Notes: Only frequent gamblers ($Pr(G = 1) \geq 0.5$) are included. Pairwise $Proximity_{ij} = Pr(G_j = 1|G_i = 1)$ is the probability that worker j gambled on worker i's gambling participation days.

Figure B7: Within-Game # Participants and Distribution of Payoffs



Notes: Observations are weighted by $1/(\#within - game - participants \times \#games)$ so that the sum of weights in Panel (B) is one and the graph is comparable to the distribution of a univariate random variable.

Table B3: Descriptive Statistics on Workers: Gamblers vs Non-Gamblers

Variable	Frequent Gamblers	Infrequent Gamblers	Difference
Age at Feb. 2014	28.28	28.78	-.50 (0.695)
Married (yes=1)	.370	.428	-.057 (0.676)
Have Children (yes=1)	.275	.357	-.081 (0.528)
Months in Factory till May. 2014	35.5	32.2	3.3 (0.335)
Years as Manufacturing Worker	7.60	7.78	-.18 (0.838)
# Loterries Rejected (Loss Aversion)	3.60	4.71	-1.11*** (0.000)
# observations	116	14	-

Notes: Frequent gamblers are those who participated in card games more that 50% of workdays. p-values of differences in parentheses ($p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

B2 Table Appendix

Table B1: Information on Manufacturing Factories

Factory	A	B	C	D	E	F
Established	05/2008	03/2010	06/2010	09/2011	02/2012	07/2009
Number of Workers	19	26	30	28	27	17
Mar 2011-Mar 2012	×					
Dec 2012-Apr 2013	×	×				
Oct 2013-May 2014	×	×	×	×	×	×
Product	Bearing	Wheel and Axle	Bearing	Fan	Bracket	Doorknob
Piece rate (RMB)	4/item	4/item	5/item	6/item	5/item	4/item
Worker entry (quit)	3(5)	0(0)	2(2)	0(2)	3(0)	0(2)
Obs	8,831	7,150	5,042	4,807	4,606	2,847

Table B2: Descriptive Statistics on Workers

Variable	Notation	Mean	St Dev.	Min	Max	# Obs
Panel A: Worker×day observations						
Daily Total Production (RMB)	TP	144.67	23.34	72	216	33,103
Morning Production (RMB)	MP	78.14	8.62	44	110	33,103
Afternoon Production (RMB)	AP	66.52	20.98	12	120	33,103
Gamble Participation (yes=1)	G	.592	.491	0	1	33,103
Daily Gambling Income (RMB)	GI	.60	37.07	-104	128	19,771
Panel B: Worker observations						
Age at Feb. 2014	Age	28.33	4.49	20	43	130
Married (yes=1)	$Married$	.376	.486	0	1	130
Have Children (yes=1)	$Children$	.284	.452	0	1	130
Months in Factory till May. 2014	$Duration$	35.2	12.0	15	73	130
Years as Manufacturing Worker	$Experience$	7.62	3.13	2	15	130
# Loterries Rejected (Loss Aversion)	$Loss\ Aversion$	3.72	.72	2	5	130

Notes: Notations for variables are used in equations in the empirical estimation. In Panel A, observations are weighted by the inverse of individual total workdays.

Table B4: Descriptive Statistics on Frequent Gamblers' Workdays: Gambling vs Non-Gambling Days

Variable	Participating Days ($G = 1$)	Non-Participating Days ($G = 0$)	Difference
Daily Total Production (RMB)	143.85	144.11	-.26 (0.359)
Morning Production (RMB)	77.58	77.78	-.20* (0.057)
Afternoon Production (RMB)	66.26	66.32	-.05 (0.816)
Daily Gambling Income (RMB)	.36	-	-
# observations	19,090	10,948	-

Notes: Frequent gamblers are those who participated in card games more that 50% of workdays. Observations are weighted by the inverse of total gambling participation days of each individual. p-values of differences in parentheses ($p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table B5: Variance Decomposition of Gambling Participation and Gambling Income

	(1)	(2)	(3)	(4)
	Participation G		Gambling Income: $GI \mid G = 1$	
Total variance	0.232		1390.42	
	Fraction Explained	p-val F stat	Fraction Explained	p-val F stat
Individual	0.0199***	0.0000	0.0078	0.1402
Factory×(Monthly) Pay Cycle	0.0027*	0.0931	0.0033	0.6107
# Days to Payday	0.0143***	0.0000	0.0019	0.5377
Day of the Week	0.0001	0.7741	0.0003	0.1932
Card Game Opponents	0.0159***	0.0000	0.0088	0.1056
Morning Production	0.0001**	0.0402	0.0000	0.4342
Yesterday Participation	0.0013***	0.0000	0.0000	0.7538
Yesterday Gambling Income	0.0000	0.6026	0.0002*	0.0696
Total Explained	0.0543***	0.0000	0.0223*	0.0994
# Obs	30,038		19,090	

Notes: Only frequent gamblers are included. For G, observations are weighted by the inverse of total workdays of each individual; for $GI|G = 1$, observations are weighted by the inverse of total gambling participation days of each individual. I code gambling income=0 if the worker did not gamble on that day.
p-values of differences in parentheses ($p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table B6: Rosenbaum (2005) Test of Within-Game Level Gambling Income Distribution

# Players in Each Game L	# Occurence n_L	p-value (actual payoffs $\hat{F}_L^{act.}$ vs simulated lotteries $\hat{F}_L^{sim.}$)
4	3	0.59
5	9	0.75
6	20	0.55
7	47	0.50
8	78	0.39
9	107	0.42
10	133	0.49
12	125	0.50
13	155	0.09*
14	127	0.21
15	108	0.16
16	107	0.38
17	104	0.29
18	87	0.48
19	57	0.33
20	30	0.72
21	17	0.47
22	10	0.38
23	7	0.77
24	1	-

Notes: The Rosenbaum (2005) cross-match test is an exact, distribution free test of equality of 2 high dimensional multivariate distributions. p-values of differences in parentheses ($p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table B7: Variance Decomposition of Morning and Afternoon Production

	(1)	(2)	(3)	(4)
	Morning Production $MP \mid G=1$		Afternoon Productin: $AP \mid G=1$	
Total variance	79.25		533.69	
	Fraction Explained	p-val F stat	Fraction Explained	p-val F stat
Individual	0.2145***	0.0000	0.0233***	0.0000
Factory×(Monthly) Pay Cycle	0.4419***	0.0000	0.0398***	0.0000
# Days to Payday	0.1146***	0.0000	0.0113***	0.0000
Day of the Week	0.0968***	0.0000	0.0069***	0.0000
Card Game Opponents	0.0001***	0.0000	0.0001***	0.0000
Explained by anticipated	0.6684***	0.0000	0.0727***	0.0000
Gambling Income	0.0000***	0.0000	0.3669***	0.0000
Total Explained	0.6684***	0.0000	0.4334***	0.0000
# Obs	19,090		19,090	

Notes: Only gambling participation workdays of frequent gamblers are included. Observations are weighted by the inverse total gambling participation days of each individual.
p-values of differences in parentheses ($p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

B3 Other Appendix

B3.1 Appendix A

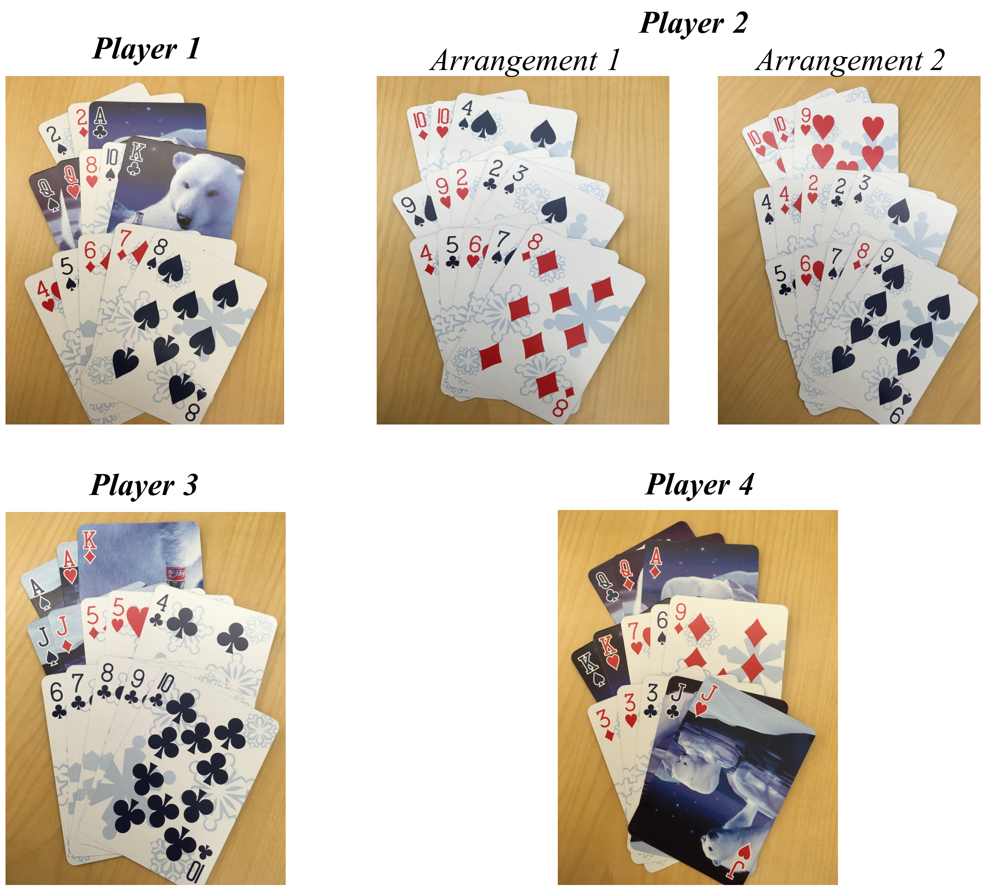
Card Game and Players' Payoffs per Table-Round

There were 4 players in a table-round, each with 13 cards drawn in turn from a deck. Then players chose how to arrange their cards to get a three-row combination (3 (head)-5 (body)-5 (foot)), trying to form each row as some combination such as Straight Flush, Four of a kind, Full House, Flush, Straight, Straight Flush, Three of a kind, Two Pairs, One Pair, Separate, etc. There were rules in which the same row (head, body or foot) of each player were pairwise comparable and they player with the "better" row won one point. After everyone finalized their card arrangement, they showed their combinations simultaneously, and their combinations are compared pairwise with all the other 3 players. Each player could win 3 (winning all 3 rows), 1 (winning 2 rows and losing 1), -1 (winning 1 row and losing 2), -3 (losing 3 all rows) points from another player, and all the pairwise wins/losses are summed up to determine a player' overall win/loss in the round. Each point was worth 2 RMB so the individual payoff from each round is two times his total points won. See an example in Figure A1, the players' payoffs from which are shown in Panel (A) of Table A1 if Player 2 arranged his cards in Method 1, and Panel (B) if he adopted Method 2. Therefore, the payoffs depends on which cards players drew and the way players arranged his cards at hand.

Table B8: Pairwise Win/Loss and Players' Payoffs

	Pairwise Win/Loss Vector (Head, Body, Foot)				Payoff
	Player 1	Player 2	Player 3	Player 4	
		Arrangment 1 (Player 2)			
Player 1	-	(-1,-1,1)=-1	(-1,-1,-1)=-3	(-1,-1,-1)=-3	-7×2=-14
Player 2	(1,1,-1)=1	-	(-1,-1,-1)=-3	(-1,1,-1)=-1	-3×2=-6
Player 3	(1,1,1)=3	(1,1,1)=3	-	(1,1,1)=3	9×2=18
Player 4	(1,1,1)=3	(1,-1,1)=1	(-1,-1,-1)=-3	-	1×2=2
		Arrangment 2 (Player 2)			
Player 1	-	(-1,-1,-1)=-3	(-1,-1,-1)=-3	(-1,-1,-1)=-3	-9×2=-18
Player 2	(1,1,1)=3	-	(-1,-1,-1)=-3	(-1,1,-1)=-1	-1×2=-2
Player 3	(1,1,1)=3	(1,1,1)=3	-	(1,1,1)=3	9×2=18
Player 4	(1,1,1)=3	(1,-1,1)=1	(-1,-1,-1)=-3	-	1×2=2

Figure B8: A Table-Round Example of the Card Game



B3.2 Appendix B

Derivation of the Likelihood Function (2.19)

Worker i stops working if and only if $U\left(I^L_{it,j+1}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) - U\left(I^L_{itj}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) < 0$, where $U\left(I^L_{itj}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right)$ is defined in Equation (8.6).

$$\begin{gathered} U\left(I^L_{it,j+1}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) - U\left(I^L_{itj}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) \\ = \lambda\left[I\left[K_t I^L_{it,j+1} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{it,j+1} + I^U_{it}\right) - I\left[K_t I^L_{itj} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{itj} + I^U_{it}\right)\right] \\ + I\left[K^t I^L_{it,j+1} + I^U_{it} - T_{it} \geq 0\right]\left(K_t I^L_{it,j+1} + I^U_{it}\right) - I\left[K_t I^L_{itj} + I^U_{it} - T_{it} \geq 0\right]\left(K_t I^L_{itj} + I^U_{it}\right) \\ - \tfrac{\theta}{1+\nu}\left[\left(I^L_{it,j+1}\right)^{1+\nu} - \left(I^L_{itj}\right)^{1+\nu}\right] + \zeta_{it,j+1}. \end{gathered}$$

where $K_t = \beta^{I[t<\bar{t}]}\delta^{\bar{t}-t}$ is the compound discount factor. Notice that

$$\begin{gathered} \left\{I\left[K_t I^L_{it,j+1} + I^U_{it} - T_{it} \geq 0\right]\left(K_t I^L_{it,j+1} + I^U_{it}\right) - I\left[K_t I^L_{itj} + I^U_{it} - T_{it} \geq 0\right]\left(K_t I^L_{itj} + I^U_{it}\right)\right\} \\ + \left\{I\left[K_t I^L_{it,j+1} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{it,j+1} + I^U_{it}\right) - I\left[K_t I^L_{itj} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{itj} + I^U_{it}\right)\right\} \\ = I^L_{it,j+1} - I^L_{itj} = w_i, \end{gathered}$$

where w_i is the piece rate for worker i, and denote

$$A_{itj} = I\left[K_t I^L_{it,j+1} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{it,j+1} + I^U_{it}\right) - I\left[K_t I^L_{itj} + I^U_{it} - T_{it} < 0\right]\left(K_t I^L_{itj} + I^U_{it}\right),$$

we have

$$\begin{gathered} U\left(I^L_{it,j+1}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) - U\left(I^L_{itj}, I^U_{it}|T_{it};\lambda,\nu,\theta,\beta,\delta\right) = \\ \lambda A_{itj} + (w_i - A_{itj}) + (w_i - A_{itj}) - \tfrac{\theta}{1+\nu}\left[\left(I^L_{it,j+1}\right)^{1+\nu} - \left(I^L_{itj}\right)^{1+\nu}\right] + \zeta_{it,j+1}. \end{gathered}$$

Replace $\zeta_{it,j+1}$ with $c^\zeta + x_{it}\beta^\zeta + \alpha^\zeta(j+1) + \epsilon^\zeta_{it,j+1}$, the probability of stopping after piece j *conditional* upon continuing after piece $j-1$ is given by:

$$\begin{gathered} Pr\left(stop_{itj} = 1|stop_{itl} = 0,\ 0 \leq l < j\right) \\ = \Pr\left(\lambda A_{itj} + (w_i - A_{itj}) - \tfrac{\theta}{1+\nu}\left[\left(I^L_{it,j+1}\right)^{1+\nu} - \left(I^L_{itj}\right)^{1+\nu}\right] + c^\zeta + x_{it}\beta^\zeta + \alpha^\zeta(j+1) + \epsilon^\zeta_{it,j+1} < 0\right) \\ = \Phi\left(\tfrac{F_{itj}}{\sigma_\zeta}\right), \end{gathered}$$

where $F_{itj} = \frac{\theta}{1+\nu}\left[\left(I^L_{it,j+1}\right)^{1+\nu} - \left(I^L_{itj}\right)^{1+\nu}\right] - w_i - (\lambda-1)A_{itj} - \left[c^\zeta + x_{it}\beta^\zeta + \alpha^\zeta(j+1)\right]$.

Set starting value $\Pr(stop_{it0} = 1) = 0$[1], the *unconditional* probability of stopping after piece j is given by:

[1]This assumption implies the worker at least produced one piece in the afternoon.

$$
\begin{aligned}
\Pr\left(stop_{itj}=1\right) &= Pr\left(stop_{itj}=1 | stop_{itl}=0,\ 0\leq l<j\right)\prod_{l=1}^{j-1} Pr\left(stop_{itl}=0 | stop_{itk}=0,\ 0\leq k<l\right) \\
&= \Phi\left(\frac{F_{itj}}{\sigma_\zeta}\right)\prod_{l=1}^{j-1}\left[1-\Phi\left(\frac{F_{itl}}{\sigma_\zeta}\right)\right].
\end{aligned}
$$

The likelihood function is

$$
\mathscr{L}=\prod_{i}\prod_{t\in\{t|G_{it}=1\}}\prod_{j=1}^{J_{it}}\Pr\left(stop_{itj}=1\right)^{stop_{itj}}\Pr\left(stop_{itj}=0\right)^{1-stop_{itj}},
$$

where $J_{it}=AP_{it}/w_i$ is the number of items produced in the afternoon.

B3.3 Appendix C

Derivation of the Formula (2.20)

Suppose log unanticipated and anticipated hourly wages are independent and normally distributed with mean zero and variances $\alpha\sigma^2$ and $(1-\alpha)\sigma^2$, respectively. Therefore the total variance of log wage is σ^2.

Elasticity of labor supply is equal to $\frac{1}{\nu}$ (Frisch) to anticipated log wage variation.

With respect to unanticipated wage variation $\ln w^u \sim N\left(0, \alpha\sigma^2\right)$, the worker chooses unanticipated hours h to maximize utility

$$U\left(w^u, h|T\right) = \left\{\lambda I\left[w^u h - T < 0\right] + I\left[w^u h - T \geq 0\right]\right\} w^u h - \frac{\theta}{1+\nu} h^{1+\nu},$$

which is essentially the same as Equations (1) and (2) in Farber (2015). T is the daily income target (net of anticipated earnings). The solution is given by:

$$\ln h^*\left(w, T\right) \begin{cases} \frac{1}{\nu}\left(\ln\lambda - \ln\theta + \ln w^u\right), & if \ \ln w^u \leq \frac{1}{1+\nu}\left(\ln\theta - \ln\lambda + \nu\ln T\right) \\ \ln T - \ln w^u, & if \ \frac{1}{1+\nu}\left(\ln\theta - \ln\lambda + \nu\ln T\right) < \ln w^u < \frac{1}{1+\nu}\left(\ln\theta + \nu\ln T\right) \\ \frac{1}{\nu}\left(-\ln\theta + \ln w^u\right), & if \ \ln w^u \geq \frac{1}{1+\nu}\left(\ln\theta + \nu\ln T\right) \end{cases}$$

Assume T is set such that the interval of log wage in which elasticity is -1 is centered around the mean value zero, we have

$$\begin{cases} \frac{1}{1+\nu}\left(\ln\theta - \ln\lambda + \nu\ln T\right) = -\frac{\ln\lambda}{2(1+\nu)} \\ \frac{1}{1+\nu}\left(\ln\theta + \nu\ln T\right) = \frac{\ln\lambda}{2(1+\nu)} \end{cases} \Rightarrow \ln T = \frac{1}{2\nu}\left(\ln\lambda - 2\ln\theta\right).$$

Unanticipated wage elasticity is equal to $\frac{1}{\nu}$ (Frisch) outside $\left(-\frac{\ln\lambda}{2(1+\nu)}, \frac{\ln\lambda}{2(1+\nu)}\right)$, and -1 inside. The the expected unanticipated wage elasticity:

$$\begin{aligned} \varepsilon^u &= -\Pr\left(-\frac{\ln\lambda}{2(1+\nu)} < \ln w^u < \frac{\ln\lambda}{2(1+\nu)}\right) + \frac{1}{\nu}\left\{\Pr\left(\ln w \leq -\frac{\ln\lambda}{2(1+\nu)}\right) + \Pr\left(\ln w \geq \frac{\ln\lambda}{2(1+\nu)}\right)\right\} \\ &= -\left(2\Phi\left(\frac{\ln\lambda}{2(1+\nu)\sqrt{\alpha}\sigma}\right) - 1\right) + \frac{2}{\nu}\left(1 - \Phi\left(\frac{\ln\lambda}{2(1+\nu)\sqrt{\alpha}\sigma}\right)\right). \end{aligned}$$

Anticipated wage elasticity $\varepsilon^a = \frac{1}{\nu}$. Therefore, the expected composite wage elasticity:

$$\begin{aligned} \varepsilon^{comp.} &= \alpha\varepsilon^u + (1-\alpha)\varepsilon^a \\ &= \alpha\left[-\left(2\Phi\left(\frac{\ln\lambda}{2(1+\nu)\sqrt{\alpha}\sigma}\right) - 1\right) + \frac{2}{\nu}\left(1 - \Phi\left(\frac{\ln\lambda}{2(1+\nu)\sqrt{\alpha}\sigma}\right)\right)\right] + (1-\alpha)\frac{1}{\nu} \\ &= \underbrace{-\alpha\left[2\Phi\left(\frac{\ln\lambda}{2\left(1+\nu\right)\sigma\sqrt{\alpha}}\right) - 1\right]}_{\text{RD elasticity } \varepsilon^{RD} \text{ (-)}} + \underbrace{\frac{1}{\nu}\left[1 + \alpha - 2\alpha\Phi\left(\frac{\ln\lambda}{2\left(1+\nu\right)\sigma\sqrt{\alpha}}\right)\right]}_{\text{Neoclassical elasticity } \varepsilon^{Neo.} (+)}. \end{aligned}$$

Expected log earnings from unanticipated wage is the following:

$$\begin{aligned}
\mathbb{E}\left[\ln\left(I_{u.}^{L,RD}\right)\right] &= \int_{-\infty}^{-\frac{\ln\lambda}{2(1+\nu)}} \left[\tfrac{1}{\nu}\left(\ln\lambda - \ln\theta + \ln w^u\right) + \ln w^u\right] dF\left(\ln w^u\right) \\
&+ \int_{-\frac{\ln\lambda}{2(1+\nu)}}^{\frac{\ln\lambda}{2(1+\nu)}} \left[\tfrac{1}{2\nu}\left(\ln\lambda - 2\ln\theta\right) - \ln w^u + \ln w^u\right] dF\left(\ln w^u\right) \\
&+ \int_{\frac{\ln\lambda}{2(1+\nu)}}^{\infty} \left[\tfrac{1}{\nu}\left(-\ln\theta + \ln w^u\right) + \ln w^u\right] dF\left(\ln w^u\right) \\
&= \tfrac{\ln\lambda - 2\ln\theta}{2\nu} + \tfrac{1+\nu}{\nu}\left(1 - \Phi\left(\tfrac{\ln\lambda}{2(1+\nu)\sqrt{\alpha}\sigma}\right)\right)\left\{\mathbb{E}\left[\ln w^u \middle| \ln w^u \le -\tfrac{\ln\lambda}{2(1+\nu)}\right] + \mathbb{E}\left[\ln w^u \middle| \ln w^u \ge \tfrac{\ln\lambda}{2(1+\nu)}\right]\right\} \\
&= \tfrac{\ln\lambda - 2\ln\theta}{2\nu}.
\end{aligned}$$

Suppose the worker switches to a neoclassical labor supplier, and he chooses h to maximize utility

$$U\left(w^u, h\right) = \lambda w^u h - \frac{\theta}{1+\nu} h^{1+\nu},$$

where the marginal value of income is λ which is equal to that at the loss side under reference dependence[2]. The solution is $\ln h^{**}\left(w^u\right) = \frac{1}{\nu}\left(\ln\lambda - \ln\theta + \ln w^u\right)$.

Expected log earnings from unanticipated wage is the following:

$$\begin{aligned}
\mathbb{E}\left[\ln\left(I_u^{L,Neo.}\right)\right] &= \int_{-\infty}^{\infty} \left[\tfrac{1}{\nu}\left(\ln\lambda - \ln\theta + \ln w^u\right) + \ln w^u\right] dF\left(\ln w^u\right) \\
&= \tfrac{\ln\lambda - \ln\theta}{\nu}.
\end{aligned}$$

Expected log earnings from anticipated wage are identical for both neoclassical and reference-dependent preferences over income. Therefore the difference in expectations of total wage is

$$\alpha\left(\mathbb{E}\left[\ln\left(I^{L,Neo.}\right)\right] - \mathbb{E}\left[\ln\left(I^{L,RD}\right)\right]\right) = \frac{\alpha}{2}\ln\lambda.$$

[2]The marginal value of income in the reference dependent utility function should be viewed as jumping down when surpassing the reference point from left, rather than jumping up when crossing the point from the right.

B3.4 Appendix D

Curvature in Utility Function of Daily Income

Diminishing marginal return to real or face-valued mental daily income can generate negative non-labor income elasticities of daily labor supply, even without targeting. This can result from consumption not being smoothed across days. Consider such form of utility function on day t:

$$U\left(K_t I_t^L, I_t^U\right) = u\left(K_t I_t^L + I_t^U\right) - \frac{\theta}{1+\nu}\left(I_t^L\right)^{1+\nu},$$

where $K_t = \beta^{I[t<\bar{t}]}\delta^{\bar{t}-t}$ if the worker considers real discounted daily income, or $K^t = 1$ if he looks at face-valued mental daily income. $u''(\cdot) < 0$. I maintain the assumption that effort and income are separable. First order condition is given by:

$$K_t u'\left(K_t I_t^{L*} + I_t^U\right) - \theta\left(I_t^{L*}\right)^{\nu} = 0.$$

By the Implicit Function Theorem,

$$\frac{dI_t^{L*}}{dI_t^U} = -\frac{K_t u''\left(K_t I_t^{L*} + I_t^U\right)}{K_t^2 u''\left(K_t I_t^{L*} + I_t^U\right) - \theta\nu\left(I_t^{L*}\right)^{\nu-1}} = -\frac{1}{K_t - \frac{\theta\nu\left(I_t^{L*}\right)^{\nu-1}}{K_t u''\left(K_t I_t^{L*}+I_t^U\right)}}.$$

Notice that $\partial\left(\frac{dI_t^{L*}}{dI_t^U}\right)/\partial K_t < 0$ so $\partial\left(\frac{dI_t^{L*}}{dI_t^U}\right)/\partial\left(\bar{t}-t\right) > 0$ if $K_t = \beta^{I[t<\bar{t}]}\delta^{\bar{t}-t}$, which is not supported by data (See Figure 10). Therefore we have $K_t = 1$ (the worker takes into account the face-valued mental daily income) and $I_t^{L*} = I_t^{L*}\left(I_t^U\right)$. When $I_t^U = 0$, we have:

$$\begin{cases} u'\left(I_t^{L*}(0)\right) = \theta\left(I_t^{L*}(0)\right)^{\nu} \\ \frac{dI_t^{L*}(0)}{dI_t^U} = -\frac{1}{1-\frac{\theta\left(I_t^{L*}(0)\right)^{\nu-1}}{u''\left(I_t^{L*}(0)\right)}} \end{cases} \Rightarrow \frac{dI_t^{L*}(0)}{dI_t^U} = -\frac{1}{1-\frac{u'\left(I_t^{L*}(0)\right)}{I_t^{L*}(0)u''\left(I_t^{L*}(0)\right)}} = -\frac{1}{1+\frac{1}{RRA|_{I_t^U=0}}}$$

$$\Rightarrow RRA|_{I_t^U=0} = \frac{1}{1+\frac{dI_t^{L*}(0)}{dI_t^U}} - 1.$$

From Columns (4) and (5) of Table 9, we see that $\frac{dI_t^{L*}(0)}{dI_t^U} < -0.8$ (at least), therefore $RRA|_{I_t^U=0} > 4$. A coefficient of relative risk aversion higher than 2 is unrealistic (**?**), therefore the model with diminishing marginal return to daily income can hardly explain the negative non-labor income elasticities in the setting of this study.

Appendix C

Appendix of Chapter III

C1 Table Appendix

Table C1: Relationship between HQ and Estab. Wages: Private Sec Firms

Log Establishment Wage by:	Skill Level		Occupation	
	(1)	(2)	(3)	(4)
Log Wage at HQ (Skill)	0.125	0.122***		
	(0.143)	(0.014)		
Log Wage at HQ (Skill)		-0.242		
× Low Ineq. Aversion		(0.432)		
Log Wage at HQ (Occ'n)			0.310***	1.067***
			(0.062)	(0.007)
Log Wage at HQ (Occ'n)			-0.242	-0.150***
× Low Ineq. Aversion			(0.432)	(0.018)
Log Benchmark Wage	0.057**	0.064***	0.015***	0.015***
	(0.027)	(0.008)	(0.003)	(0.003)
Occupation-Firm FE	Y	Y	Y	Y
Estab. Country-Year FE	Y	Y	Y	Y
Observations	2,506	2,416	2,921	2,924
R-squared	0.972	0.975	0.975	0.963

Note: This table shows the correlation between private sector firms' wage levels at their headquarter and establishments. The outcome variable in columns 1 and 2 is the skill-level-specific log wage at an establishment. The outcome in columns 3 and 4 is the occupation-specific log wage at an establishment. In columns 2 and 4, we interact the main independent variable, headquarter wage, with a binary variable indicating whether a country is classified as having low inequality aversion according to the Hofstede measures of culture. If the variable "Low Ineq. Aversion" equals one, it indicates that the country is more accepting of inequality than the average country in the sample. Standard errors are reported in parentheses and clustered at the firm level. (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table C2: Heterogeneous Wage Effects of Home Minimum Wage Change: Private Sec Firms

Panel A	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage	0.206***	0.513***	
	(0.074)	(0.150)	
%Δ HQ Wage (IVed)			0.401**
			(0.185)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	40,301	3,828	40,301
R-squared	0.433	0.507	0.433

Panel B	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage	0.449***	0.204	
× High Ineq. Aversion	(0.100)	(0.176)	
%Δ Min Wage	0.005	0.521***	
× Low Ineq. Aversion	(0.083)	(0.150)	
%Δ HQ wage			2.200
× High Ineq. Aversion (IVed)			(1.959)
%Δ HQ wage			0.009
× Low Ineq. Aversion (IVed)			(0.160)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	40,301	3,828	40,301
R-squared	0.436	0.508	0.436

Note: Panel A (panel B) replicates Table 3.3 (Table 3.6) restricting the sample to private-sector firms.

Table C3: Impact of Home Min. Wage Change on Firm Wages at Home and Abroad: Pre-Existing Jobs

Panel A	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage	0.269***	0.458***	
	(0.073)	(0.150)	
%Δ HQ Wage (IVed)			0.587**
			(0.250)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	215,280	5,842	215,280
R-squared	0.255	0.450	0.255

Panel B	%Δ Estab. Wage (1)	%Δ HQ Wage (2)	%Δ Estab. Wage (3)
%Δ Min Wage	0.477***	0.540**	
× High Ineq. Aversion	(0.099)	(0.237)	
%Δ Min Wage	-0.014	0.456***	
× Low Ineq. Aversion	(0.060)	(0.156)	
%Δ HQ wage			0.883*
× High Ineq. Aversion (IVed)			(0.469)
%Δ HQ wage			-0.020
× Low Ineq. Aversion (IVed)			(0.130)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	215,280	5,900	215,280
R-squared	0.258	0.450	0.258

Note: Panel A (panel B) replicates Table 3.3 (Table 3.6) restricting to the set of occupations that were already present in the relevant foreign establishment in the immediately preceding year surveyed. (i.e. pre-existing jobs).

Table C4: Impact of Home Min Wage Change on Occupations (Excluding NGOs)

	Establishment				Headquarter	
	Occ. Leaves		Occ. Added		Occ. Leaves	Occ. Added
	(1)	(2)	(3)	(4)	(5)	(6)
%Δ Min Wage	0.093	0.318**	0.026	-0.058	-2.941***	0.033***
	(0.072)	(0.130)	(0.024)	(0.059)	(0.851)	(0.011)
%Δ Min Wage		-0.353***		0.120**	2.560***	-0.041***
× Low Ineq. Aversion		(0.134)		(0.058)	(0.816)	(0.010)
Firm-Occupation FE	Y	Y	Y	Y	Y	Y
Estab. Country-Year FE	Y	Y	Y	Y	N	N
HQ Country FE	N	N	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y	Y
Observations	75819	75819	417996	417996	10655	285077
R-squared	0.544	0.544	0.156	0.156	0.473	0.053

Note: This table shows the impact of a 100% minimum wage increase in a firm's HQ country on the existence of occupations in the firm's establishments (columns 1-4) and headquarter (columns 5-6). The sample excludes NGOs. The outcome variables in columns 1, 2 and 5 is a dummy variable indicating that an occupation that previously existed in a firm's establishment or HQ no longer existed in the year after the minimum wage increase. The outcome variable in columns 3, 4, and 6 is a dummy variable indicating that an occupation that did not exist in a firm's establishment or HQ before the minimum wage increase, appeared in the establishment or HQ in the year following the minimum wage increase. "Low Ineq. Averse" is a dummy variable indicating that the country has an above-average tolerance for inequality (a high Power Distance Index score). Standard errors are reported in parentheses and are clustered at the firm-HQ country level. (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table C5: Impact of Min Wage Change at Estab. on HQ and Other Establishments

%Δ Wage in:	Establishment j (1)	Headquarter (2)
%Δ Min Wage at Estab$\neq j$	0.0028***	0.0020
	(0.0006)	(0.0038)
Occupation-Firm FE	Y	Y
Estab. j Country FE	Y	N
Headquarter Country FE	N	Y
Firm-Year FE	Y	Y
Observations	69,959	3,348
R-squared	0.348	0.795

Note: This table shows the impact of a 100% minimum wage increase in a firm's establishment j on the wages at the firm's other establishments and headquarter. Standard errors are reported in parentheses and are clustered at the firm-establishment country level. (*=p<0.10, **=p<0.05 ,***=p<0.01)

Table C6: Impact of Home Exchange Rate Shocks on Firm Real Wages at Home and Abroad

Panel A	Log Estab. Wage (1)	Log HQ Wage (2)	Log Estab. Wage (3)
Log Min Wage	-0.101**	-0.369***	
	(0.044)	(0.122)	
Log HQ Wage (IVed)			0.273*
			(0.150)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	248,021	7,166	248,021
R-squared	0.912	0.975	0.912

Panel B	Log Estab. Wage (1)	Log HQ Wage (2)	Log Estab. Wage (3)
Log Min Wage	-0.162***	-0.866***	
× High Ineq. Aversion	(0.055)	(0.157)	
Log Min Wage	-0.012	-0.266***	
× Low Ineq. Aversion	(0.071)	(0.106)	
Log HQ wage			0.187***
× High Ineq. Aversion (IVed)			(0.072)
Log HQ wage			0.045
× Low Ineq. Aversion (IVed)			(0.327)
Occupation-Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Estab. country-Year FE	Y	-	Y
Observations	248,021	7,166	248,021
R-squared	0.912	0.975	0.912

Note: Panel A shows the impact that a 100% local currency depreciation (to USD) in a firm's home country has on gross wages (in USD) in its foreign establishments (column 1) and its headquarter (column 2). Panel B looks at heterogeneity in the impact that a 100% local currency depreciation (to USD) in a firm's home country has on gross wages (in USD) in its foreign establishments (column 1) and headquarter (column 2), based on whether the home country has high or low inequality aversion. Inequality aversion is defined according to the Hofstede measures of culture. We use the "Power Distance" index, which measures the extent to which people in a group or society accept that power and opportunity is distributed unequally. In the table, "Low Ineq. Averse" (("High Ineq. Averse)) is a dummy variable indicating that the country has an above-average (below-average) tolerance for inequality (a high Power Distance Index score). We perform two-sample 2SLS estimation in column 3, where the full headquarter sample (first stage, column 2) and the full foreign establishment sample (reduced form, column 1) are used. Outliers with wage changes larger than 4 or smaller than -0.8 are excluded. Standard errors are reported in parentheses and clustered at the firm level. TS2SLS standard errors are computed following Pacini and Windmeijer (2016). (*=p<0.10, **=p<0.05 ,***=p<0.01)